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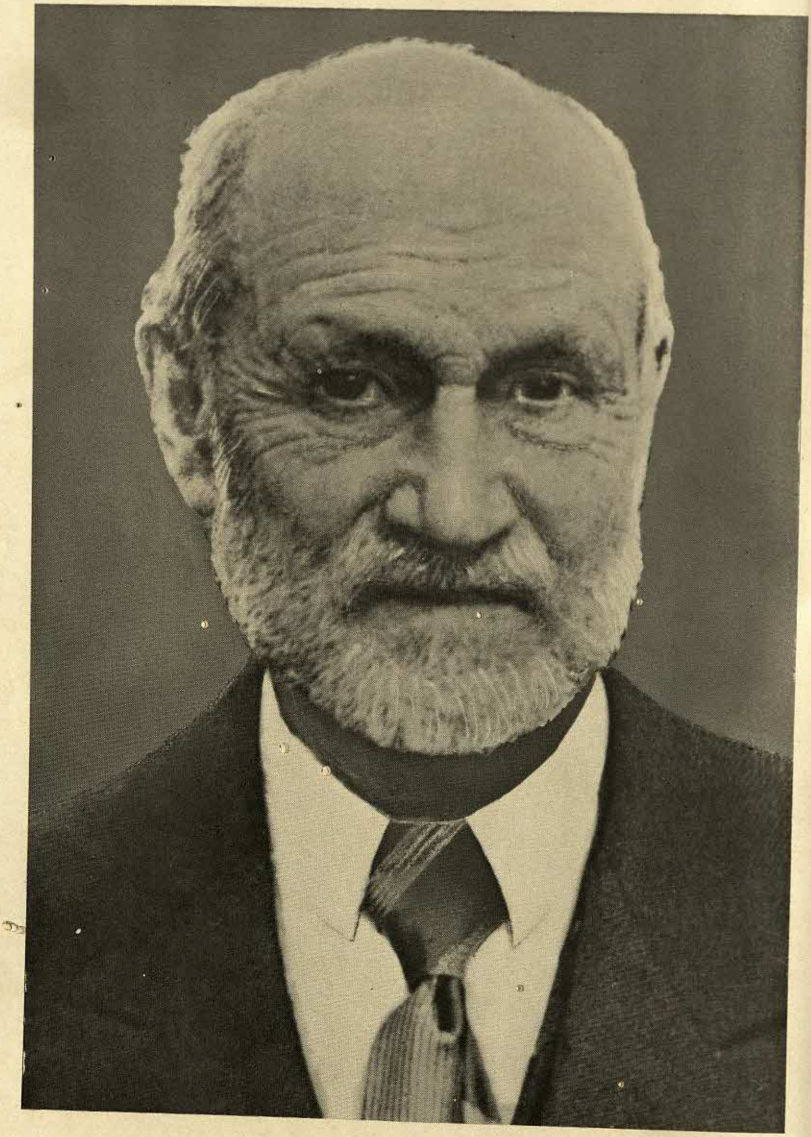

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LEARNING AND MOTIVATION IN THE IMMATURE RAT

By WILLIAM A. ROBERTS, Vassar College

The role of the brain in learning may be studied in various ways. One is to compare the learning of normal animals with that of animals whose brains have been altered surgically. A second is to study the functional correlates of naturally occurring differences in brain-structure. The natural differences may be among adult animals at different phylogenetic levels, or among animals of the same species, but at different ontogenetic levels.

The neurosurgical and phylogenetic approaches have already yielded results which suggest that differences in brain-structure may give rise to qualitative differences in the learning process.[1] For example, experiments on habit-reversal and on probability-learning revealed quite distinct modes of adjustment in rat and fish: in experiments on habit-reversal, the rat shows progressive improvement, but the fish does not; in experiments on probability-learning, the fish shows random matching, but the rat does not.[2] That the cortex plays an important role in these differences is suggested by comparing the performance of normal and extensively decorticated rats: in visual (although not in spatial) problems, the results for operated rats resemble those for the fish.[3] The experiments reported in

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[1] M. E. Bitterman, Phyletic differences in learning, *Amer. Psychologist*, 20, 1965, 396-410.

[2] M. E. Bitterman, Jerome Wodinsky, and D. K. Candland, Some comparative psychology, this JOURNAL, 71, 1958, 94-110.

[3] R. C. Gonzalez, W. A. Roberts, and M. E. Bitterman, Learning in adult rats with extensive cortical lesions made in infancy, this JOURNAL, 77, 1964, 547-562.

this paper were designed to explore the ontogenetic approach to the role of brain-differences in learning. Habit-reversal and probability-learning in immature rats were studied, and the results were compared with those obtained in experiments with adults.

Data on learning in rats of different ages are, of course, to be found in the literature, but previous investigators have, with few exceptions, been interested, not in the emergence of new capabilities, but in quantitative improvement in learning with age—improvement in the *rate* of learning. Rats of different ages have been trained in mazes, problem- and discriminative-boxes, and their time-, trial-, and error-scores compared. These experiments have shown no simple relationship between age and speed of learning. The results have often been contradictory, and the investigators often surprised to find better performance in immature animals than in adults. The superiority of young animals usually has been explained in terms of drive rather than of learning.

Using problem-boxes which required animals to depress a series of platforms in a particular sequence to be admitted to a goal-compartment containing food, Stone found that 30-50 day-old rats learned to depress the correct platforms faster than adult rats.[4] The difference was attributed to the greater activity of the immature animals, which led them to chance upon the correct platforms sooner than the adults. This interpretation was supported by two other findings: the total number of platforms stepped on was higher in the young rats than in the adults, and differences in time disappeared when the drive-level of the adults was increased.

Studies of maze-learning have yielded particularly inconsistent results. Using a circular maze, Hubbert found that 25 and 65 day-old rats learned faster than adult animals in terms of time, trials, and errors.[5] Liu studied rats ranging from 30-250 days of age in a Carr-maze which required the animals to choose from among a number of parallel pathways.[6] Speed of learning increased as age increased from 30-75 days, and decreased thereafter. It is interesting to note that the incidence of forward entrances into culs decreased with age, while the incidence of retracing increased with age. Liu attributed this result to a tendency of the younger animals to go through the maze in an impulsive, headlong fashion, while older animals proceeded more cautiously. Liu's findings were contradicted by the results of an experiment by Stone, also with the Carr-maze.[7] Stone found that 4-48 mo.-old rats learned faster than 30-70 day-old rats. In experiments with other mazes, however, Stone did find better performance in immature animals than in adults.[8] In a multiple-*T* maze, the performance of young animals equalled or surpassed that of post-pubescent, middle-aged, and early-senescent animals. Similar results were obtained with a maze in which the *Ss* alternately ran two patterns which required opposing sequences of turns; although no animal completely mastered the problem, the rate of error-elimination was greater in animals of 51-90 days than in animals of 161-605 days.

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[4] C. P. Stone, The age factor in animal learning: I. Rats in the problem box and the maze, *Genet. Psychol. Monogr.*, 5, 1929, 1-130.

[5] H. B. Hubbert, The effect of age on habit formation in the albino rat, *Behav. Monogr.*, 2, 1915, (No. 6), 1-

[6] S. Y. Liu, The relation of age to the learning ability of the white rat. *J. comp. Psychol.*, 8, 1928, 75-85.

[7] Stone, *op. cit.*, 1-130.

[8] Stone, *op. cit.*, 1-130; The age factor in animal learning: II. Rats on a multiple light discrimination box and a difficult maze, *Genet. Psychol. Monogr.*, 6, 1929, 125-202.

Ontogenetic comparisons of discriminative learning have shown either no difference in performance or a progressive decrement with increasing age. In a series of six experiments with a multiple-unit light-discriminational apparatus, Stone studied learning in rats ranging from 31-770 days of age.[9] On each trial in this apparatus, *S* was confronted with a series of five choice-points, each offering a choice between a lighted and an unlighted pathway. In the first five experiments, with the lighted pathway correct, no age-difference was found. In the sixth experiment, rats of 51, 246, and 770 days were reversed after having learned to choose the lighted pathway. Although the original problem had not differentiated among these groups, the number of trials and the number of errors to criterion in the reversal were found to increase with age. That this result could be attributed to differences in drive was suggested by the fact that adults trained under high drive learned faster than those trained under low drive. Two recent studies also have shown better performance in young animals than in adults. Fields found that 30-90 day-old rats learned five serially presented discriminative problems faster than 450-480 day-old rats.[10] D'Amato and Jagoda, training 30, 60, and 90 day-old rats deprived of water for 23 hr. to enter the illuminated arm of a *Y*-maze with water as reward, found that trials to criterion increased significantly with age.[11]

One investigator who did look for some qualitative differences in the intelligence of immature and adult rats was Maier.[12] Maier used the three-table problem to study reasoning in 50-75 and 120 day-old rats. While both groups scored above chance, the performance of the adults was markedly superior to that of the younger animals, and Maier concluded that the capacity to combine isolated past experiences develops with age.

Because differences in drive are confounded with differences in age, and because speed of learning varies with drive, no satisfactory answer has been found to the question of whether the speed of learning changes with age, and a similar difficulty is encountered in the search for qualitative age-differences in learning. Suppose, for example, that progressive improvement fails to appear in a habit-reversal experiment with young rats. Is it to be concluded that the young rats lack the reversal-capability of adults, or that they have the capability, but do not manifest it because they have been tested under relatively unfavorable conditions of drive? Questions of the same sort arise in phylogenetic comparisons, and they may be dealt with in ontogenetic comparisons in the same way as in phylogenetic comparisons.[13] While there is at present no way to equate drive in animals of different ages (as there is none to equate drive in animals of different species), equation of drive is not necessary for the conclusion that differences in performance are not due merely to differences in drive. Suppose that we vary drive in the immature animal over a wide range and still fail to find progressive improvement, despite the fact that work with adult animals yields improvement over a wide range of drive-levels. It then becomes quite unlikely that the difference in performance is due to a difference in drive. Any other variable confounded with age can, like drive, be controlled by the method of systematic variation. In the present experiments with immature rats, drive was varied from the outset to check on the possibility that any differences in performance which might appear were due to differences in drive.

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[9] Stone, *op. cit., Genet. Psychol. Monogr.*, 6, 1929, 125-202.

[10] P. E. Fields, The age factor in multiple-discrimination learning by white rats, *J. comp. physiol. Psychol.*, 46, 1953, 387-389.

[11] M. R. D'Amato and H. Jagoda, Age, sex, and rearing conditions as variables in simple brightness discrimination, *J. comp. physiol. Psychol.*, 53, 1960, 261-263.

[12] N. R. F. Maier, Age and intelligence in rats, *J. comp. Psychol.*, 13, 1932, 1-6.

EXPERIMENT I

Since no adequate technique for manipulating drive in immature rats was available, the first step was to develop one. Manipulation of body-weight, the technique commonly used with adults, seemed unfeasible, and the possibilities of prefeeding were therefore explored. (The method of prefeeding has been used successfully with the fish, for which the manipulation of body-weight also is unfeasible.)[14] In the present experiment, the effects of prefeeding were measured in terms of the time taken to traverse a runway leading to food.

Subjects. The *S*s were 18 immature male and female albino rats of the CF-Nelson strain, born in the laboratory and weaned at 20 days of age.

Apparatus. The apparatus consisted of a starting box, runway, and goal-box, all made of wood and painted flat gray. The starting box was separated from the runway by a wooden guillotine-door. Hinged translucent Plexiglas doors covered the top of the apparatus. The height of the walls in all compartments was 6 in. The width of the starting box and the runway was 4 in., while the width of the goal-box was 6 in. The starting box was 8 in. long, the runway 62.5 in. long, and the goal-box 11.5 in. long.

Starting latencies and running times were measured with two Standard Electric Timers. At the start of each trial, *S* was placed in the starting box. When *S* was oriented toward the door, *E* raised the door manually. Opening the starting box door operated a microswitch which started the first timer. When *S* broke a light-beam 6 in. beyond the door, the first timer stopped (providing a measure of latency) and the second timer started. When *S* broke a second light beam 56.5 in. beyond the first beam, at the entrance to the goal-box, the second timer stopped (providing a measure of running time). To prevent retracing, a translucent Plexiglas door, located 9.5 in. before the entrance to the goal-box, was closed manually after *S* had entered the goal-box. Reward was found in a round food-cup fixed to the floor at the rear of the goal-box.

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[13] Bitterman, Toward a comparative psychology of learning, *Amer. Psychologist*, 15, 1960, 704-712.

[14] R. M. Eskin and M. E. Bitterman, Fixed-interval and fixed-ratio performance in the fish as a function of prefeeding, this JOURNAL, 73, 1960, 417-428.

Procedure. After weaning, the animals were fed 45-mg. Noyes pellets *ad lib* for 30 min. twice a day at 12-hr. intervals. At each feeding, a rectangular box made of fine-mesh wire-screening was placed inside the animal's home-cage, and the animal ate from a food-cup containing a measured number of 45-mg. pellets. In this way, no pellets could fall through the bottom of the cage, and the animal's total intake could be calculated by subtracting the number of pellets remaining at the end of feeding from the number originally given.

After two days of this procedure, the experiment began. There were two sessions per day, at 12-hr. intervals. In each session, each animal was prefed a certain number of pellets, then was given runway-training, and half an hour afterwards was fed *ad lib* on pellets. The amount each animal was prefed varied from session to session, being either 5%, 25%, or 50% of the total number of pellets consumed in the previous session. The animals were divided into three subgroups of 6 each. Within each group, each animal experienced all three prefeeding-conditions over a block of three sessions, the order of prefeeding-values being counterbalanced over *S*s. Each group went through seven prefeeding cycles of three sessions each.

Two of the experimental groups were given one runway-trial per session. Out of an interest in the effects of amount of reward on the performance of the immature rat, the size of reward was varied. The *S*s of one group always received 25% of the number of pellets consumed in the last session, while the *S*s of the other group received only 5% of the previous session's intake. The third group of *S*s was given 10 massed trials per session and received one pellet per trial in the goal-box. The animals were kept in a gray detention-box during the 1-min. intertrial intervals.

Results. Since latencies and running-times both showed essentially the same relationships, they were summed to make total-time scores, which were converted to logs for purposes of statistical analysis. Over cycles, log time decreased in a negatively accelerated manner for all groups, reaching an asymptote by about the fourth cycle. In Fig. 1, mean log time is plotted as a function of percentage-prefed for all seven cycles combined. In all three groups, log time increases as percentage-prefed increases. The large-reward group has slightly lower time-scores than the small-reward group at the high and low prefeeding values, but the two groups do not differ at the intermediate prefeeding value. An analysis of variance based on the spaced-training data yields a significant Cycles-Effect ($F = 37.75$, with 6 and 60 *df*) and a significant Prefeeding-Effect ($F = 3.82$, with 2 and 20 *df*). Amount of reward is not a significant effect ($F < 1.00$), and none of the interactions is significant. Orthogonal comparisons between pairs of prefeeding-values show that neither the difference between 5% and 25% nor the difference between 25% and 50% is significant. For the massed training data, analysis of variance reveals a significant Cycles-Effect ($F = 30.06$, with 6 and 30 *df*) and a significant Prefeeding-Effect ($F = 13.90$, with 2 and 10 *df*). The interaction does not approach significance ($F < 1.00$). Orthogonal comparisons show a significant difference between the 5% and 25% prefeeding values ($F = 7.02$, with 1 and 10 *df*) and a significant difference between 25% and 50% ($F = 6.87$, with 1 and 10 *df*).

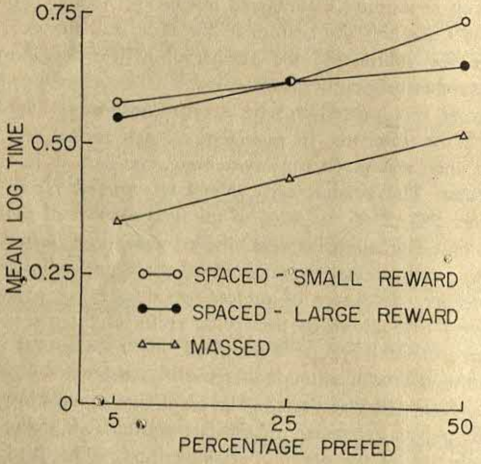

FIG. 1. MEAN LOG TIME AS A FUNCTION OF PERCENTAGE-PREFED

In Fig. 2, within-sessions plots are provided for the massed-training groups in terms of log time averaged over all seven cycles. None of the curves shows a clear rising or declining trend over trials, nor do the effects of prefeeding seem to be more marked in one portion of the curves than in another. This evaluation is substantiated by an analysis of variance which shows an insignificant Trials-Effect ($F = 1.00$) and an insignificant Trials × Prefeeding interaction ($F = 1.02$, with 18 and 90 *df*).

It is clear from these results that reliable manipulation of drive, as measured by locomotion in the runway, can be produced in the immature rat by prefeeding different amounts of food. While the Prefeeding-Effect appeared both with spaced and with massed training, it was more marked with massed than with spaced training, since the massed condition showed significant differentiation between adjacent prefeeding-values and the spaced condition did not. When massed training was given, furthermore, the effect of differential prefeeding was constant over trials within sessions. The failure to find a significant effect of amount of reward on running time is in contrast to results for the adult rat, which typically is found to run faster to large rewards than to small rewards. It is possible, however, that wider variation in amount of reward might produce a like effect in the immature rat.

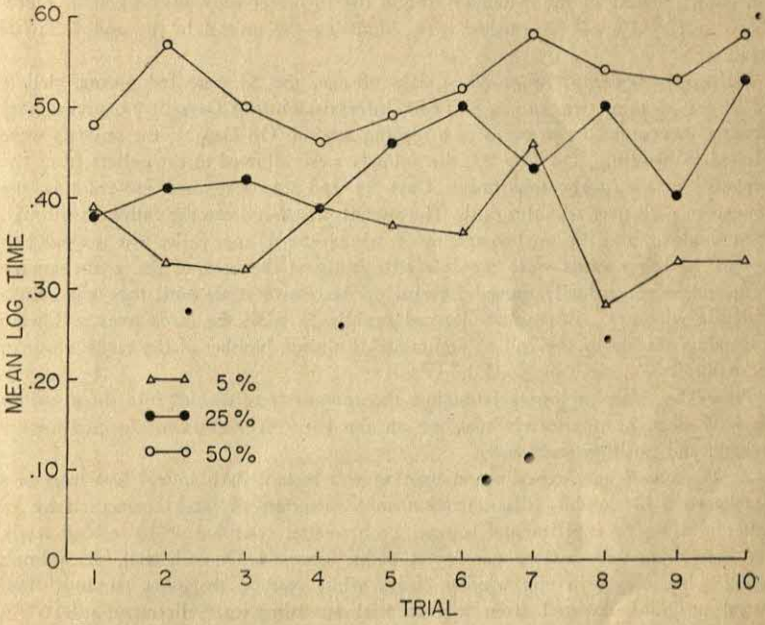

FIG. 2. MEAN LOG TIME AS A FUNCTION OF TRIALS WITHIN SESSIONS AT THREE PREFEEDING VALUES

EXPERIMENT II

In the second experiment, criterion-reversal of a simultaneous visual discrimination was studied in the immature rat. To control motivation, the work was done with three different schedules of prefeeding.

Subjects. The *Ss* were 15 immature rats of both sexes, bred from stock of the CF-Nelson strain. They were raised in the laboratory from birth and weaned at 20 days of age.

Apparatus. The apparatus consisted of a scaled-down model of the modified

Lashley jumping stand used in the Bryn Mawr laboratory for adult rats. The entire apparatus was made of wood and was painted flat gray. An elevated starting platform, 2.5 in. wide and 5.5 in. long, branched into two pathways, each 2.5 in. wide and 5.75 in. long, which led to two windows, each 2.5 in. wide and 3 in. high. The windows were 1.5 in. apart and were separated by a triangular partition which projected outward 2.5 in. The windows opened into a rectangular goal-compartment, 8.5 in. wide and 10.25 in. long. At the back of the goal-compartment, a circular food-dish was fixed to the floor. During experimental training, stimulus-cards were placed in the windows. While the incorrect card was locked in place, the correct card could be pushed over, admitting the animal to the goal-compartment.

Preliminary training. From 20-24 days of age, the *S*s were fed 45-mg. pellets *ad lib* for 30 min. twice a day at 12-hr. intervals. During Days 21-24, preliminary training was conducted prior to each feeding session. On Day 21, the animals were adapted to handling. On Day 22, the animals were allowed to eat pellets from the food-dish in the goal-compartment. Days 23 and 24 were used for training the animals to push over stimulus-cards. The animals first ran down the pathways through open windows into the goal-compartment, where one 45-mg. pellet was provided as reward. Mid-gray cards were then placed upright at the back of the goal-compartment and were gradually moved forward on successive trials until they were flush with the windows. All animals learned rapidly to push the cards over and were responding readily by the end of preliminary training. Neither of the cards was ever locked in place during this procedure.

Procedure. After preliminary training, the animals were divided into three groups of 5 *S*s each, approximately matched on the basis of adjustment to preliminary training and position-preferences.

At 25 days of age, experimental training was begun. Each animal first learned a simultaneous black-white discrimination to a criterion of 90% correct choice or better in a single experimental session. Each session consisted of 10 massed trials, and there were two sessions per day at 12-hr. intervals. On each trial, the animals found a black card in one window and a white card in the other window, their lateral positions reversed from trial to trial according to Gellermann-orders.[15] A modified correction-procedure was used. After making an incorrect response, *S* was picked up, returned to the starting platform, and allowed to make another choice. After three repetitive errors on one trial, *S* was manually guided to the correct-stimulus on the next run. Each trial, therefore, was terminated by a correct response and one pellet of food. During the intertrial-interval (about 15 sec.), *S* was kept in a gray detention-box while *E* placed the stimuli in the proper arrangement or the next trial. For 3 *S*s in each of the three experimental groups, the white card was correct on the original problem (Reversal 0); for the other 2 *S*s in each group, the black card was correct. When the original problem had been learned to criterion, the correct and incorrect stimuli were reversed and the animal was trained again to criterion, until 20 criterion-reversals were learned by each animal.

The three experimental groups were trained under different levels of drive. The prefeeding technique developed in Experiment I was used to establish high-, medium-, and low-drive states in the three groups. The High-Drive Group was prefed 5% of the previous session's total food-intake before every training session, the Medium-Drive Group was prefed 25%, and the Low-Drive Group was prefed 50%.

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[15] L. W. Gellermann, Chance orders of alternating stimuli in visual discrimination experiments, *J. genet. Psychol.*, 42, 1933, 206-208.

Results. In Fig. 3, the performance of the three groups is plotted in terms of median initial errors per reversal. All three drive-groups make about the same number of errors on Reversal 0. In the early reversals, errors increase in all three groups, the increase being much greater in the High-Drive Group than in the Medium- and Low-Drive Groups. Thereafter, all of the curves decline progressively until, by the final reversals (at which point the animals are 80-120 days old), initial errors have fallen to the same low level in all groups. An analysis of variance based on initial errors yields a significant Drive-effect ($F = 4.27$, with 2 and 12 df), a significant Reversals-Effect ($F = 13.38$, with 20 and 240 df), but an insignificant Reversals × Drive interaction ($F = 1.12$, with 40 and 240 df). Orthogonal comparisons show that the difference between the High-Drive Group and the combined Low- and Medium-Drive groups is significant ($F = 8.30$, with 1 and 12 df), but the difference between the Low-Drive Group and the Medium-Drive Group is not ($F < 1.00$).

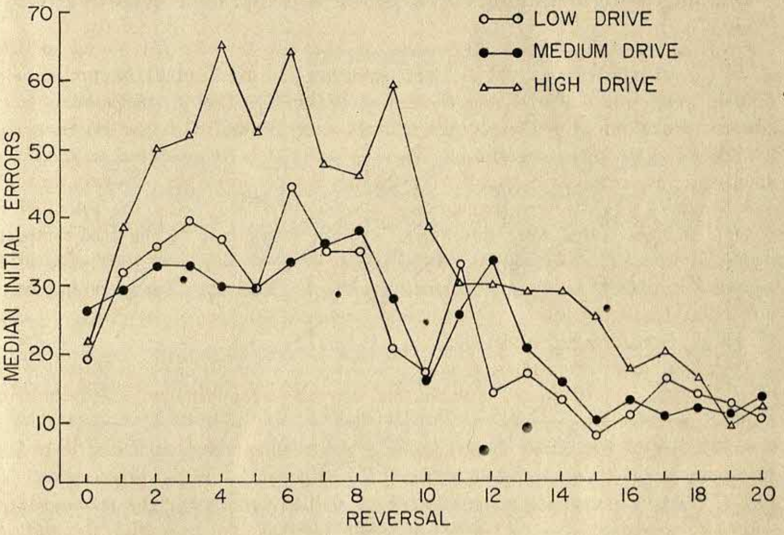

FIG. 3. CRITERION -REVERSAL PERFORMANCE IN SIMULTANEOUS VISUAL DISCRIMINATION AT THREE LEVELS OF DRIVE

In Fig. 4, the course of learning over the first four sessions of each reversal is plotted for successive blocks (A, B, C, and D) of five reversals. For purposes of plotting these curves, an animal which had reached criterion in fewer than four sessions on a given reversal was assigned an error-score of zero for the remaining sessions. At each drive-level, we have a family of essentially parallel curves closely resembling those obtained in work with the adult rat by Gatling[16] and by Bitterman, Wodinsky, and Candland.[17]

One interesting finding of this experiment is that high levels of motivation impair the performance of immature rats in habit-reversal. Unfortunately, there are no comparable data for adult rats. Another interesting finding is that improvement in habit-reversal did not appear until the ninth reversal, when the average age of the animals was 61 days or more. In the experiment of Gatling[18] and of Gonzalez, Roberts, and Bitterman[19] on criterion-reversal learning in adult rats, improvement was evident by the third reversal. While the conditions of the adult-rat experiments differed in many respects from those of the present experiment, the possibility of an ontogenetic difference should be considered. It might be hypothesized,

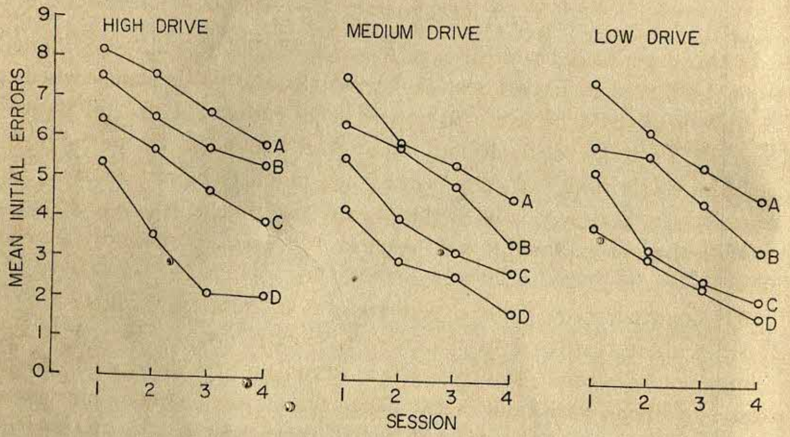

FIG. 4. WITHIN-REVERSALS PERFORMANCE IN SIMULTANEOUS CRITERION-REVERSAL AT THREE LEVELS OF DRIVE
(*A*, reversals 1-5; *B*, 6-10; *C*, 11-15; *D*, 16-20.)

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[16] F. P. Gatling, The effect of repeated stimulus reversals on learning in the rat, *J. comp. physiol. Psychol.*, 45, 1952, 347-351.

[17] Bitterman, Wodinsky, and Candland, *op. cit.*, 94-110.

[18] Gatling, *op. cit.*, 347-351.

[19] Gonzalez, Roberts, and Bitterman, *op. cit.*, 547-562.

for example, that the capacity necessary for improvement does not develop in the rat until about 60 days of age. An alternative explanation of these results is that the immature rat possesses the capacity necessary for improvement but requires much more experience with habit-reversal than the adult rat before improvement is manifested. The latter hypothesis can, of course, be tested by increasing the amount of reversal experience given the immature animals before they reach the age of 60 days.

Finally, it should be noted that, when improvement does appear in the immature rat, it seems to take exactly the same form as in the adult rat.

EXPERIMENT III

In this experiment, habit-reversal was studied under conditions different from those used in Experiment II. Its purpose was to determine whether improvement could be made to appear at an earlier age than in Experiment II. Since the relatively late improvement found in the previous experiment might have been due to the small number of prior reversals, the animals were reversed daily in the present experiment, which provided a great deal more experience with reversal in a given number of calendar days.

A second purpose of this experiment was to compare simultaneous reversal with successive reversal and with spatial reversal. Interest in the simultaneous-successive comparison was stimulated by the work of Gonzalez and Shepp with adult rats.[20] They found progressive improvement in both problems, beginning with the fifteenth reversal, but the rate of improvement was faster in the successive problem than in the simultaneous problem. The adult rat also has been found to show more marked improvement in spatial problems than in simultaneous problems.[21] Gonzalez, Roberts, and Bitterman, investigating the course of criterion-reversal learning in cortically ablated adult rats in simultaneous, successive, and spatial problems, found progressive improvement in spatial and successive problems but no improvement in the simultaneous problem.[22] These results suggested that the immature rat might show improvement much more readily in spatial and successive problems than in simultaneous problems.

In this experiment, simultaneous and successive reversal were studied at each of two drive levels—high and medium—the levels that produced the clearest differentiation of performance in Experiment II. Spatial reversal was studied only at the high-drive level.

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[20] R. C. Gonzalez and Bryan Shepp, Simultaneous and successive discrimination-reversal in the rat, this JOURNAL, 74, 1961, 584-589.

[21] Bitterman, Wodinsky, and Candland, *op. cit.*, 94-110.

[22] Gonzalez, Roberts, and Bitterman, *op. cit.*, 547-562.

Subjects. The *Ss* were 28 immature male and female albino rats of the CF-Nelson strain, raised in the laboratory from birth and weaned at 20 days of age.

Apparatus. The apparatus was the modified jumping stand used in Experiment II.

Procedure. The preliminary training was exactly the same as in Experiment II, except that there was only one feeding-session per day, each *S* eating 45-mg. pellets *ad lib* for 1 hr. After preliminary training, the *Ss* were divided into five groups. The four visual groups, each containing 6 *Ss*, were designated Simultaneous-High-Drive, Simultaneous-Medium-Drive, Successive-High-Drive, and Successive-Medium-Drive. A fifth (Spatial) group, made up of 4 *Ss*, was trained on a spatial problem under the high-drive condition. The five groups were matched on the basis of adjustment to preliminary training and position-preferences.

At 25 days of age, the *Ss* were trained on Reversal 0, with 30 massed trials by the correction-and-guidance procedure of Experiment II. The simultaneous groups encountered the same stimulus-conditions as those used in Experiment II, the white card being correct for half the *Ss* in each group and the black card correct for the other half. The successive groups encountered either two white cards or two black cards on each trial; they were rewarded for choosing the left-hand member of one pair and the right-hand member of the other pair. The two pairs of cards used in each problem were presented in accordance with Gellermann orders. For half of the *Ss* in each successive group, black-right and white-left were correct in Reversal 0, while, for the other half of the *Ss*, black-left and white-right were correct. Of the Spatial *Ss*, two were trained with a pair of white cards, while the other two were trained with a pair of black cards; for one animal in each pair, the right card was correct in Reversal 0, while left was correct for the other two animals.

At 26 days of age, all *Ss* were reversed for the first time (Reversal 1). Simultaneous animals previously rewarded for response to white, now were rewarded for response to black; successive animals previously rewarded for response to white-left and black-right, now were rewarded for response to white-right and black-left; spatial animals previously rewarded for response to left, now were rewarded for response to right; and so forth. At 27 days of age, the animals were reversed again (Reversal 2), and so on, each day, until the visual animals had been reversed 70 times and the spatial animals 25 times.

After each daily session, the animals were fed *ad lib* for a period of 1 hr. Immediately before each session, the high-drive and medium-drive animals were prefed 5% and 25%, respectively, of the number of pellets eaten on the previous day.

Results. The performance of the visual groups is shown in Fig. 5. The mean error-score for each block of five reversals was calculated for each *S*, and the median of these scores was plotted for each group. The simultaneous curves begin to show improvement at Blocks 7 and 8, by which time the animals are 55-60 days old, and then continue to decline, the high-drive curve at a somewhat faster rate than the medium-drive curve. By Block 14, both the simultaneous groups are reversing quite well, making an average of only about three errors per reversal. By contrast, the successive animals show very little improvement until Block 11, at which point the high-drive curve begins to decline. Analysis of variance yields a significant Problems-effect ($F = 14.26$, with 1 and 20 *df*), a significant Blocks-effect ($F = 27.68$, with 13 and 260 *df*), and a significant Blocks × Problems interaction ($F = 10.32$, with 13 and 260 *df*). The effect of drive is not significant ($F < 1.00$), and none of the other interactions approaches significance ($F < 1.00$).

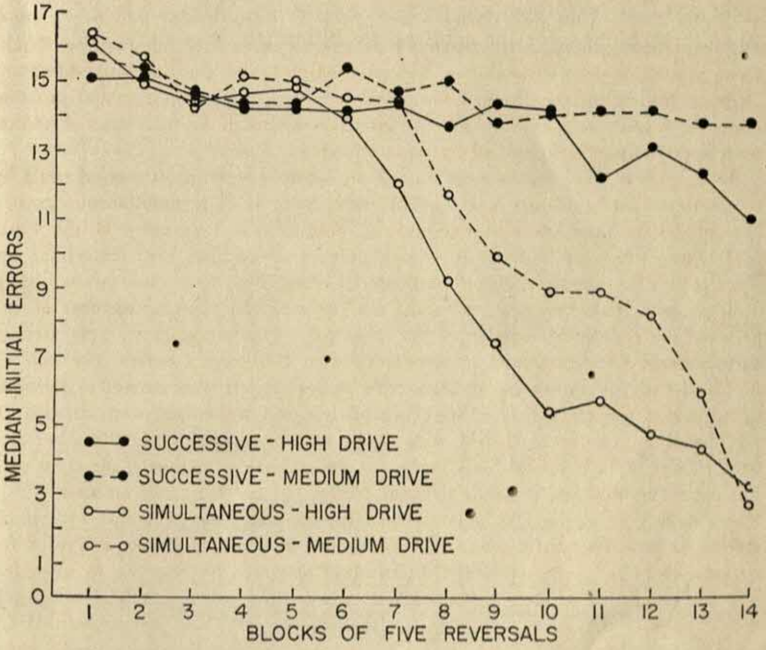

FIG. 5. DAILY-REVERSAL PERFORMANCE IN SIMULTANEOUS AND SUCCESSIVE DISCRIMINATION AT TWO LEVELS OF DRIVE

In Fig. 6, the performance of the Spatial Group in 25 reversals is plotted in terms of median errors per reversal. There it can be seen that improvement occurs quite early, the curve closely resembling that for the adult rat.

Fig. 7 shows the course of within-reversals improvement on each of these three problems. Mean initial errors are plotted for six-trial blocks, with blocks of 10 reversals as the parameter for the successive and simultaneous problems, and blocks of five reversals as the parameter for the spatial problem. For the visual problems, the data for the high- and medium-drive groups are combined, since the effect of drive was not significant. Looking first at the spatial problem, we find marked within-reversals improvement in the very first block of reversals and in each succeeding block. The first two points of these curves show parallel improvement over reversals, while at the final points the curves tend to merge due to the fact that performance is essentially errorless. These curves are identical to those plotted for the adult rat by Bitterman, Wodinsky, and Candland.[23] The simultaneous problem also yields an orderly family of curves, that for each consecutive block of reversals being lower than the preceding one. The pattern is much the same as that found for simultaneous criterion-reversal in Experiment II (Fig. 4), although within-reversals improvement is relatively slight for the first three blocks of reversals (Reversals 1–30). In the last four blocks (Reversals 31–70), the curves are steeper, indicating more marked improvement within reversals. The curves for the successive problem show little within- or between-blocks improvement until the last block or two (Reversals 51–70), when clear-cut improvement begins to appear.

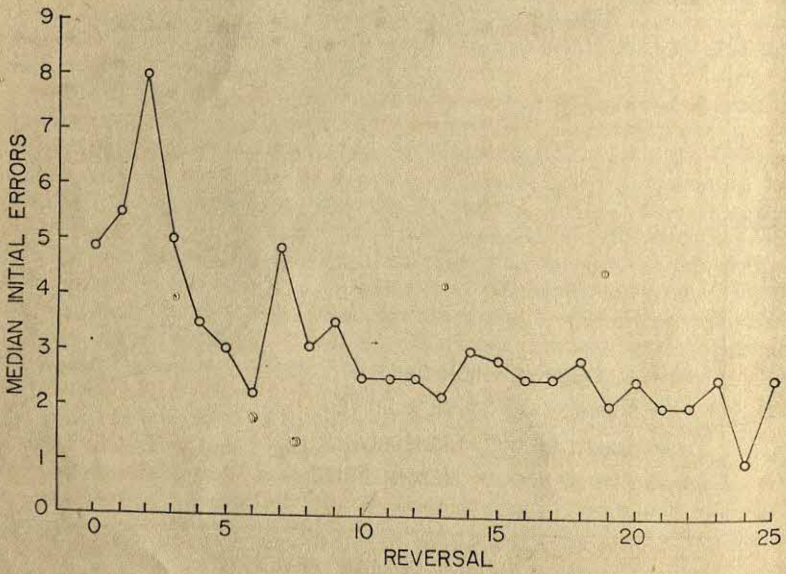

FIG. 6. DAILY-REVERSAL PERFORMANCE IN A SPATIAL DISCRIMINATION

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[23] Bitterman, Wodinsky, and Candland, *op. cit.*, 94-110.

Several aspects of these data require comment. First, although high drive retarded reversal-learning when each reversal was carried to criterion in Experiment II, drive had no significant effect on daily-reversal. Why drive should operate differently in the two kinds of experiment is unknown. Secondly, progressive improvement in spatial reversal appeared early, just as it does in adult rats. The hypothesis was suggested previously that immature rats do not develop the capacity for progressive improvement before about 60 days of age, but it now appears that this hypothesis must be restricted to visual problems. Different results for visual and spatial problems are a familiar feature of comparative experiments.[24] Third, although the improvement of immature animals was much more striking in the simultaneous problem than in the successive, the adult animals of Gonzales and Shepp improved more rapidly in the successive problem than in the simultaneous problem. This result suggests that the basis of the successive discrimination is different in immature and in adult rats.

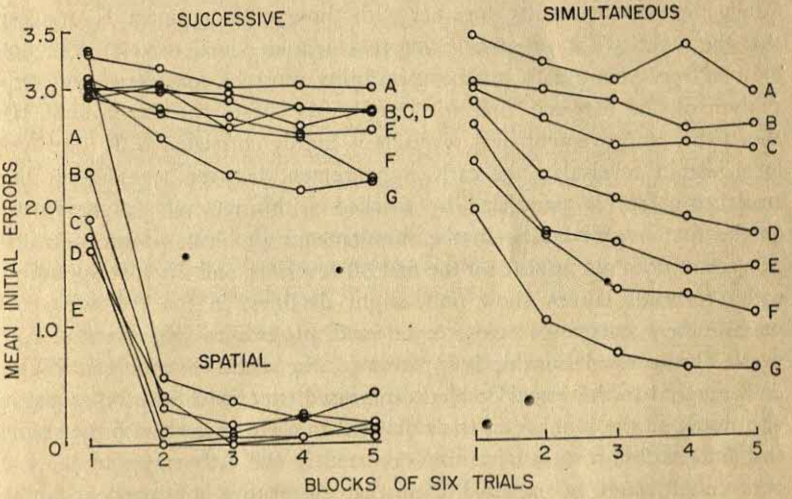

FIG. 7. WITHIN-REVERSALS PERFORMANCE IN SUCCESSIVE, SIMULTANEOUS, AND SPATIAL DISCRIMINATION

Fourth, improvement in visual reversal seemed to appear much later in immature rats than it does in adults. In the simultaneous problem, the immature rats did not show clear improvement until they were 55-60 days old, the same age at which improvement began to appear in simultaneous criterion-reversal, although the daily-reversal animals had substantially more experience with reversal by that time. With the successive problem, there was no indication of improvement until the animals were about 75 days of age; such improvement as did occur was minor and could be found only in the high-drive animals. Much more rapid improvement in adult rats was found by Gonzalez and Shepp, whose animals, trained on 20-trial daily reversals, showed improvement both in the successive and in the simultaneous problem beginning with about the fifteenth reversal.[25] While the present results, together with those of Experiment II, suggest that the capacity for progressive improvement in visual reversal does not exist in very young rats, another possibility must be considered. A comparison of the between- and within-reversals curves for Experiment III shows that improvement over reversals is highly correlated with improvement within reversals. The early improvement between reversals in the spatial problem is paralleled by marked within-reversals improvement on the first five reversals. In the simultaneous problem, progressive improvement does not appear for the first 30 reversals, and the corresponding within-reversals curves show only slight declines; in the following reversals, these curves become steeper, and progressive improvement appears. The same relationship is apparent for the successive problem. While daily reversal in the visual problems increased experience with reversals, it also restricted the number of trials devoted to each reversal with the result that little within-reversals learning occurred in the early stages of the experiment. It might be expected, then, that progressive improvement might appear much earlier in a daily-reversal experiment in which enough training was given on each reversal to insure clear within-reversals learning.

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[24] Bitterman, The evolution of intelligence, *Scient. Amer.*, 212, 1965, 92-100.

EXPERIMENT IV

In Experiment IV, one group of immature rats was reversed daily in a simultaneous problem with 60 training trials per reversal. On the chance that a more palatable reward might produce earlier improvement, the reward for these animals was wet mash instead of pellets. To unconfound number of trials per reversal and kind of reward, a second group of immature animals was trained with 30 trials per reversal (as in the previous experiment) and wet mash as reward.

Subjects. The *Ss* were 10 immature male albino rats of the Wistar strain. They were brought into the laboratory at the age of 21 days. Preliminary training was begun immediately.

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[25] Gonzalez and Shepp, *op. cit.*, 584-589.

Apparatus. The apparatus was the modified jumping stand used in Experiments II and III.

Procedure. The preliminary training was exactly the same as in Experiment II. The *Ss* were fed 45-mg. pellets *ad lib* for 30 min. twice a day at 12-hr. intervals. Experimental training began when the animals were 25 days of age. Throughout this experiment, all the animals were trained at high drive; there was no prefeeding prior to the training sessions. The animals were rewarded on each trial by being allowed to nibble wet mash for a few seconds from a glass dish at the rear of the goal-compartment.

There were two experimental sessions in each day. In the first training session of Day 25, all *Ss* were trained for 30 trials on the black-white simultaneous problem with white correct for 4 *Ss* and black correct for the other 6 *Ss*. On the basis of the number of initial errors made on these trials, the *Ss* were assigned to two groups of 5 *Ss* each which were roughly equal in total errors, the white card having been correct for 2 *Ss* in each group and the black card for 3 *Ss* in each group. One group was designated the 30-trial Group and the other was designated the 60-trial Group. The 30-trial Group was trained for only 30 trials per day (in the first session of each day). The 60-trial Group was trained for 30 trials in the first session of each day and for 30 additional trials in the second session 12 hrs. later. Both the 30-trial and 60-trial groups were allowed to eat *ad lib* for 30 min. twice a day (half an hour after each training session). This procedure was followed each day with the correct stimulus being reversed daily for a total of 50 daily reversals.

Results. In Fig. 8, the performance of the two groups is plotted in terms of mean initial errors per reversal. The 60-trial Group shows marked negative transfer from the original problem (Reversal 0) to Reversal 1, followed by a steady decline in the curve indicating progressive improvement in habit-reversal. The results for the 30-trial Group are rather different. The curve for this group remains essentially flat, showing no progressive improvement in 50 reversals. A Groups × Reversals analysis of variance yields a significant Reversals-effect ($F = 4.04$, with 50 and 400 *df*), a significant Groups-effect $F = 15.61$, with 1 and 8 *df*), and a significant Groups × Reversals interaction ($F = 2.86$, with 50 and 400 *df*).

Within-reversals learning curves are presented in Fig. 9 for the 60-trial Group. Improvement within reversals is apparent in the very first block of 10 reversals (Curve A) and in all the succeeding blocks. Within-reversals plots for the 30-trial Group, which are not presented, show a series of essentially flat curves that do not differ from each other. Again, improvement over reversals does not occur in the absence of within-reversals improvement.

It is clear from these results that the immature rat is capable of progressive improvement in the simultaneous problem at an early age if enough experience with reversal and enough training on each reversal are given. That the progressive improvement of the 60-trial Group can be attributed to the number of training trials and not to the change in kind of reward is shown by the failure of the 30-trial Group to show improvement. In fact, these results for the 30-trial Group taken together with those of Experiment III suggest that the wet mash may retard progressive improvement, although the feeding schedule, which was confounded with the kind of reward across the two experiments, may be responsible.

One question left unanswered by these results is whether the immature rat is capable of progressive improvement in the successive problem. In Experiment III, there was little improvement on a successive problem even after 70 daily reversals, but there was also little within-reversals learning. Furthermore, the conditions of the experiment were not optimal for the successive problem, in that the stimuli were spatially noncontiguous and the *Ss* made contact with the stimuli.[26] Another daily-reversal experiment should be done which maximizes the possibility of finding early

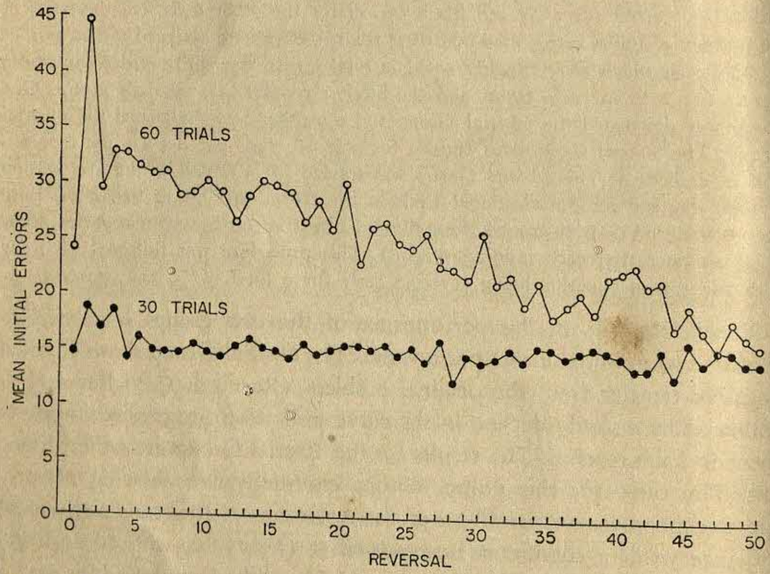

FIG. 8. DAILY-REVERSAL PERFORMANCE IN SIMULTANEOUS DISCRIMINATION FOR 30-TRIAL AND 60-TRIAL GROUPS

progressive improvement in the successive problem, that is, under the conditions of contiguity and noncontact which favor a configurational solution, and with a large number of training trials on each reversal.

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[26] Jerome Wodinsky, M. A. Varley, and M. E. Bitterman, Situational determinants of the relative difficulty of simultaneous and successive discrimination, *J. comp. physiol. Psychol.*, 47, 1954, 337-340.

EXPERIMENT V

Experiment V is concerned with probability-learning in the immature rat. The ontogenetic study of probability-learning is suggested by the results of previous phylogenetic studies and by studies of decorticated adult rats. While the normal adult rat typically shows maximizing or nonrandom probability-matching, the fish, the pigeon, and the cortically-deprived rat all have shown random matching.[27] These results are partially dependent upon the type of discriminative problem which is studied; while the fish shows random matching both in visual and in spatial problems, the pigeon and the operated rat show random matching only in visual

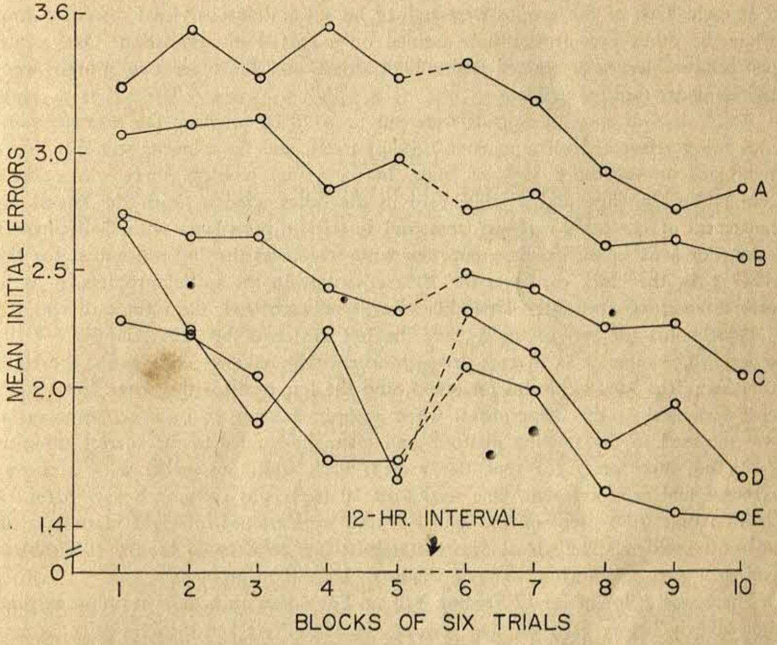

FIG. 9. WITHIN-REVERSALS PERFORMANCE IN SIMULTANEOUS DISCRIMINATION FOR THE 60-TRIAL GROUP

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[27] Bitterman, *op. cit., Scient. Amer.*, 212, 1965, 92-100; *op. cit., Amer. Psychologist*, 20, 1965, 396-410.

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problems. In the present experiment, therefore, both visual and spatial problems are used. Drive again is systematically varied.

Subjects. The *Ss* were 20 immature albino rats of both sexes, bred from stock of the CF-Nelson strain. They were raised in the laboratory from birth and weaned at 20 days of age.

Apparatus. The apparatus was the modified jumping stand used in previous experiments.

Procedure. The preliminary training was the same as in Experiment III. At the end of preliminary training, the animals were divided into four matched groups of 5 *Ss* each. Two of the groups were trained on a simultaneous visual discrimination while the other two groups were trained on a spatial discrimination. One group on each problem was trained under high drive, and the other two groups were trained under medium drive.

At 25 days of age, all animals were put on a 70:30 problem, the majority-stimulus being reinforced on a random 70% of trials, and the minority-stimulus being reinforced on the other 30% of trials. In the visual problem, there was a black card in one window and a white card in the other window, with the lateral arrangement of the cards reversed from trial to trial in accordance with Gellermann's rules. For 3 *Ss* in each drive-group, the white card was the 70% stimulus; for the other 2 *Ss*, the black card was the 70% stimulus. In the spatial problem, 3 *Ss* in each drive-group encountered two black cards on each trial; the right card was the 70% stimulus for two of the *Ss*, and the left card was the 70% stimulus for the other *S*. The other 2 *Ss* in each drive-group encountered two white cards, the right card being the 70% stimulus for one *S*, and the left card for the other. Pure guidance was used in this experiment: when an animal made on incorrect response, it was returned to the starting platform and manually guided to the correct stimulus (with no opportunity for repetitive error). Each trial thus ended with a correct response and reinforcement. One session of 20 trials was given each day. After 12 days on the 70:30 problem, the spatial groups were trained for eight more days on a 50:50 problem. The visual *Ss* were trained for 20 days on the 70:30 problem and then were given 10-15 days of training on a 50:50 problem.

There was 1 hr. of *ad lib* feeding half an hour after each daily training session. Immediately before each training session, the high-drive and medium-drive groups were prefed 5% and 25%, respectively, of their total intake on the previous day.

Results. In both the spatial and visual problems, the percentage of trials on which the 70% stimulus was chosen increased in negatively accelerated fashion from about 50% on Day 1 of 70:30 training to about 100% at asymptote. The spatial curves reached asymptote by the seventh day of training, while the visual curves did so in 14 days. Table I shows the percentage choice of the 70% stimulus in the last 100 trials of 70:30 training. All animals tended to maximize, both in the spatial and visual problems, and under both levels of drive. After being shifted to the 50:50 problem, each animal was trained until performance stabilized once more. Performance on the last 100 trials of 50:50 training is shown in Table I.

All the spatial animals, except one in the high-drive group, continued to choose the 70% position of the previous problem on nearly 100% of the trials; the exceptional animal reversed its initial preference, making all but one of its responses to the previously 30% stimulus. In the visual groups, 3 *S*s in the high-drive group and 2 *S*s in the medium-drive group continued to choose the previous 70% card in the 50:50 problem, while the other 5 *S*s fell into position-habits (*P*). These results give no hint of an ontogenetic difference in probability-learning.

SUMMARY

In the first of a series of five experiments, a prefeeding technique was shown to afford control of hunger in the immature rat as measured by speed of locomotion in a runway. This technique then was used to manipulate drive in the subsequent experiments on habit-reversal and probability-learning. The main results of these experiments were: (1) In criterion-reversals of a simultaneous visual discrimination, significantly more errors were made at high drive than at low or medium drive, but performance did not vary with drive in daily-reversals. (2) Immature animals showed early progressive improvement both in spatial and in visual reversals. (3) A daily-reversal experiment with immature animals showed more rapid progressive improvement in a simultaneous than in a successive visual problem. (4) In probability-learning, immature animals maximized both under visual and spatial conditions.

The difference in rate of improvement in the simultaneous and successive problems was the only finding which suggested a qualitative difference in the learning of immature and adult rats.

TABLE I

PERCENTAGE OF TRIALS ON WHICH THE MAJORITY-STIMULUS WAS CHOSEN IN THE LAST 100 TRIALS OF 70:30 TRAINING AND IN THE LAST 100 TRIALS OF SUBSEQUENT 50:50 TRAINING

(*P* means response to one position on more than 90% of trials in the visual problem.)

Spatial (high drive)			Spatial (medium drive)			Visual (high drive)			Visual (medium drive)		
S	70:30	50:50	*S*	70:30	50:50	*S*	70:30	50:50	*S*	70:30	50:50
1	100	100	6	98	99	11	90	*P*	16	98	*P*
2	98	93	7	99	100	12	99	95	17	100	97
3	100	1	8	100	100	13	96	*P*	18	96	*P*
4	100	100	9	99	97	14	94	94	19	99	95
5	100	100	10	100	89	15	87	95	20	94	*P*

ROLE OF THE OTOLITH ORGANS IN THE PERCEPTION OF HORIZONTALITY

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Man within the earth's gravitational field needs to depend very little upon his gravireceptors to orient himself visually with ease and precision so long as adequate visual cues are present.[1] When, however, only nonvisual cues are available, the position of man with respect to gravity becomes a critical factor in his visual localization of the direction of this force.[2] For example, tilting the head (body) axis normal to the upright results typically in gross deviations in judgments of the phenomenal vertical (or horizontal) and in instability (rotary autokinesis).[3] The cause or causes of these perceptual changes has never been satisfactorily explained. Common use of such general terms, as proprioceptive and postural (cues), points up our limited knowledge of the specific nonvisual mechanisms underlying man's perception of space.

In the present experiment, an attempt was made to determine the influence of the vestibular (otolithic) component of the proprioceptor group of cues upon the perception of horizontality. The fact that the Aubert illusion[4] and its variants[5] have been reportedly[6] observed by certain deaf

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* Received for publication February 16, 1965. Conducted under the sponsorship of the Office of Life Science Programs, National Aeronautics and Space Administration, Order No. R-37.

[1] M. E. Mulder, Ons oordeel over verticaal, bij neiging van het hoofd naar rechts of links, *Arch. Anat. u. Physiol.*, 2, 1888, 340-352; Joseph Jastrow, On the judgment of angles and positions of lines, this JOURNAL, 5, 1893, 214-248; Herbert Kleint, Versuche über die Wahrnehmung., *Z. Psychol.*, 140, 1937, 109-138; E. F. Miller, II, and Ashton Graybiel, Rotary autokinesis and displacement of the visual horizontal associated with head (body) position, *Aerospace Med.*, 34, 1963, 915-919.

[2] G. E. Müller, Über das Aubertsche Phänomen, *Z. Sinnesphysiol.*, 49, 1916, 109-244; Elizabeth Neal, Visual localization of the vertical, this JOURNAL, 37, 1926, 287-291; H. A. Witkin and S. E. Asch, Studies in space orientation: III. Perception of the upright in the absence of a visual field, *J. exp. Psychol.*, 38, 1948, 603-614.

[3] Hermann Aubert, Eine scheinbare bedeutende Drehung von Objecten bei Neigung des Kopfes nach rechts oder links, *Arch. path. Anat. Physiol.*, 20, 1861, 381-393; Mulder, *op. cit.*, 340-352; W. A. Nagel, Ueber das Aubert'sche Phänomen und verwandte Täuschungen über die vertikale Richtung, *Z. Psychol.*, 16, 1898, 373-398; Müller, *op. cit.*, 109-244; Kleint, *op. cit.*, 109-138; Miller and Graybiel, *op. cit.*, 915-919.

[4] Aubert, *op. cit.*, 381-393.

[5] G. Alexander and R. Bárány, Psychophysiologische Untersuchungen über die Bedeutung des Statolithenapparates für die Orientierung in Raume an Normalen

*S*s by no means proves that the labyrinths do not influence this perception. A search for qualitative differences in illusory response between normal and labyrinthine defective *S*s must rest on the assumption that the vestibular apparatus is solely or predominantly the cause of the illusion. An alternate approach, and the one used in the present study, is to test for significant quantitative intergroup differences in visual orientation based upon nonvisual cues.

METHOD

Subjects. Nine normal men, 17 to 32 years of age, and 10 bilateral labyrinthine-defective (*L-D*) men, 20 to 43 years of age, served as *S*s. All normal *S*s were students, 7 from medical schools. Three of the *L-D S*s were instructors and 7 were students from Gallaudet College; all were deaf and had lost the function of the semicircular canals. The clinical findings and results of functional tests of auricular sensory organs of the *L-D* group are presented in Table I. The *S*s were by design separated into two groups according to whether or not they possessed functional integrity of their vestibular organs as determined by caloric irrigation, nystagmography, and counterrolling (Table I). Several of the *L-D S*s revealed some residual function of the otolith organs (Counterrolling Index, Table I), but in

TABLE I

CLINICAL FINDINGS AND RESULTS OF FUNCTIONAL TESTS OF AURICULAR ORGANS IN *S*S WITH LABYRINTHINE DEFECTS

S	Age	Deafness		History	Hearing		Caloric Test*		Counter-rolling index (CI)
		etiology	age of onset	motion sickness	R	L	R	L	
ST	20	meningitis	12½ yr.	Car, Bus†	≤130 db	≤135 dv	Neg[1]	Neg[1]	117
PE	33	meningitis	12 yr.	None	None	None	Neg	Neg	30
GU	21	meningitis	4½ yr.	None	≤145 db	≤145 db	Neg[2]	Neg[3]	89
HR	29	meningitis	13 yr	Small boat‡	None	None	Neg	Neg	53
LA	23	meningitis	6 yr.	None	≤115 db	≤110 db	None	None	109
ZA	20	meningitis	3½ yr.	None	≤135 db	≤130 db	None	None	36
JO	34	meningitis	7½ yr.	None	None	None	None	None	176
MY	25	meningitis	8 yr.	None	None	None	None	None	82
DO	43	meningitis	13 yr.	None	None	None	None	None	74
PI	22	meningitis	3 yr	None	None	None	None[4]	—[5]	85

* Observable nystagmus when tympanum irrigated with cold water (4.5–6°C); numerals refer to nystagmographic findings: [1] vertical nystagmus(?); [2] nystagmus(?); [3] minimal nystagmus; [4] "blink" nystagmus; [5] not tested.

† Nausea.

‡ Malaise.

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und Taubstummen, *Z. Psychol.*, 37, 1904, 321-362, 414-457; Müller, *op. cit.*, 109-244.

[6] H. Feilchenfeld, Zur Lageschätzung bei seitlicher Kopfneigung, *Z. Psychol.*, 31, 1903, 127-150; Alexander and Bárány, *op. cit.*, 187-192; M. H. Fischer, Messende Untersuchungen über die Gegenrollung der Augen und die Lokalisation der scheinbaren Vertikalen bei seitlicher Neigung des Körpers, Kopfes und Stammes: III. Mitteilung. Untersuchungen an einem Ertaubten mit Funktionsuntüchtigkeit beider Vestibularapparate und einem einseitig Labyrinthlosen. *Von Graefes Arch. Ophthal.*, 123, 1930, 509-531.

each case it was substantially less than in any of the normals.[7] An attempt was made to select *S*s with similar age, intelligence, level of education (college), and good general health that differences in perception attributable to factors other than vestibular would be minimized.

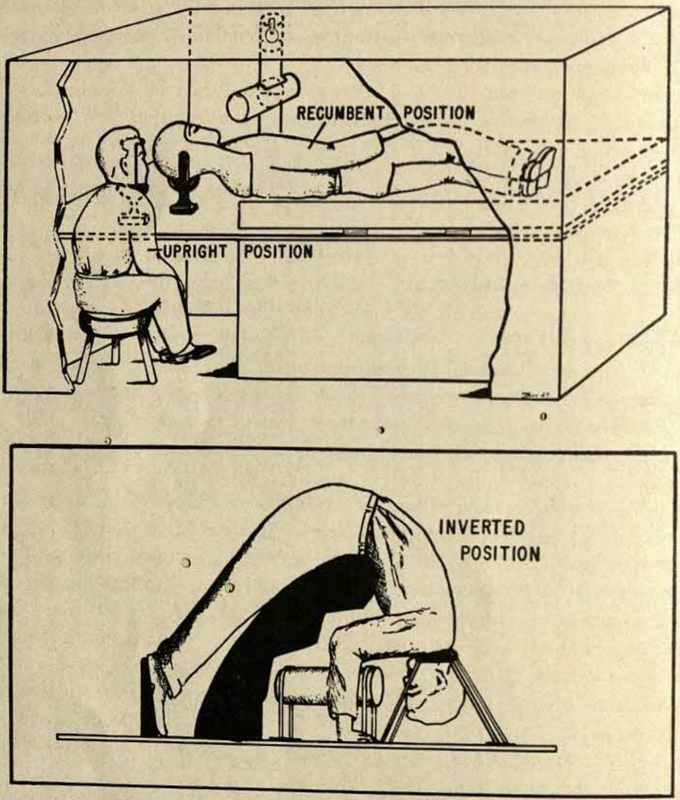

FIG. 1. DIAGRAM OF THE APPARATUS USED TO DETERMINE THE LOCATION OF THE VISUAL HORIZONTAL WITH THE *S* IN AN UPRIGHT, RECUMBENT, AND INVERTED POSITION

Apparatus. The apparatus used has been described previously and is depicted in Fig. 1 along with the assumed positions.[8] With head inverted, the *S* rested on foam rubber supports at his mid-section and shoulders with his legs braced against a footrest.

The target, placed about 1.5 m. from *S*'s face, consisted of a narrow luminous line broken at the center to form a star that could be rotated about its central point either by the *S* or *E*. The rotary position of the star to the nearest 0.25° relative to true horizontal was indicated on a dial which was completely shielded from the *S*'s view. A lamp containing a 75-w. bulb was used to light the interior of the cubicle which contained several strong vertical and horizontal contour cues; similarly, in the inverted position the overhead room light illuminated the whole interior of the room which contained numerous spatial cues. When these lights were turned off, only the luminous star-target was visible.

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[7] Miller and Graybiel, A comparison of ocular counterrolling movements between normal persons and deaf subjects with bilateral labyrinthine defects, *Ann. otol., etc., St. Louis*, 72, 1963, 885-893.

[8] Miller and Graybiel, *op. cit.*, 915-919.

Communication between the *S* and *E* was accomplished by voice or light signal and between *E* and recorder by an electronic sound-system that was inaudible to *S*. A vibrator device applied to the fingers of the *L-D S*s was used by the *E* for signalling purposes.

An electronic timer provided the signal every 2 sec. in the continuous setting method for recording the position of the target.

Procedure. The experiments were carried out in a dark room where *S* was (1) seated, (2) lying on his left side, or (3) with his head inverted. For the first two conditions the room contained an inner cubicle in which *S* was positioned. Two tasks were assigned: (1) *E* offset the luminous line, and *S* was required to reposition it coincident with his apparent horizontal; (2) *S* was required to maintain the luminous line at horizontal throughout the entire experimental period. Task 2 always followed Task 1 and both tasks were used in each of three positions. A rest-period of at least and usually much more than 1 hr. was required when the head (body) position was changed. In each position, *S* was adjusted for adequate comfort and rigidity. The target was placed in *S*'s fronto-parallel plane and directly before his right eye; his left eye was occluded by an opaque patch. Every *S* was retested on a separate day by both tasks in the upright and recumbent positions.

Single settings. With background objects illuminated, *E* offset the star and signalled *S* to restore it to horizontal. During the offset, the star was not illuminated in order to eliminate any cues that might result from viewing the rotation of the star. *S* was carefully instructed to regard the "star" target as a remote object dissociated from himself when judging its position. *S* signalled each time he had reset the target to its apparent horizontal. Ten offsets with 10 subsequent settings to the horizontal were made. No time limit was imposed. This method was then repeated with the background lights turned off.

Continuous settings. When *E* initially offset the unilluminated star, the background lights were turned on so that *S* was visually oriented to his surroundings. On signal, the star was illuminated, and *S* set the star immediately and as quickly as possible to its subjective horizontal. He attempted to maintain it in this position by continuous adjustments whenever necessary during the entire test-period. The position of the star to the nearest 0.25° was recorded every 2 sec. throughout each test-session in which the background was rendered alternately light for 2 min. and dark for 5 min. Three such light and dark cycles followed by a single light period constituted a test-session (23-min. duration) in the erect and recumbent positions. For the more uncomfortable inverted position, the test was shortened to 16 min. by eliminating one dark and light period.

The lag in the corrective response of the *S*s to perceived spontaneous inclinations of the star from apparent horizontal previously had been found to be approximately

2 sec. in duration.[9] All data were corrected for this factor by subtracting 2 sec. from the recorded response time.

RESULTS

Single settings. Since each *S* in all head (body) positions consistently localized his apparent horizontal close to the physical horizontal when visual cues were present, no statistical inquiry into test-reliability under these conditions was considered necessary. On the other hand, visual localization of the horizontal based upon nonvisual cues was more variable and less accurate so that under these conditions a measure of test-reliability was important in determining the stability of perceptual judgment. Coefficients of reliability of the individual sessions (I and II) and the test-retest (I vs. II) are presented in Table II. It is apparent that intratest reliability as calculated by the split-half method and corrected by the Spearman-Brown prophecy formula is quite high for each group tested in the upright and recumbent positions.[10] Interest reliability (Table II), however, was found to be considerably lower. This contrasts with the high test-retest correlations (0.85, 0.88) reported by Witkin in his tilting-room-tilting-chair experiments,[11] but agrees in essence with the variance of an individual's perception of horizontality (or verticality) between observation periods as reported by other authors.[12]

TABLE II

INTRA- AND INTER-TEST RELIABILITY FOR BOTH GROUPS OF *S* IN THE UPRIGHT AND RECUMBENT POSITIONS

Position	*S*s	Reliability coefficients of judgments based upon nonvisual cues		
		Session I (odd vs. even trials*)	Session II (odd vs. even trials*)	Session I vs. II (all trials)
Upright	Normal	.79	.90	.29
	L-D	.86	.90	.16
Left Side	Normal	.91	.93	.33
	L-D	.68	.77	.28

* Corrected by the Spearman-Brown Formula.

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[9] *Idem*, 915-919.

[10] J. P. Guilford, *Psychometric Methods*, 1936, 418-419.

[11] Witkin, The nature and importance of individual differences in perception. Interrelationships between perception and personality: A symposium. Part II, *J. Personality*, 18, 1949, 145-170.

[12] Mulder, *op. cit.*, 340-352; Feilchenfeld, *op. cit.*, 127-150; Müller, *op. cit.*, 109-244; Fischer, Messende Untersuchungen über die Gegenrollung der Augen und die

To gain a more reliable representation of an individual's ability to judge the horizontal, values obtained in both sessions were combined for the intergroup comparisons (Table III), Magnitude without regard for direction (average error) of inclination of phenomenal horizontal with respect to physical horizontal was used as the dependent variable. In the upright and inverted positions, settings tended to vary in both the clockwise (+) and counterclockwise (−) direction from true horizontal; in the recumbent position, settings were always counterclockwise (−), counterbalancing the illusory shift of phenomenal horizontal in the invariable Aubert direction.

TABLE III

INTERGROUP COMPARISONS OF AVERAGE SETTINGS IN EACH HEAD (BODY) POSITION WITH AND WITHOUT VISUAL BACKGROUND CUES

Position	*S*s	Average setting background		Difference in average setting*
		illuminated	dark	
Upright	Normal	1.22±0.53	1.39±0.72	0.17*P* not sig.
	L-D	0.83±0.27	2.62±0.99	1.78*P* <0.01
		diff. 0.39	1.23	
		P <0.10	<0.01	
Recumbent	Normal	1.63±0.48	17.78±8.38	16.25*P* <0.01
	L-D	1.63±0.57	29.67±7.82	28.04*P* <0.01
	L-D	diff. 0.10	11.89	
		P not sig.	<0.01	
Inverted	Normal	1.28±0.99	3.61±1.91	2.33*P* <0.01
	L-D	1.02±0.55	3.94±1.99	2.92*P* <0.01
		diff. 0.26	0.33	
		P not sig.	not sig.	

* Illuminated versus dark background.

The *t*-test of significance in intergroup differences in mean absolute settings (Table III) revealed that, when visual cues were present, the two groups were on the average similar and extremely accurate in visually localizing the horizontal. Significant intergroup differences in this perception were found, however, in the upright and recumbent positions when vision was restricted to the line target. When upright, the *L-D* *S*s, unlike the normals, were significantly less accurate in their settings after the visual background was eliminated. In the recumbent position, both

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Lokalisation der scheinbaren Vertikalen bei seitlicher Neigung (des Kopfes, des Stammes und des Gesamtkörpers): I. Mitteilung: Neigungen bis zu 40°. *Von Graefes Arch. Ophthal.,* 118, 1927, 633-680; Graybiel and Brant Clark, Perception of the horizontal or vertical with head upright, on the side, and inverted under static conditions and during exposure to centripetal force, *Aerospace Med.,* 33, 1962, 147-155.

groups were significantly and markedly affected by the background change, but more importantly the *L-D* group suffered a significantly greater loss in ability to localize the horizontal. In the inverted position, both groups showed a smaller but significant decrement in performance with the removal of visual cues, but no significant intergroup difference was found.

Continuous settings. The results obtained by almost continuous monitoring of the visual localization of horizontal are graphically portrayed in Figs. 2 through 7. Data obtained in the original test-session are plotted as a

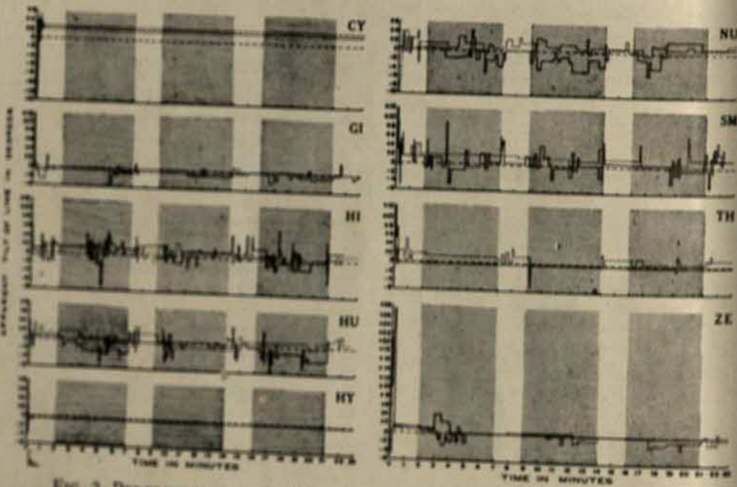

FIG. 2. PERCEPTION OF THE HORIZONTAL BY NORMAL Ss PLACED IN AN UPRIGHT POSITION AS A FUNCTION OF TIME AND VISUAL FRAMEWORK (Alternate shaded and white strips denote intervals in which the background was darkened and illuminated, respectively.)

solid line, while those in the retest appear as a broken line. The original test-data were lost for the upright position in one *L-D S, DO*. Those data collected during periods in which the visual field was illuminated or dark are plotted against a white or stippled gray background, respectively. The reliability of the findings can be judged visually by the similarity among the results obtained under dark or illuminated conditions of the background. For a given test or retest, the reliability of the settings is quite satisfactory in most cases; intersessional comparisons, on the other hand, as in the case of the single settings, often reveal considerable changes in the perceived magnitude and sometimes direction of the illusion.

The time-course pattern of the illusion, as determined by the almost continuous method of recording, was qualitatively similar for all the *Ss* in the present experiment and closely resembled the findings of a previous study.[13] The target was localized accurately with little variability in each of the three head (body) positions as long as visual cues were present. Each time these cues were removed, except for normal *Ss* in the upright position (Fig. 2), the target, after a certain lag period, appeared to move slowly away from physical horizontal and to oscillate. This perceived oscillatory movement of a target about its center has been termed *rotary* *autokinesis*.[14] The general performance of the normal *Ss* positioned upright was relatively unaffected by the successive alternation of a dark and illuminated background. On the other hand, the over-all performance of the *L-D Ss* in this position is clearly more dependent (Fig. 3) upon the visual background. The accurate and fixed localization of the horizontal sharply contrasts with the increased error and with rotary autokinesis measured during the illuminated and dark background intervals, respectively.

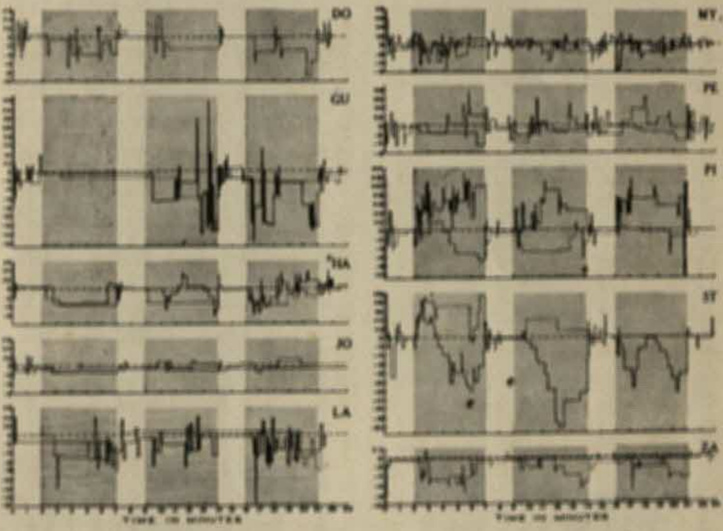

FIG. 3. PERCEPTION OF THE HORIZONTAL BY LABYRINTHINE-DEFECTIVE *Ss* PLACED IN AN UPRIGHT POSITION AS A FUNCTION OF TIME AND VISUAL FRAMEWORK
(Alternate shaded and white strips denote intervals in which the background was darkened and illuminated, respectively.)

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[13] Miller and Graybiel, *op. cit.*, 915-919.
[14] *Ibid.*, 915-919.

The dependency of the *L-D Ss* upon visual cues for accurate judgments increases in the recumbent position (Fig. 5) and is evident to a lesser extent in the inverted position (Fig. 7). A qualitatively similar and significant regular change in performance as a function of visual framework was also found in the normal *Ss* when recumbent (Fig. 4) and inverted (Fig. 6).

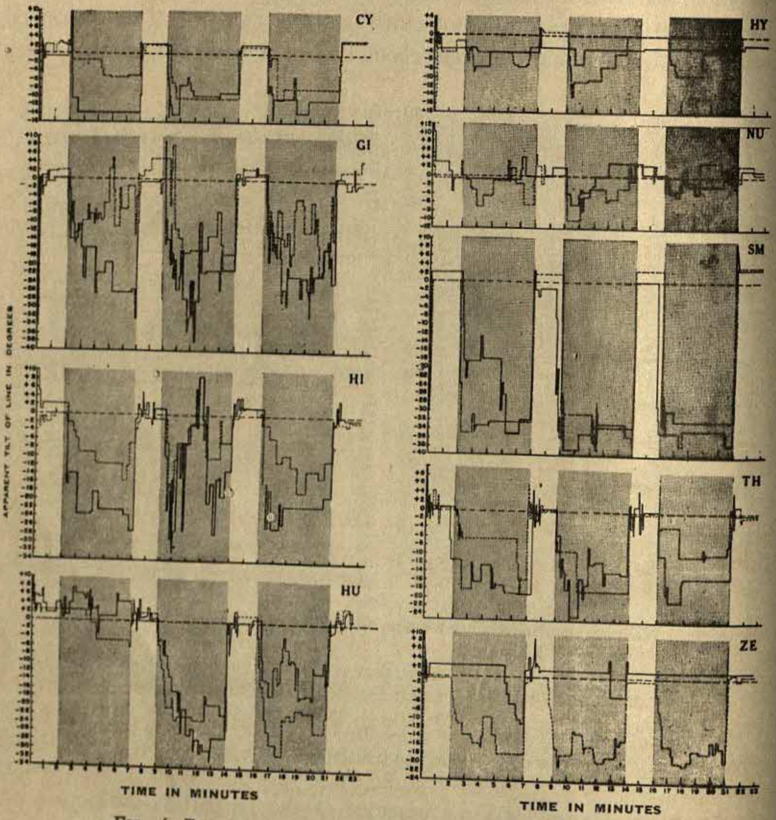

FIG. 4. PERCEPTION OF THE HORIZONTAL BY NORMAL *Ss* PLACED IN A RECUMBENT POSITION AS A FUNCTION OF TIME AND VISUAL FRAMEWORK
(Alternate shaded and white strips denote intervals in which the background was darkened and illuminated, respectively.)

Examination of Figs. 2 through 7 reveals considerable inter- and intragroup differences in perception of horizontality using nonvisual cues. The majority of *L-D Ss* in the upright and recumbent position would appear to manifest greater error than the normal *Ss*. When comparing results of the

L-D and normal *Ss* in a recumbent position, it should be noted that the size of the ordinate scale in Fig. 5 relative to Fig. 4 is reduced almost one-third. Intergroup differences in performance in each of the three head (body) positions were tested by the Mann-Whitney statistic (U-test).[15] All the *Ss* were ranked according to the average over-all magnitude of error as judged by inspection. In the upright and recumbent positions, some overlap in the distribution of the normal and *L-D Ss* was found, but statistically the groups were significantly different at the 0.02 level (upright) and 0.025 level (recumbent). In the inverted position, the considerable overlap of the groups in rank order yielded no statistical intergroup difference.

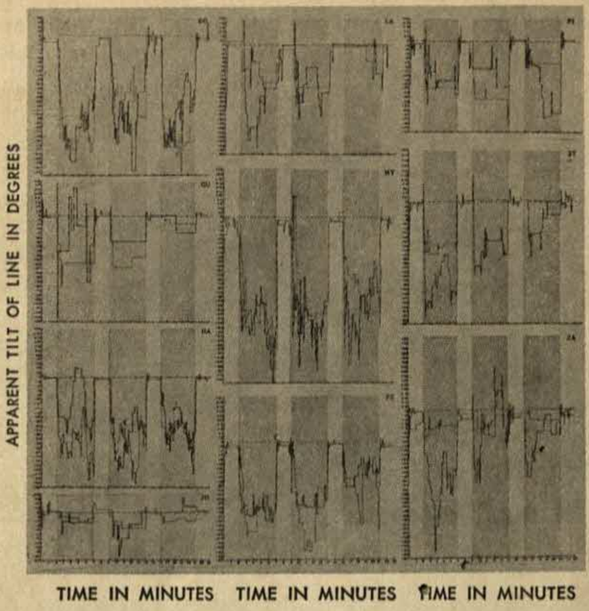

FIG. 5. PERCEPTION OF THE HORIZONTAL BY LABYRINTHINE-DEFECTIVE *Ss* PLACED IN A RECUMBENT POSITION AS A FUNCTION OF TIME AND VISUAL FRAMEWORK
(Alternate shaded and white strips denote intervals in which the background was darkened and illuminated, respectively.)

DISCUSSION

Individual performances in each of the three head (body) positions with the visual background illuminated proved that basically every *S* was quite capable of making precise adjustments to the horizontal when adequate cues were available. For this reason the intergroup differences found in the present study would appear to be most logically explained by the difference in cues for visual orientation furnished by the vestibular organs (probably the otoliths). Other factors,[16] obviously, would be involved and contributed to the variability among *S*s within each group, but none could account for the significant intergroup differences. The presence of some residual otolith function as revealed by the counterrolling index in certain *L-D S*s could have acted to reduce these intergroup differences.[17]

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[15] Donavan Auble, Extended tables for the Mann-Whiteny statistic, *Bull. Inst. educ. Res.*, 1, 1953, No. 2.

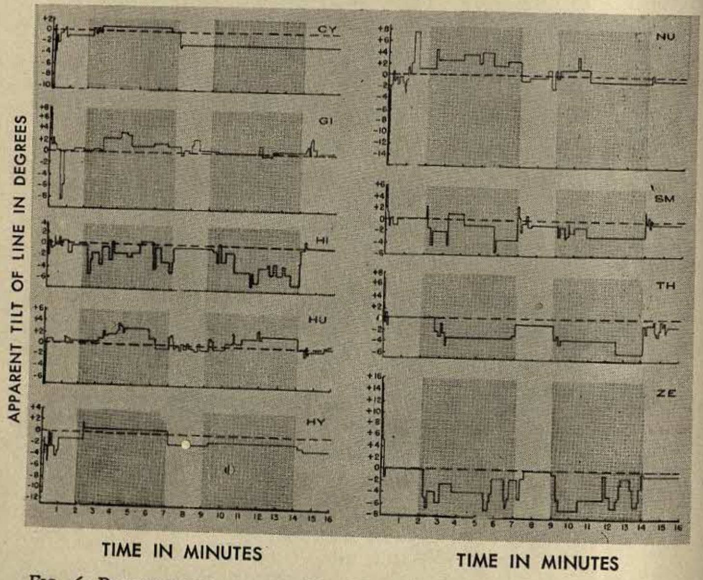

FIG. 6. PERCEPTION OF THE HORIZONTAL BY NORMAL *S*S PLACED IN AN INVERTED POSITION AS A FUNCTION OF TIME AND VISUAL FRAMEWORK

(Alternate shaded and white strips denote intervals in which the background was darkened and illuminated, respectively.)

When upright, the normal *S*s showed no dependence upon a visual framework for precise localization of the horizontal as did the *L-D S*s. The latter, deprived of visual and normal otolithic cues, however, still were relatively accurate in their settings compared to the recumbent position. In the recumbent position, both groups perceived Aubert's illusion to a marked extent, but the magnitude of the illusion was significantly less in the normal *S*s. When the head and therefore the otolith apparatus was inverted, a small yet significant increase in constant error was measured for both groups with no significant intergroup differences.

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[16] Witkin, *op. cit.*, 145-170, 1949; C. W. Mann and R. O. Boring, The role of instruction in experimental space perception, USN Sch. Aviat. Med. Proj. Rep., Pensacola, Florida, 1952.

[17] Miller and Graybiel, *op. cit.*, 885-893.

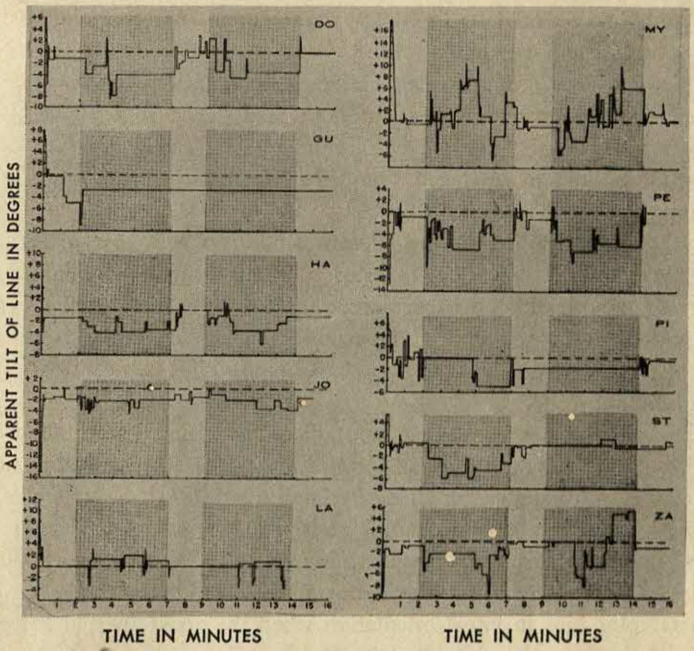

FIG. 7. PERCEPTION OF THE HORIZONTAL BY LABYRINTHINE-DEFECTIVE *S*S PLACED IN AN INVERTED POSITION AS A FUNCTION OF TIME AND VISUAL FRAMEWORK
(Alternate shaded and white strips denote intervals in which the background was darkened and illuminated, respectively.)

The interesting fact that the judgment of horizontality (or verticality) is relatively accurate in the head down position has been reported previously for normal[18] and 'labyrinthless' *S*s.[19] Unlike in the erect and recumbent head positions, according to the "inward shearing" and pressure theories of otolith stimulation,[20] the utricles of the otolith organs in the inverted head position could not be stimulated physiologically. If the utricular component only is involved in this perception, one would expect the normal and *L-D Ss* in this study to perform in a similar fashion, since their judgments would be based apparently upon cues common to both groups. According to counterrolling data, however, the saccules of the otoliths would be active in man under these conditions.[21] Other experimental evidence is not so clear-cut. The oculogravic illusion, a specific indication of otolith function, was found in the head down position in one study but not in another.[22] The difference in those two studies in the position of the *S*'s body with respect to his inverted head might explain this discrepancy. Since, in the present study, no significant intergroup differences were found in the inverted position, it might be assumed that either the saccules in this particular position do not influence the perception of the horizontal in any appreciable way or that the saccular component was functionally intact in all *Ss*, a possibility which was not investigated.

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[18] Aubert, *op. cit.*, 381-393; Nagel, *op. cit.*, 373-398; Fischer, Messende Untersuchungen ueber die Gegenrollung der Augen und die Lokalisation der scheinbaren Vertikalen bei seitlicher Neigung des Gesamtkörpers bis zu 360°: II Mitteilung. *Von Graefes Arch. Ophthal.*, 123, 1930, 476-508; Graybiel and Clark, *op. cit.*, 147-155.

[19] Fischer, *op. cit.*, 509-531.

The evidence here presented contradicts any claim that the labyrinth is a factor which causes error in localization associated with head (body) tilt and is corroborated by an earlier finding that *L-D Ss* perceive significantly greater amounts of autokinesis, another indicator of egocentric visual localization.[23] More recently, Miller and Graybiel, using normal and *L-D Ss*, showed further evidence of the importance of the otolithic gravireceptor cues in visual orientation when the gravitoinertial force was varied independently of its direction.[24]

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[20] Miller, Counterrolling of the human eyes produced by head tilt with respect to gravity, *Acta otolaryng., Stockh.*, 54, 1962, 479-501; F. H. Quix, De Functie der otolithen, *Ned. Tijdschr. Geneesk.*, 65, 1921, 2670-2673; The function of the vestibular organ and the clinical examination of the otolithic apparatus, *J. Laryng.*, 40, 1925, 425-443; 493-511.

[21] Miller, *op. cit.*, 479-501.

[22] Graybiel, Oculogravic illusion, *Arch. Ophthal., N.Y.*, 48, 1952, 605-615; The importance of otolithic organs in man based upon a specific test for utricular function., *Ann. Otol., etc; St. Louis*, 65, 1956, 470-487; Graybiel and Clark, *op. cit.*, 147-155; J. A. Weaver, and R. F. Gray, The perception of oculogravic illusions by inverted subjects, NADC-MA-6207, Johnsville, Penna., 1962.

[23] Fischer, *op. cit.*, 509-531; Miller and Graybiel, Comparison of autokinetic movement perceived by normal persons and deaf subjects with bilateral labyrinthine defects, *Aerospace Med.*, 33, 1962, 1077-1080.

[24] Miller and Graybiel, Magnitude of gravito-inerital force, an independent variable in egocentric visual localization of the horizontal, USN Sch. Aviat. Med. Proj. Rep., Pensacola, 1964, (in press).

SUMMARY

The influence of the sensory organs of the inner ear on man's ability to align a visual target with the physical horizontal was measured by a discrete and a continuous method of setting. Both test-methods yielded similar results under all test-conditions. When visual background cues were present, settings of the luminous line target to the horizontal were quite accurate in the 9 normal and 10 labyrinthine defective (*L-D*) *S*s placed in three positions: upright, recumbent, and inverted. Removal of empirical visual cues revealed, particularly in the recumbent position, that qualitatively all the *S*s perceived the typical lag in onset, relatively slow rotation to the maximal illusion, and rotary autokinesis. Quantitatively, however, there were significant intergroup differences. When upright, the normals were able on the average to maintain their accuracy while the *L-D S*s deviated significantly in their settings. Both groups of *S*s in the recumbent position perceived the Aubert illusion, but the magnitude of the illusion was considerably less in the normal group. When inverted, both groups were less accurate in the dark but no significant intergroup difference was found. In spite of the fact that there was some overlap in the group distributions of settings obtained in the upright and recumbent positions, indicating other factors were involved, the intergroup perceptual differences are best explained as an effect of loss of otolith function in the *L-D S*s. It was concluded that the otolith organs in man act to increase his accuracy in egocentric visual localization at least in the upright and recumbent positions.

CUE-RESPONSE CORRELATIONS IN THE ATTAINMENT OF A SCALAR CONCEPT

By Hiroshi Azuma, University of Tokyo, and
Lee J. Cronbach, Stanford University

The usual study of concept attainment involves a classificatory concept. *E* divides a collection of stimuli into two classes on the basis of the presence or absence of certain characteristics, and *S* is asked to discover the "concept" or basis for classification. A smaller number of studies have employed a scalar concept. *E* assigns a scale value to each stimulus in the collection by applying some rule to the numerical values of selected attributes; *S* is asked to learn to rate stimuli on this scale. To succeed on the classificatory problem, *S* has to determine which of the attributes are relevant and what conjunctive or disjunctive combination of attribute-values is common to the positive instances of the concept. To succeed on the scalar problem, *S* has to identify relevant attributes and find a way of combining the information into an estimate of the scale-value assigned by *E*. One might regard the classificatory problem as requiring a response on a two-valued scale; each response is either right or wrong. From this point of view, the scalar concept is a more general case.

The present study examines how *Ss* use multiple cues in solving a scaled-concept problem. The desired response to each stimulus corresponds to a weighted sum of certain cue-variables. *S* is required on each trial to estimate the scale-value assigned to the stimulus by the (unknown) rule; he is then told the correct value.

The earliest research of this character, by Smedslund, was strongly influenced by Brunswik's description of the way *Ss* combine cues in perceptual learning.[1] Smedslund displayed two (or three) pointers whose directions changed from trial to trial. *E* defined a variable which we may call *k* by expressing the pointer directions numerically, averaging them, and adding a small randomly determined component in order to make the relationship probabilistic. *S* reported his estimate of *k* on any trial by sliding a knob along an unmarked scale; to provide feedback, *E* then moved the knob to the setting that represented the predetermined value of *k*. More recently, Summers and Uhl studied performance on similar tasks.[2] Each presented stimuli defined by three cues and required *S* to learn to estimate a *k* defined as a weighted sum of the cue-values.

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* Received for publication May 28, 1964. This paper is based in part on a dissertation submitted to the University of Illinois by Hiroshi Azuma in 1960, prepared under the direction of Professors L. J. Cronbach and L. M. Stolurow. The authors were at the University of Illinois. Support from the Bureau of Educational Research and Graduate Research Board, University of Illinois, is also acknowledged.

[1] Jan Smedslund, *Multiple-Probability Learning: An Inquiry into the Origins of Perception*, 1955; Egon Brunswik, *Perception and the Representative Design of Psychological Experiments*, 1956. See also Brunswik, Reasoning as a universal behavior model and a functional differentiation between "perception" and "thinking," in K. E. Hammond (ed.), *Probabilistic Functionalism: Egon Brunswik's Psychology*, 1966.

In none of these studies did *S*s fully solve the problem, although their estimates of *k* did improve to some extent as training continued. Shepard notes that tasks requiring *S*s "to combine factors in subjective decision problems" are usually difficult, and comments as follows:

> On the level of the perceptual analysis of raw sensory inputs, man evinces a remarkable ability to integrate the responses of a vast number of receptive elements according to exceedingly complex nonlinear rules. Yet once the profusion and welter of this raw input has been thus reduced to a set of usefully invariant conceptual objects, properties, and attributes, there is little evidence that they can in turn be juggled and recombined with anything like this facility.[3]

An important feature of the Smedslund, Summers, and Uhl studies was the analysis by means of cue-response correlations. Smedslund, for example, on each block of trials in turn, correlated the scale-values selected by *S* with the settings if one of the pointers, using this as an indication of the extent to which *S* was relying on that cue at that point in training. Cues that were assigned equal weights by *E*, hence having equal relevance, came to be utilized by a given *S* at widely separated times, whereas Brunswik's writings had suggested that the utilization of cues would be in direct correspondence to their demonstrated relevance. Smedslund also obtained a multiple correlation of response with all pointer settings together; this correlation will be high if *S* adds the cues in any consistent fashion, whether or not his weights resemble those used by *E*. The multiple correlation was interpreted as an indication of the degree to which learning of some rule had taken place.

The use of cue-response correlations as a method of analysis derives from a conception of learning in a multiple-attribute situation as a gradual strengthening or weakening of associations, *S* coming to utilize valid cues and to ignore the others. Such an analysis fits the observed behavior to a model that assumes that *S* combines information additively. A similar linear model is used in studies (*e.g.* Hoffman and Ward)[4] in which ratings given by clinicians or officers are correlated with measured attributes of the persons rated. These additive models may be quite inappropriate when *S*s process information relationally.

Brunswik introduced cue-response correlation in his discussion of perceptual processes, and for a time such writers as Smedslund and Bruner found no need to distinguish a perceptual interpretation from deliberate problem-solving.[5] Brunswik himself, however, made a sharp distinction, seeing "ratiomorphic behavior" as complexly mediated and perceptual behavior as a simple composite based on cue-validity.[6]

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[2] S. A. Summers, The learning of responses to multiple weighted cues, *J. exp. Psychol.*, 64, 1962, 29-34; C. N. Uhl, Learning of interval concepts: I. Effects of differences in stimulus weights, *J. exp. Psychol.*, 66, 1963, 264-273.

[3] Roger N. Shepard, On subjectively optimum selection among multiattribute alternatives, in M. W. Shelly, II, and G. L. Bryan (eds.), *Human Judgments and Optimality*, 1964, 257-281.

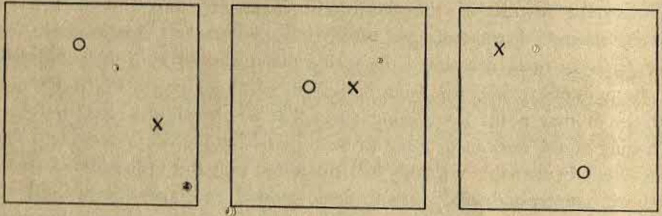
[4] P. J. Hoffman, The paramorphic representation of clinical judgment, *Psychol. Bull.*, 57, 1960, 116-131; J. H. Ward, Jr., and Kathleen Davis, Teaching a digital computer to assist in making decisions, *Tech. Doc. Rep. Lackland Air Force Base*, 1963.

This study will describe the way *S*s improve in a relatively simple four-attribute task. Particular interest will attach to a comparison of the cue-response correlations, which fit the behavior to a linear model, with the *S*s' verbal descriptions of the processing rules they are using at the end of training.

METHOD

Task. A collection of stimuli was prepared, each consisting of a circle and a cross located in a 2.5-in. square frame (Fig. 1). In reporting, we refer to the coördinates of the circles as *x*' and *y*', and those of the cross as *x*" and *y*". Each of these parameters takes on one of four values (0.5, 1.0, 1.5, and 2.0, in inches from the left or bottom margin) with equal probability. No numerical values were given on the figures displayed to *S*, but *S* was given a sheet displaying the four standard stimuli described in Table I. *S* was told that the stimulus-patterns varied with respect to some unknown property *k*, and that he was to learn to judge the *k*-value for each figure. The standard figures, he was told, were ranked in order of *k*-value, with A having the smallest value and D the largest. He was asked to respond to each training stimulus by telling which of the standard stimuli it was most alike in *k*-value, responding A, B, C, or D. The standard stimuli were unlike any of the training or test-stimuli, and gave information as to the differential relevance of the four coördinates.

FIG. 1. SAMPLES OF STIMULI EMPLOYED



A: $x' = 1.0$ $y' = 2.0$ $x'' = 2.0$ $y'' = 1.0$

B: $x' = 1.0$ $y' = 1.5$ $x'' = 1.5$ $y'' = 1.5$

C: $x' = 1.5$ $y' = 0.5$ $x'' = 0.5$ $y'' = 2.0$

The 128 training stimuli were presented in a booklet, six to the page. *S* was to circle his choice on a four-choice answer sheet mounted in an SRA 'self-scorer' and then to punch a hole at that point. If his response was correct, this exposed a colored spot. If his first choice was wrong, he continued punching, without circling, until he located the correct response. A pace of 2 min. per page was suggested. The *Ss* were told, however, that 10 min. extra would be allowed for those who lagged behind this pace. After *Ss* had completed the training booklet, a second (test) booklet of 64 stimuli was presented. *S* responded to these without feedback. At the end of the test, *S* was asked to write the rule or hunch used to estimate *k*.

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[5] J. S. Bruner, J. J. Goodnow, and G. A. Austin, *A Study of Thinking*, 1956, 231.

[6] Compare, for example, the "digital" vs "analogue" processes of G. A. Miller, Eugene Galanter, and K. H. Pribram, *Plans and Structure of Behavior*, 1960, 91-92.

For purposes of analysis, the stimuli were classified into overlapping blocks of 32; Block 1+2 consists of Trials 1—32, Block 2+3 of Trials 17—48, and so on up to Block 7+8 (Trials 97—128). Within each half-block of 16 trials, each possible combination of *x*' and *x*'' appeared once. Values of *y*' and *y*'' were so chosen that the four parameters were essentially independent. Within each block of 32 trials four stimuli appeared twice, to provide information on the stability of response.

E had defined *k* as the weighted average (2*x*'' + *x*'')/3. Since the values of this average for the standard stimuli were 0.6, 1.0, 1.5, and 1.9 respectively, the answers A, B, C, D, were correct for *k*s in the intervals 0-0.8, 0.8-1.25, 1.25-1.7, and 1.7-2, respectively. (Since all weighted averages ended in 0.00, 0.33, or 0.67, there were no borderline values.) Because of the broad grouping, *S* could estimate *k* correctly if he combined *x*' and *x*'' with any weights between 1:1 and 3:1. The correlation of the parameters with the rounded *k* values were as follows: *x*', 0.84; *x*'', 0.45; *y*', 0; y'', 0.

TABLE I

SPECIFICATIONS OF STANDARD STIMULI
(in inches)

Standard figure	Coördinates			
	x'	y'	x''	y''
A	.6	.4	.6	.8
B	1.0	.8	1.0	1.2
C	1.5	1.3	1.5	1.7
D	1.9	1.7	1.9	2.1

Our task was designed to be easier than Smedslund's. These differences may be noted: Our stimuli vary in four dimensions, two of which are irrelevant. No noise is introduced into feedback. Our stimulus variables are closely related, phenomenally, to the scale used for responding. The most striking departure of our task from those of Summers and Uhl is the presence of irrelevant cues.

Subjects. Sixteen students in psychology were the *Ss*. They served to fulfill a course requirement.

RESULTS

Method of analysis. The data consist of the A, B, C, or D response given by each *S* to each one of 128 training stimuli and 64 test-stimuli. These response-values were replaced by numbers for statistical treatment.

Our first analysis followed Smedslund and others in calculating the product-moment correlation of *x'*, *y'*, *x''* and *y''* with *S*'s response, there being one correlation on each trial. These are the correlations Uhl refers to as "criterialities." Since the parameters varied independently, these correlations are proportional to the beta weights of the cues in the linear regression equation that predicts *S*'s response. The correlation of *S*'s response with *k* (a measure of accuracy) was also calculated for each block. As an index of stability of response, the difference between *S*'s two responses to each pair of repeated items was determined and the four such differences in each block were added without regard to sign.

Learning as indicated by cue-response correlations. Table II presents two examples of individual results. S_{12} attained perfect performance in the test trials, while S_5 was erratic throughout. The stability index is zero when performance is perfectly consistent; for chance performance the expected value is near five. S_{12} evidently did not stabilize his performance until Block 3. S_5, who performed with little better than chance consistency, may have changed hypotheses frequently or he may have responded haphazardly.

TABLE II

LEARNING RECORDS OF TWO *S*s

Correlations of *S*'s response with characteristics of the figure

	Figure characteristic	Learning blocks*							Test blocks		
		1+2	2+3	3+4	4+5	5+6	6+7	7+8	1+2	2+3	3+4
S_{12}	x'	.14	*.71*	*.84*	*.85*	*.82*	*.77*	*.80*	*.85*	*.85*	*.84*
	y'	*.24*	.18	−.01	.01	−.07	−.07	−.02	−.02	−.02	−.00
	x''	.11	.06	.20	.24	.36	.44	.41	.50	.50	.45
	y''	−.05	.04	.06	.10	.19	.15	.08	−.01	−.01	−.12
	Criterion *k*	.18	.56	.74	.81	.88	.84	.88	1.00	1.00	1.00
	Stability index	4	4	1	1	0	1	1	0	0	0
S_5	x'	.05	−.12	.02	*.37*	*.52*	.37	.01	*.44*	.15	.10
	y'	*.44*	.29	*.37*	.22	−.01	.03	.16	−.04	.09	.03
	x''	−.33	−.12	.14	.28	.46	*.64*	*.27*	.19	.24	−.13
	y''	.35	*.34*	−.02	−.22	−.12	−.07	.24	.13	*.27*	*.10*
	Criterion *k*	−.11	−.12	.11	.44	.67	.61	.16	.48	.21	.00
	Stability index	6	3	4	3	1	2	3	5	2	3

* Largest cue-response correlation in each block is shown in italics.

Fig. 2 plots the cue-response correlations for S_{12} for the two relevant cues. Each point represents the correlations for one block. The interpretation of these diagrams will be discussed in some detail below. S_{12} quickly detected the relevance of x', and only gradually increased the weight given x''.

Fig. 3 shows the mean cue-response correlations for each block. Correlations were transformed to z, averaged, and converted back to r. In this chart, stars indicate the correlations that would be associated with perfect performance. The correlations of x' and x'' with response tended to increase, while those of y' and y'' decreased. The mean correlation of x' with response is always higher than that of x'', reaching at the end approximately to a two-to-one ratio. Thus the Ss as a group progressed toward

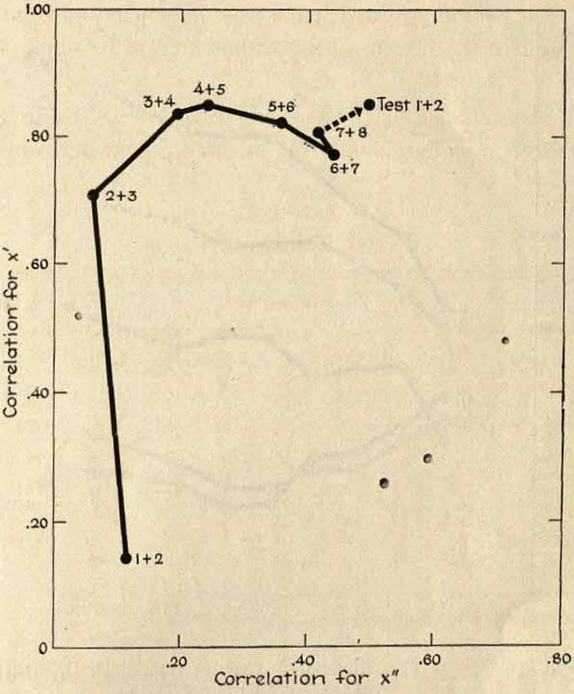

FIG. 2. CORRELATIONS OF RESPONSE WITH RELEVANT CUES FOR S_{12} ON SUCCESSIVE (OVERLAPPING) BLOCKS

proper weighting of relevant cues. Average curves, however, do not describe the learning of individuals; they are especially misleading here because the cue-response correlations are not independent, a high correlation for one cue limiting the size of the others.

For a closer look at the variability among individuals, we use further charts like Fig. 2. Each point in Fig. 4 represents the correlation of an S's response with x' and x'' for the block indicated. Since correlations for y' and y'' cannot be displayed, a small circle, rather than a solid point, is used whenever *S*'s cue-response correlation for *y*' or *y*'' is greater than the average of his correlations for *x*' and *x*'' on that block. Two circles and a straight line are added to the figure to help evaluation.

When less than half of an *S*'s response variance is accounted for by the best linear composite of *x*' and *x*'', his point falls within the inner circle. If he depends wholly on *x*' and *x*'', but responds first to one and then the other, his point stays on the diagonal line. Any *S* whose response is entirely accounted for by some linear composite of *x*' and *x*'' falls on the outer circle. The star again represents the correct weighting. It can be seen especially in Fig. 4 B that the *S*s move upward and outward rather rapidly, save for 5 *S*s left at the post. In the last training block and the first test-block, 10 out of 16 *S*s are in the region between the diagonal and the outside circle. These *S*s detected relevant cues and combined them. On the test, 7 *S*s show the correct weighting and 3 are near to equal weighting.

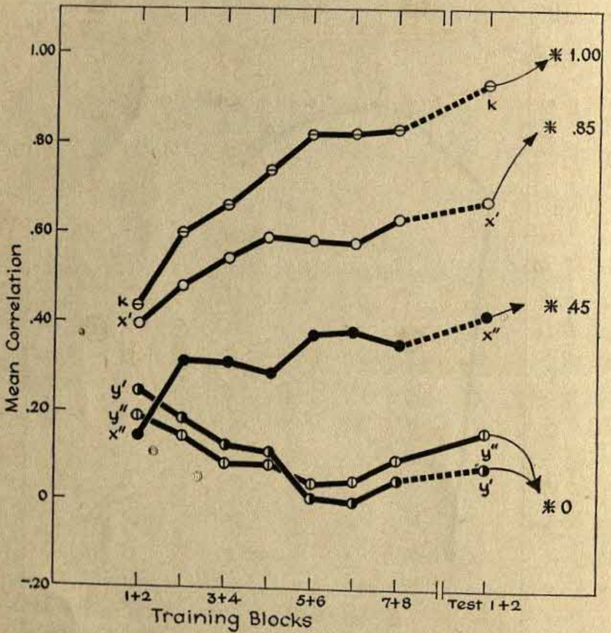

FIG. 3. MEAN CUE-RESPONCE CORRELATIONS AT VARIOUS STAGES OF LEARNING

The results for all the *S*s may be summarized as follows: In Blocks 1 + 2 some were like S_{12}, whose response was not well accounted for by any additive combination of the cues, some paid equal attention to all four cues, some apparently tried the idea that the position of the circle (defined by x' and y') was important, and so on through many other variations. Most commonly, the relevance of x' was detected early in the learning; x'' was not yet differentiated from irrelevant cues, and x'', y', y'' all had low but positive correlation with *S*'s response. After the first block the weight assigned to x'' typically rose, while the weights of irrelevant cues declined to near zero.

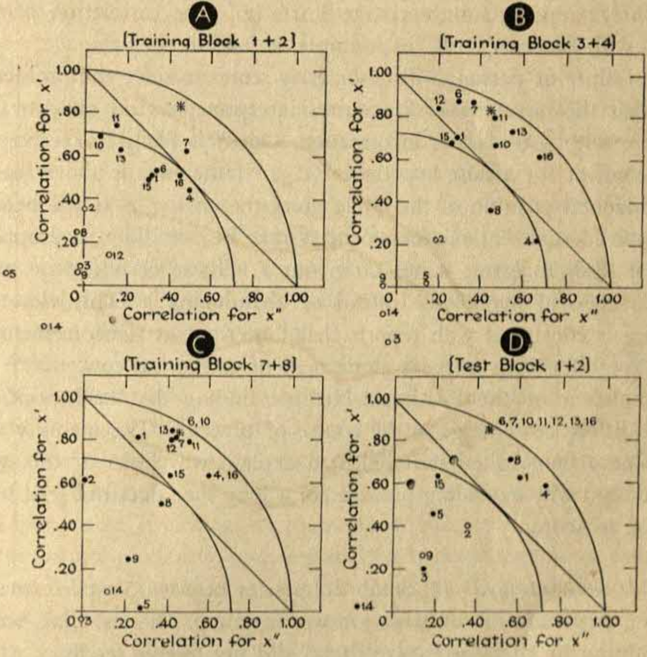

FIG. 4. CUE-RESPONSE CORRELATIONS OF INDIVIDUAL *Ss* ON THREE TRAINING BLOCKS AND ONE TEST-BLOCK

Consistent individual differences. Good and poor learners exhibited a strikingly different course of learning. The *Ss* successful on the tests were also, with few exceptions, performing very successfully as early as Blocks 3 + 4 (Trials 33-64). Conversely, those poor on test-trials were poor on Blocks 3 + 4 (Fig. 4). Evidently, successful *Ss* early discriminated relevant from irrelevant cues or perhaps mastered certain of the easier configurations. By Trial 32, 7 *S*s had learned to judge stimuli in which x' equals x''. Five of these were among the 7 *S*s who responded to Test-Blocks 1 + 2 without error. Success on these items indicate that *S* had learned to disregard the irrelevant cues y' and y''.

Those who were seriously confused as late as Trial 64 were not helped by further trial-with-feedback. Block-to-block correlations of learning measures show a much higher degree of consistency on this task than Humphreys reports for associative learning.[7] The correlation of Blocks 3 + 4 with Blocks 7 + 8, for example, is 0.93 in our data.

The failure of persons with poor early scores to solve the problem suggests that the unsuccessful *S* acquires interpretations on early trials that interfere with use of later information. Our *S* is likely to receive partial reinforcement for a false hypothesis (*e.g.* "if the cross is above the circle, the horizontal position of the circle gives the answer'). If the hypothesis has received sufficient reinforcement, *S* may be unwilling to drop it even when it leads to error; rather often our *S* adds to or otherwise modifies the unsuccessful hypothesis instead of abandoning it. This view of the problem is consistent with reports that "intermittent reinforcement of an irrelevant dimension impedes attainment of categorical concepts."[8] It also is reminiscent of the Fleishman-Hempel finding that task-specific individual differences emerge in the course of practice.[9] The inability of *S* to overcome a poor initial start helps to explain why tasks of this sort are difficult and why extending practice for a long time does not lead to much increase in score.

Trials without feedback. Some differences between *S*'s performance on training trials and on test-trials are worth noting. The test-trials were like the training in all respects save that *S* did not receive feedback as to the correctness of his responses. Six *S*s had appreciably better correlations with the criterion on Test Blocks 1 + 2 than on the last training block (see Fig. 4E). This suggests that on late training trials they were deliberately experimenting with new hypotheses even though they risked error in trying these ideas.[10] It follows that scores on trials with feedback are not a suitable measure of concept attainment and probability learning, and that blocks of non-feedback trials for testing purposes should be inserted at various points in training. *S*'s strategy for obtaining information may not be the same as his strategy for maximizing a test score.

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[7] L. G. Humphreys, Investigations of the simplex, *Psychometrika,* 25, 1960, 313-323.

[8] L. E. Bourne, Jr., and R. C. Haygood, Effects of intermittent reinforcement of an irrelevant dimension and task complexity upon concept identification, *J. exp. Psychol.,* 60, 371-375.

[9] E. A. Fleishman and W. E. Hempel, Jr., Changes in factor structure of a complex psychomotor test as a function of practice, *Psychometrika,* 19, 1954, 239-252.

[10] Cf. Bruner, Goodnow, and Austin, *op. cit.,* 194.

We have postponed to this point consideration of the later test-trials. The last 32 stimuli (Test Blocks 3 + 4) differed in no systematic way from Test Blocks 1 + 2, but there was a striking decrease in performance on them. While 7 *S*s gave essentially perfect performances on both blocks, 8 of the remaining 9 *S*s did worse (see Table III). The stability measures indicate that these *S*s were not merely becoming erratic. Nor, according to the protocols, did they shift to a stereotyped response. While the loss of accuracy is small, the consistent trend suggests that the *S*s who did not fully master the solution during training were unable to encode what knowledge they had so as to retain it even for a short time. Goodnow found that, on trials without feedback, *S*s who had hitherto been reinforced probabilistically tended to fall back on a simple hypothesis that kept error within limits even if it could not be consistently correct.[11] Our data, however, show no such effect.

The results are reminiscent of the role of feedback in maintaining perceptual skill. Most skills and associations are maintained by using them in situations where their practical consequences provide feedback. For some discriminations learned by practice where feedback is arranged by the instructor (critiques, scores, etc.), feedback from the environment after the end of training is lacking, inconsistent, or equivocal. Training of submarine personnel to classify sounds heard on hydrophones is an example; spelling is another. Our findings suggest that only the person who understands the difference between appropriate and inappropriate responses well enough to criticize himself can maintain his knowledge in the absence of feedback from authority or reality. No theoretical explanation is available for this observation. The test-trials were not 'extinction' trials in the usual sense, because the absence of reinforcement was in no way disconfirming.

Reports by S*s*. So far, our data seem to indicate that a good many *S*s mastered the concept by adding relevant cues with adequate weights. The introspective reports by successful *S*s as to how they performed the task, however, indicate that this was not necessarily the case.

Following the test-trials, we asked *S* to write down the principle he had used. Successful learners generally gave clear accounts of their principles. These could be classified into two major types. An example of the first type is the report by S_{13} who had given perfect responses in test-trials. With the aid of diagrams, he wrote approximately as follows:

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[11] *Ibid.*, 208 ff.

> If both the circle and cross appeared in the same column (*i.e.* if $x' = x''$), the column was the answer. If the cross and circle appeared next to each other, the column in which the circle appeared was the answer. If they were two columns apart, the answer was the column between the two. If they were three columns apart, then the column next to the column in which the circle lay was the answer.

Capitalizing on the limited number of parameter values and response alternatives, S_{13} partitioned the stimuli into four types according to the horizontal separation of circle and cross, and found a solution for each type. This type of solution was used by 4 of 7 successful *S*s. The second approach is exemplified by S_7:

> I put a dot marking the middle between the cross and the circle. If the dot fell in line (vertically, with the standard sheet cross and circle, then that was the correct answer. If the dot didn't line up *exactly*, then the side which the circle was on was the correct answer.

This second type of solution was employed by 3 of 7 successful *S*s. They start by finding the midpoint between the circle and the cross. This in effect forms an unweighted composite of x' and x'', and gives a spatial representation to k. Finding that this is not always the correct k value, they add a codicil to take care of the exceptions. It should be noted that all these *S*s who judged k successfully had not attained the rule employed by *E*. This is always possible with a scalar concept, since a given body of data can be fitted in a number of ways.

DISCUSSION

Smedslund has said, "The functional dependence of response on each of the single cues represents a clear-cut operational measure of the structure of the *S*'s perceptual field during any given period of time."[12] This view must now be discarded. Cue-response correlations are not a sufficient indication of *S*'s solution.[13]

Summers and Uhl, as well as Smedslund, followed Brunswik's conception that a cue has a certain perceived relevance represented by its correlation with the phenomenon of interest; the cue-response correlation is defined for the universe of stimuli. Non-Brunswikian concept-formation experiments also examine a summary measure of *S*'s performance over all the stimuli in the experiment, as if there were a single thing to be learned.

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[12] Smedslund, *op. cit.*, 22.

[13] A personal communication indicates that Smedslund now agrees.

We observe, however, that *S does not solve a single problem* even though *E* regards the universe as an undifferentiated whole. *S* isolates sub-universes and finds a suitable response for one sub-problem separate from another. We would not disagree with the suggestion that learning be viewed as the establishment of cue-response correlations, but such correlations can describe *S*'s solution only if they are calculated separately for the sub-universes for which *S* has developed separate rules.

Concept formation with stimuli such as ours seems to consist of two processes that move forward simultaneously: isolation of sub-universes and identification of a rule applying within the sub-universe. At a later stage, perhaps *S* does merge the sub-universes and apply a single parsimonious rule.

It must be admitted that in our experiment in which the number of values of k was limited, the sub-dividing strategy was more effective than it would have been under other conditions. If the stimulus-universe were more varied (*e.g.* if x' and x'' took on values 0.6, 0.7, 0.8, . . . 1.9) solution by sub-division would be more difficult.

We suspect that man is better able to use information on frequency than information on metrical separation. The *Ss* of Azuma and Ochiai identified a relevant cue much better when the correct answer exactly matched a certain cue-value on 72% of the trials and was greatly displaced from it on the remaining trials, than in the condition where the cue-value was always close to the correct answer but exactly the same on only half the trials.[14] The results of Peterson and Miller also suggest that counts are more readily used than measurements.[15] If so, it follows that scalar concepts may be more readily attained by identifying small sub-universes, to each of which some rule applies perfectly, than by starting with a general rule that applies to all stimuli but gives response-values that are only approximately correct.

We conclude that a more powerful method of analysis than the calculation of cue-response correlations is required to describe the operations *S* uses at any point in training. Over-all cue-response correlations calculated from training trials are at best a first approximation to the process by which a concept is attained.

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[14] Hiroshi Azuma and Takako Ochiai, The identification of correlational cues and probabilistic cues (abstract, in Japanese), in *Proceedings of 1964 Annual Convention of the Japanese Psychological Association*, 1964, 120-121.

[15] Cameron Peterson and Alan Miller, Mode, median and mean as optimal strategies, *J. exp. Psychol.*, 68, 1964, 363-367.

A STUDY OF DIFFERENCES BETWEEN PERFORMANCE AND PSYCHOPHYSICAL JUDGMENTS

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Perhaps no other kinds of judgments are made so universally and frequently—for economic, social, political, or scientific reasons—than judgments of the performance of others; yet there is very little systematic knowledge about how these judgments are affected by numerous variables known or suspected to be operant in situations in which judgments are made.

The literature on psychophysical judgment suggests that even in the simplest situation the evaluations made by different persons judging the same phenomena are significantly influenced by such variables as the range of stimulus-values presented, the presence or absence of anchor-stimuli, prior exposure to similar or dissimilar stimuli, the time-interval between presentation of the stimuli and the recording of the judgment, and so on.[1] One can only speculate about the effects of the much greater number of pertinent variables in a more complex judgmental situation, such as the assessment of an individual's performance of a technical work-task.

In few areas of human behavior is so much reliance placed on phenomena that are so poorly understood as are judgments or 'ratings' of performance. Ratings, whether formal or informal, are generally accepted as *criteria* of performance, from both necessity and convenience. There also seems to be a sort of intuitive satisfaction with the validity of performance-ratings in spite of the many studies showing that they do not correlate substantially with more objective measures of performance or, in many cases, with predictors to which, on logical grounds, they should be related.[2]

The plain fact is that the validity of performance-ratings cannot be determined unless there are objective measures of the same performance with which they can

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[1] J. P. Guilford, *Psychometric Methods*, 2nd ed., 1954, 302-340.

[2] D. N. Buckner, The predictability of ratings as a function of interrater agreement, *J. appl. Psychol.*, 43, 1959, 60-64.

be compared. This need for objective criteria of performance can be underscored by comparing judgments in a psychophysical study with those in a rating-study. In a psychophysical study, *Ss* judge stimuli on psychological dimensions whose *physical values are known*. If one stimulus is judged to have more of a particular attribute than another, the validity of the judgment can be determined directly with reference to the physical, objective values of the stimuli. In a rating-study, however when one performance is judged to be 'better' in some respect than another, there is no way of telling, without objective criteria, whether the performances were actually different and the rater was able to discriminate this difference, or whether in fact the performances were equal or differed in the opposite direction and the judged difference was determined by extraneous factors. In the psychophysical situation, statements can be made about discriminative thresholds in terms of the physical values of stimuli. No such statements are possible in rating-studies, in spite of the fact that a fundamental assumption in the use of ratings is that raters *can* discriminate along given dimensions, such as the 'goodness' of performance. It is obvious that unless there are objective criteria of performance available, this fundamental assumption cannot be tested.

The need for objective criteria suggests that studies of rating-behavior should be conducted under laboratory conditions. Only the control attainable under such conditions will make it possible to observe accurately the objective values of performance, as well as to control the presence and magnitude of variables that influence judgments of performance. With such control, it will be possible to investigate functional realtionships between values of stimulus and of response. The accurate determination of the S–R relationships, rather than the R–R relationships observed in the usual field-studies, is essential if the influences of many pertinent variables are to be understood. Once the relationships have been established under laboratory conditions, the study of rating-behavior can be extended to more practical situations in the field. This extension must be made, though, on the firm ground of rigorously tested and verified hypotheses that have taken account of the influence of numerous important variables, and their interactions, upon judgments of performance.

Exploratory experiment. An initial investigation was made of a method by which behavior during the performance of judgments could be observed under laboratory-like conditions.[3]

The *Ss* (raters) in this study judged the *same* stimuli, under two conditions: an 'objective' condition, in which they were told the stimulus was simply a flashing light and was to be judged on a physical scale; and a 'performance'-condition, in which they were told the flashing light represented performance on a pursuit-rotor and was to be judged on a psychological scale. Similar nine-category judgmental scales were used for each condition. Under the objective condition, the categorical names were in terms of the *duration* of the flashing light (*e.g.* 'very short,' 'long'); under the performance condition, they were in terms of the *goodness of performance* (*e.g.* 'very poor,' 'good').

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[3] William Harris and D. N. Buckner, A methodological study of the judgment of human performance. Paper presented at the Western Psychological Association Meeting, San Diego, 1959.

The purpose of using this method was to try to observe what might be called a 'pure' effect of judgmental performance. If it were assumed that the 'objective' judgments reflected only the perceptual ability of the *Ss*, and that this was relatively constant or varied in a determinable way, then any discrepancy between these and the judgments of performance could be attributed to the altered *meaning* of the stimuli presented under the different conditions.

This exploratory study was conducted in the laboratory of Human Factors Research, Inc., with clerical personnel serving as *Ss*. The first group of *Ss* judged the same series of stimuli on two successive occasions under objective conditions; a second group judged the series first under objective conditions and then under 'performance'-conditions. For the 'performance'-condition, the *Ss* were led to believe that the professional members of the staff were performing on the pursuit-rotor in an adjacent room and that the 'goodness' of a particular performance was indicated by the proportion of time the stimulus-light was on during a 30-sec. period of judgment. No one was actually performing the task.

Results. The following results were obtained: (1) On the second objective session for the first group, the mean judgments 'regressed toward the mean'; that is, the slope of the judgments-on-stimuli regression became *flatter* as compared to that of the first objective session. (2) On the 'performance'-session for the second group, the mean judgments went in an opposite direction; that is, the slope of the regression became *steeper* as compared to that of the previous objective session. An analysis of variance, using the slope of individual regressions as the dependent variable, showed a significant group-by-sessions interaction.

When the stimuli represented performance of other persons, the value of poor 'performance' was generally *underestimated* and that of good 'performance' generally *overestimated*. By contrast, when the stimuli were simply durations of 'light on,' low stimulus-values were judged to be *higher*, and high ones *lower*.

Main experiment. The two groups in the main experiment were given differential training in performance on the pursuit-rotor, before making additional judgments of the same stimuli under the 'performance'-condition. The purpose of giving the *Ss* training in performing the task was to determine the effect of their own task-proficiency upon their judgments of others' performances. It was found that *Ss* with brief experience on the task, a single 30-sec. period, systematically overestimated all stimulus-values, as compared to their pre-training judgments. Because the performances of the *Ss* with brief experience were very poor, this was interpreted as a 'contrast'-effect. Judgments of *S*s with greater experience, five 30-sec. periods, were not appreciably different from their pre-training judgments.

The following hypotheses, based on the results of the methodological study, were evaluated in the study reported here: (1) Judgments of the same stimuli will have greater variance when they are made under the 'performance'-condition than when made under the objective condition. (2) *S*s with brief experience in performing the task will overestimate the value of stimuli representing performance; judgments of *S*s with greater experience will not be affected.

In addition, two other hypotheses were tested: (3) Judgments of stimuli representing performance will be less valid when performers are present during a judgmental session than when they are not; and (4) the judgments will be influenced by the *S*s' knowledge of the performers' school achievement.

Method: (1) Subjects. The *S*s were 29 students from a single class undergoing training at the U. S. Fleet Anti-Submarine Warfare (ASW) School, San Diego, California. Five men, designated as *performers,* were selected from the class on the basis of class standing; two from the upper 25%, two from the lower 25%, and one near the mean of the class. The class standing of all the students was public knowledge, as it was posted weekly for each class in the school. The remaining 24 men, designated as *S*s (raters), were randomly assigned to either Group I or Group II, 12 in each.

Procedure. Three adjoining classrooms at the ASW School were used for the study. The apparatus controlling the stimuli was in the center room; and identical displays of stimuli were placed in the other two rooms, to permit simultaneous presentation of the stimuli to two groups of *S*s.

This apparatus consisted of an arrangement of five microswitches and cams driven by a constant-speed motor, and a multiple switch for selecting stimuli to be displayed. The cams were so cut that the microswitches were 'on' or 'off' for varying times non-systematically during a period of one 30-sec. revolution. Each of the cams controlled a different stimulus-value, either 9, 12, 15, 18, or 21 sec. 'on' during the period of revolution.

The stimulus-displays were 'black boxes' with a white and a green light mounted in each. The white light signaled the duration of the stimulus-period, and the green light provided the stimulus to be judged. Table I summarizes the experimental conditions for each of the groups.

Stimuli. Both groups judged the same series of stimuli in each of the four judgmental sessions. A series consisted of 25 stimuli, exposed in haphazard order five times each at 9, 12, 15, 18, and 21 sec. A different order of stimuli was presented in each session. The stimuli were randomly assigned to a series in blocks of 10, 10, and 5, with the restriction that no adjacent series was the same. Thus, each stimulus occurred twice in the first series of 10 stimuli, twice in the second 10, and once in the last 5.

Each of the stimuli was presented for 30 sec. The white light on the display came on at the beginning of the judgmental period and remained on until the end. The green light flashed on and off intermittently, as determined by the particular cam selected, during the period. As the judgmental period was constant, the green light was on either 30%, 40%, 50%, 60%, or 70% of the time during the 30-sec. period. The time between the judgmental periods was about 20 sec., hence, the total time for a judgment session of 25 stimuli was about 20 min.

The *Ss* judged each stimulus immediately after the judgmental period, using a 9-category scale. Twenty-five scales, corresponding to the 25 stimuli to be judged, were printed on a single sheet of paper. The *Ss* were instructed not to change any judgment after another stimulus was presented.

TABLE I

EXPERIMENTAL PROCEDURE

Session	Judgmental condition	
	Group I ($N=12$)	Group II ($N=12$)
1	objective	objective
2	objective	performance
3	performance	performance*
Pursuit-rotor training	1 30-sec. period	5 30-sec. periods
4	performance	performance

* 'Performers' were present during this series.

Objective conditions. Under the objective condition, the *Ss* were instructed to judge the *cumulative duration* of the time the green light was on during each judgmental period. The judgmental scale used for this condition is as follows:

Extremely short	Very, very short	Very short	Short	Medium	Long	Very long	Very, very long	Extremely long

'Performance'-conditions. Under the conditions of performance, the *Ss* were instructed to judge the goodness of performance. Just before the first session of judgmental performance for each group (Session 2 for Group II and Session 3 for Group I), the pursuit-rotor task was demonstrated. The green light on the display was wired in a circuit with the stylus and the target on the rotor, and the *Ss* were shown how the light went on and off as contact was made and broken. *E* then performed the task for 30 sec., keeping the stylus in contact with the target about 50% of the time. The *Ss* were then told that other men would be performing the task in another room, and that they were to judge how well they did during the subsequent session. The judgmental scale used for this condition is reproduced below. Note that it is just like the one for the objective condition except that the adjectives 'poor' and 'good' have been substituted for 'short' and 'long.'

Extremely poor	Very, very poor	Very poor	Poor	Average	Good	Very good	Very, very good	Extremely good

Experimental sessions. During Session 1, each of the five men selected as performers was given five 30-sec. training trials on the pursuit-rotor. Altohugh none of the *S*s was actually to judge the performances of these men, there were two reasons for the training. One concerned the lack of control over communication between the *S*s in different groups. It was necessary to have some of the men actually performing the task to preserve the illusion that performance was being judged some of the time. Another was that the performers were ostensibly to be observed at the task during one of the judgmental sessions (described below), and it was necessary that they be practiced so the session would run smoothly.

During Session 3, the five performers were in the same room and actually performing the task while the *S*s in Group II were judging the stimulus-values. They were led to believe that they were judging the performers, and the performers thought that they were being judged. When the performers were first brought into the room, the green light was connected to the pursuit-rotor and shown to both the *S*s and the performers. The pursuit-rotor was so placed behind the stimulus-display that the *S*s could not observe the actual contact of stylus and target but could observe the performer. The performers, either while performing or while waiting to perform, could not observe the stimulus-display. Each performer performed five times during the session, and each performance coincided with the 30-sec. judgmental period for each stimulus. The actual stimuli judged were, of course, the same 9-, 12-, 15-, 18-, and 21-sec. stimuli that had been judged before, although in a different order. The performers were systematically assigned to each of the stimulus-values; that is, each of the performers was associated with each of the stimuli one time only. The performers appeared in a more-or-less random order, consistent with the restriction that they be associated once only with each stimulus.

The situation, then, for Group II in Session 3 was that the performers were identified with the stimuli being judged. The objective values, however, of their 'performances' were equal; because each performer was identified once with each stimulus, the *mean* 'time on target' for each was 15 sec., or 50% of the time.

Results. Two general analyses of the data were performed: one using the responses to all stimuli in each judgmental session, and the other using only the responses to the last five stimuli in each session. The second analysis was based on the assumption that judgments of stimuli occurring later in a series would be more stable than earlier ones and would more accurately reflect the influence of the experimental conditions. The random-block assignment of stimuli to a series of 25, such that each stimulus-value occurred once and only once in the last five, permitted this analysis.

The numerical judgmental values used in all analyses were determined by assigning the numbers 1 through 9 to the nine categories on the judgmental scales. No. 1 corresponded to the 'extremely short' and 'extremely poor,' No. 5 to the 'medium' and 'average,' and No. 9 to the 'extremely long' and 'extremely good' categories. The value of a particular judgment was indicated by the number of the category checked.

Hypothesis 1. Hypothesis 1 stated that judgments would be more variable under the performance-condition than under the objective condition.

This hypothesis was tested by comparing the slopes of the regression-lines of the judgment-on-stimuli under the two conditions. Fig. 1 shows plots of the mean judgmental values for all judgments for Groups I and II on Sessions 1 and 2. Each point on the curves represents the mean of 60 observations, five judgments by each of the 12 *S*s.

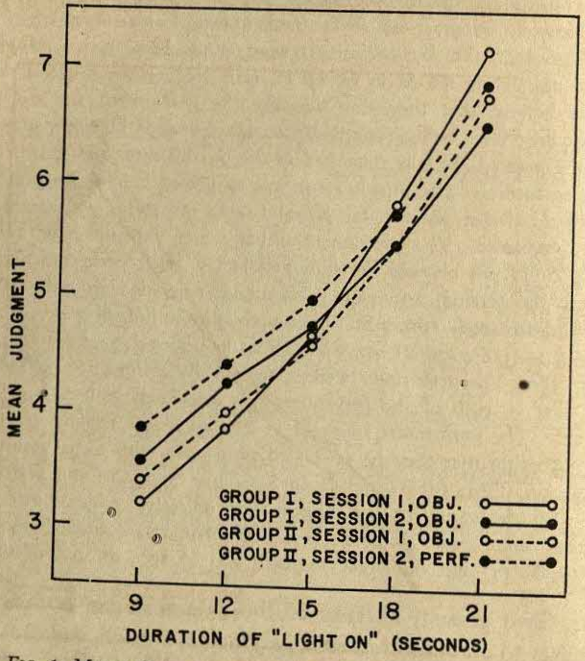

FIG. 1. MEAN OBJECTIVE AND 'PERFORMANCE'-JUDGMENTS AS A FUNCTION OF STIMULUS-DURATION FOR GROUPS I AND II

It can be seen that the judgments of Group I 'regress toward the mean,' that the slope of the regression is flatter on the second objective session than on the first. This was in line with the results of the exploratory study. The judgments of Group II, however, were not in line with either the results of the exploratory study or the prediction made. The mean judgments for all stimuli were systematically higher under the performance-condition than under the objective condition, but the slope of the regression was not steeper, as was predicted. Hypothesis 1 was not confirmed from the analysis of all judgments, although there was less regression toward the mean for Group II than for Group I on the second judgment session.

The performance-instructions had the predicted effect upon the judgments of the *S*s of Group I. The slope of the regression-line was steeper in Session 3, their first performance-judgment session, than in Session 2, their second objective session (see Fig. 2).

When only the judgments to the last occurrence of each stimulus were considered, Hypothesis 1 was confirmed. Fig. 3 shows the mean judgment-values of these stimuli on Sessions 1 and 2 for the two groups. Each point is the mean of 12 observations, one judgment for each stimulus by each of the *Ss* in the groups. A statistical test of the hypothesis was made by computing least-squares regression-lines and using the slopes of these lines as the dependent variable in analysis of variance. The evaluation of the Group × Sessions interaction-term provided a test of the hypothesis. This interaction was significant at the 0.025 level.

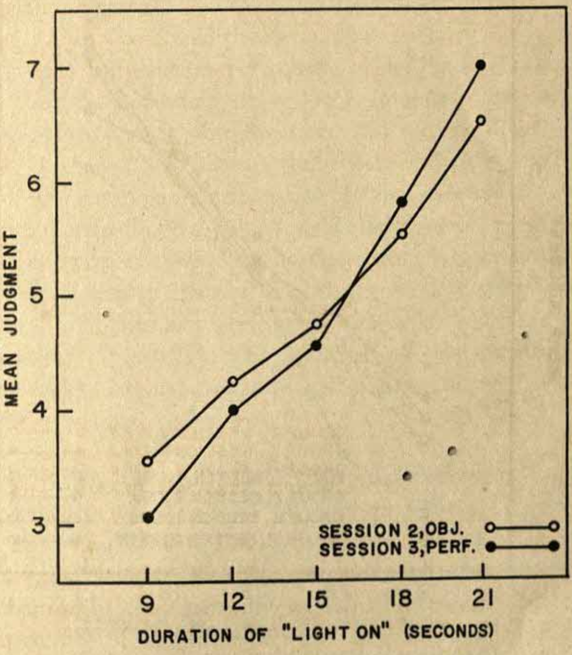

FIG. 2. MEAN OBJECTIVE AND 'PERFORMANCE'-JUDGMENTS AS A FUNCTION OF STIMULUS-DURATION FOR GROUP I

The significant Groups × Sessions interaction occurred because the mean values of the slopes of the regression-lines for the two groups were in opposite directions on Session 2 as compared to their value in Session

1, in which the regression-line of Group I was flatter, indicating less discrimination among the stimuli; and the line of Group II was steeper, indicating more discrimination.

Hypothesis 2. Hypothesis 2 stated that the amount of experience the *S*s had in performing the task would influence their judgments of performance. It had been observed in the exploratory study that the *S*s with little experience at the task systematically overestimated all stimulus-values as compared with the *S*s who had previous experience. An analysis of all judgments showed that this 'contrast'-effect did not occur for the *S*s in this study. The mean judgments of both groups were essentially the same before and after experience on the pursuit-rotor (*i.e.* Sessions 3 and 4, respectively).

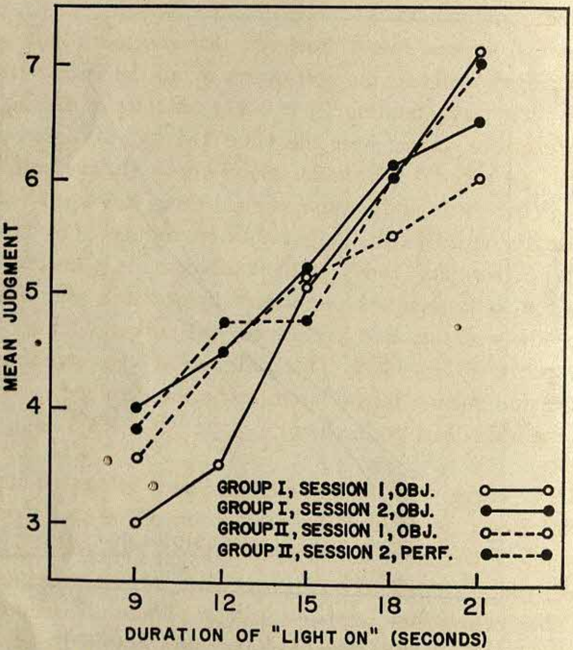

FIG. 3. MEAN OBJECTIVE AND 'PERFORMANCE'-JUDGMENTS AS A FUNCTION OF STIMULUS-DURATION FOR GROUPS I AND II
(Last of 25 judgments)

There was some evidence for an experience effect from the analysis of judgments to the last five stimuli. The Group I *S*s, who had had one 30-sec. trial on the rotor, judged the first four stimuli to be higher after training than before. Even here, though, the 'contrast'-effect was not as marked as it was in the exploratory study, where it was statistically significant with only a small group of *S*s.

Hypothesis 3. Hypothesis 3 stated that performance-judgments would be less valid when performers were present during a judgmental session and that judgments would be influenced by the *S*s' knowledge of the performers' school-achievement. The first part of this hypothesis was not confirmed in either analysis: the performers' presence had no significant effect upon the mean judgments.

Hypothesis 4. It was found, however, that the mean judgments (performance-ratings) assigned the performers by all the *S*s correlated significantly with their class standing (P = 0.82), in spite of the fact that their actual 'performance'-values were the same (15 sec. of 'light on' or 50% of the time 'on target'). Although there were only five performers and the range of the mean judgments assigned them was small (from 4.9 to 5.3), it was apparent that the judgments were influenced by the *S*s' knowledge of the performers' ability in the classroom. It is interesting to note that when the *S*s *ranked* the performers immediately after Session 3 according to how well they had 'performed,' the rankings did not correspond to class standing (P = 0.29). The influence of class standing upon the performance judgments did not seem to be the result of a conscious appraisal of the individual performers.

Discussion. There were both similarities and differences in the results of the exploratory study and the present one. The same 'performance-judgment' effect was observed in both studies: the tendency to perceive or, at least, judge stimuli to be farther apart when they represented performance than when they were just stimuli. There is evidence, however, that the occurrence of this effect depends upon how well individual variables are controlled. It was observed, for example, only in the analysis of the more stable judgments of the *S*s of Group II, whereas it was observed in the analysis of all judgments of the *S*s of Group I.

The 'regression toward the mean' of the judgments that was observed on the second objective session is interesting in itself. This result is contrary to that usually observed in a psychophysical situation: Judgments of the same stimuli on successive occasions tend to become more discriminating.[4] In this instance, though, the regressing of the judgments seemed to reflect the facts of the situation. The stimuli in the series were not far apart from one another (3-sec. intervals) and were in the middle range of a finite time-period (from 9 to 21 sec. of a 30-sec. period). If the raters considered the judgmental scale to encompass the total time-period, as apparently they did, their judgments on the second objective session probably represented a more accurate appraisal of the stimulus-values.

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[4] Guilford, *op. cit.*, 302ff.

The 'contrast'-effect, the overestimation of stimulus-values as a function of brief experience on the task, which was striking in the exploratory study, was only partially confirmed in the analysis of more stable judgments in the present study. There were important differences in the *Ss* and conditions of the two studies which were probably responsible for the different results.

(1) In the exploratory study, the *Ss* were clerical personnel, and the men they thought were performing were professional personnel—a subordinate-superior relationship. After brief training on the task, these *Ss* evidently perceived all stimuli as representing better performances than their own. Possibly because there were no constraints imposed by their relationship to the performers, they judged the stimuli in line with their perceptions. In the present study, on the other hand, the presumed performers were classmates of the *Ss*—a peer relationship. The *Ss* from the Navy were evidently reluctant to rate the performances of their peers very highly, even though they also may have perceived the post-training 'performances' to be better than their own.

(2) The Naval *Ss* did not use the extreme categories on the judgmental scales, under any condition, as frequently as did the clerical *Ss*. In the present study, the means of all the judgments for the lowest and highest stimuli were 3.7 and 6.7, respectively. In the exploratory study, the corresponding means were 3.1 and 7.9. The failure of the Naval *Ss* to use the extreme categories tended to reduce the magnitude of effects of both the 'contrast-' and 'performance-judgments,' observed in the exploratory study. This was particularly evident in the judgments of the higher stimulus-values: Their greater overestimation by the clerical *Ss* accentuated both effects.

The results of these studies emphasize the necessity for studying the influence of 'rater-performer' interactions and individual 'rater-differences' upon judgments of performance. For example, the differential results of brief experience on the task observed in the two studies seemed to be primarily a function of the different attitudes of the *Ss* toward the 'performers' being judged.

If individual variables are to be studied, it seems necessary to have a more demanding situations of judgments of performance. A major assumption of these studies is that the likelihood of irrelevant variables, such as 'rater-attitudes,' influencing 'performance' judgments is directly related to the difficulty of the task of judgment. In this case, the judgmental task was not very difficult, primarily because of limitations imposed by the performance-task. Only a single dimension was being judged, the cumulative duration of 'light on' or 'time on target.' Only a few different stimuli could be used, because of the restriction in the range of possible stimuli

(0 to 30 sec.) and the necessity for individual stimuli to be discriminable from one another under the objective condition. These considerations indicated that a more demanding performance-task, and consequently a more demanding judgmental task, be developed for future studies.

In these studies, the *Ss*' *experience* and *proficiency* in performing the task were confounded. The *Ss* with brief experience on the task were the poorer performers, simply because they had had little opportunity to practice. On the other hand, *Ss* with greater experience differed in their ability to perform the task. It seemed that the effects of experience observed could have been due either to differences in experience or in proficiency at the task and that in future studies one should be held constant while the other is varied.

SUMMARY

The *Ss* judged the cumulative duration of 'time on' of a flashing light under different instructions. Under one condition, the 'objective' condition, the *Ss* judged five durations—9, 12, 15, 18, and 21 sec. with 'light on' during a 30-sec. period—on a 9-category scale. Under the other condition, the 'performance'-condition, the *same* stimuli were judged, but the *Ss* were led to believe that the 'light on' indicated the time a man operating a pursuit-rotor in another room was 'on target'; and they rated his performance on the same scale but with different names appropriate to performance.

The following hypotheses were tested: (1) Judgments of the same stimuli will have greater variance when made under the 'performance'-condition than when made under the objective condition; (2) *Ss* with brief experience in performing the pursuit-rotor task will overestimate the value of stimuli representing performance, but judgments of *Ss* with greater experience will not be affected; (3) judgments of stimuli representing performance will be less valid when performers are present during a judgmental session than when they are absent; and (4) judgments will be influenced by the *Ss*' knowledge of the performers' school-achievement. Hypothesis 1 was confirmed in an analysis of 'stable' judgments—judgments of the last 5 stimuli in a series of 25, but not in an analysis of all judgments. Hypothesis 2 was partially confirmed in an analysis of stable judgments: All but the highest stimulus-value were overestimated under the performance-condition. The presence of the performers had no effect on the judgments, but the judgments were correlated with school-achievement of the performers.

It was concluded that a more demanding judgmental task was indicated for further study of this problem.

MULTIDIMENSIONAL ANALYSIS OF VISUAL FORM: AN ANALYSIS OF INDIVIDUAL DIFFERENCES

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and SAMUEL MESSICK, Educational Testing Service

In a recent review of suggested metrics of visual form, Michaels and Zusne note that well over a dozen such metrics have been proposed in the past several years.[1] Such measures have included, among others, informational content,[2] angle of rotation,[3] number of sides, number of angles, and complexity.[4] This multiplicity of metrics, by its very existence, suggests that a multidimensional approach to form perception might be fruitful. Other than Attneave's regression analysis and Stilson's multiple factor design, there has, however, been no attempt to approach the problem using multidimensional methods.[5]

Attneave found that approximately 90% of the variability in judgments of complexity could be accounted for by three variables: number of independent turns in the contour, symmetry of the figure, and the mean of the algebraic difference in degrees between successive turns in the contour.[6] Ratings of complexity do not, however, provide a metric which meets the normal requirements for such a measure. Stilson did propose a multidimensional analysis, but did not provide empirical data against which his design could be validated.[7]

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[1] K. M. Michels and Leonard Zusne, Metrics of visual form. *Psychol. Bull.*, 63, 1965, 74-86.

[2] Fred Attneave and M. D. Arnoult, The quantitative study of shape and pattern perception, *ibid.*, 53, 1956, 452-471.

[3] M. D. Arnoult, Measures of shape discrimination performance in a study of the effect of angular orientation of the stimulus, *United States Air Force Human Resources Center Research Note*, 51-10, 1951; Shape discrimination as a function of the angular orientation of the stimuli, *J. exp. Psychol.*, 47, 1954, 323-328.

[4] Fred Attneave, Physical determinants of the judged complexity of shapes, *ibid.*, 53, 1957, 221-227.

[5] Attneave, Physical determinants of the judged complexity of shapes, *J. exp. Psychol.*, 53, 1957, 221-227; D. W. Stilson, A multidimensional psychological method for investigating visual form, in J. W. Wulfeck and J. H. Taylor (eds), *Form Discrimination as Related to Military Problems*, 1957, 54-64.

[6] Attneave, Physical determinants of the judged complexity of shapes, *J. exp. Psychol.*, 53, 1957, 221-227.

[7] Stilson, A multidimensional psychological method for investigating visual form, in J. W. Wulfeck and J. H. Taylor (eds.), *Form Discrimination as Related to Military Problems*, 1957, 54-64.

The primary goal of the present study was to identify the principal stimulus-dimensions utilized in the observation of geometrical figures. A secondary goal was to determine if there exists distinct sub-populations that make use of essentially different dimensions in their observations.

For example, Helm and Tucker found that normal and color-blind *S*s exhibit quite distinct points of view when judging similarities among chromatic stimuli.[8] Tucker and Messick provided a method of uncovering consistent individual differences and of determining separate multidimensional perceptual spaces for each point of view.[9] Individuals having similar patterns of scores in dimensions can also be assigned to subgroups, the members of which essentially perceive the stimuli in identical fashion. Using the Tucker and Messick procedures, Silver, Jones, and Landis obtained similarity judgments of simulated air traffic control problems from 51 FAA *O*s.[10] They found four distinct subgroups that roughly corresponded to the proficiency and training classifications given the observers by their superiors. The dimensions utilized by each subgroup were quite distinguishable from those obtained from each of the other subgroups. In light of this finding that *O*s use different continua when judging visual forms, it seems reasonable to suppose that the procedure of averaging judgments across *O*s will act to distort the underlying 'real' dimensions. The present research provides data on this question.

METHOD AND PROCEDURE

Subjects. The *O*s were 50 women students at the Moore College of Art, Philadelphia (Mdn. age = 20 yr.). The *O*s were paid for their services.

Stimuli. Stimuli were obtained from three sources. Three of the figures (an equilateral triangle, a square, and a septagon) were systematically drawn to provide 'marker' stimuli. Twenty-four stimuli were taken from a previous study.[11] Three additional stimuli were generated by a random method.[12] Six sample stimuli are shown in Fig. 1. Figures were photographically enlarged (or reduced) to just fit within a 2-in. circle. This was an attempt to equate the figures roughly for area, since it was believed that area would otherwise emerge as a variable whose contribution to the total variance would be so large as to obscure less conspicuous factors. Stimuli were printed photographically on a glossy white background and were cemented in pairs on a 5 × 8-in. file card. The 30 forms thus yielded 435 pairs of stimuli.

Instructions. The *O*s received the following instructions:[13]

> Here is a stack of cards. On each card you will see that there are two patterns. There are 30 patterns in all; this pile of cards contains all of the possible pairs of

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[8] C. E. Helm and L. R. Tucker, Individual differences in the structure of color-perception, this JOURNAL, 75, 1962, 437-444.

[9] L. R. Tucker and Samuel J. Messick, An individual differences model for multidimensional scaling, *Psychometrika,* 28, 1963, 333-367.

[10] C. A. Silver, J. M. Jones, and Daniel Landis, *Characteristics of Radar Operators,* 1965, 1-78.

[11] M. N. Crook, F. E. Gray, J. A. Hanson, and Alexander Weisz, *The Effect of Noise on the Perception of Forms in Electro-Visual Display Systems,* 1959, 1-15.

[12] Attneave and Arnoult, *op. cit., Psychol. Bull.,* 53, 1956, 452-471.

[13] Instructions modified from those used by S. J. Messick, An empirical evaluation of multidimension successive intervals, *Psychometrika,* 21, 1956, 370.

those 30 patterns. Your task is to judge how similar the patterns on each card are. On some cards the patterns may seem very similar; on the other cards the patterns may appear much less similar. There are no right or wrong answers. To begin with, I would like you to separate these cards into two piles. In one pile put all of the cards on which you judge that the two patterns are quite similar to each other. In the other pile, put all of the cards in which the patterns of each pair are less similar. It is not necessary that you have exactly the same number of cards in each pile. Do you understand what you are to do?

[If there were no questions, *O* was instructed to begin, while *E* observed his performance.] When the pile had been divided into two subpiles, the following instructions were given:

Your next task is to subdivide patterns of each of these two piles again. In this pile you have all of the cards having pairs that you felt were more similar and in the other pile were all of the cards in which the patterns of each pair were less similar. Now divide this pile into two piles just as you did before. Put all of the cards on which the pairs of patterns are more similar here and put all of the cards on which the pairs of patterns are less similar there. Then do the same with the other pile. You will then have four piles of cards in which the cards with pairs of patterns that are most similar will be over here and the ones that are least similar will be over there. Do you understand what you are to do?

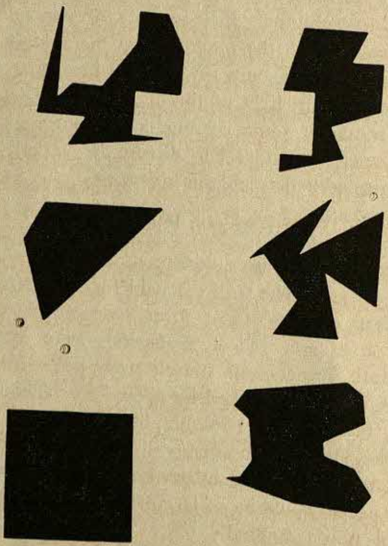

FIG. 1. SAMPLE STIMULUS-FIGURES

[*E* again observed *O*'s performance to insure that the instructions were being followed.] Similar instructions were given after the completion of the division into 4 piles. The *O*s were instructed to divide the 4 piles into 8, and finally into 16 piles. When they had completed the division into 16 piles, they were given the following instructions:

You have now divided the stack of cards into 16 piles ranging from the most similar which are here to the least similar which are over there. Now look through each pile of cards to make sure that every card in a pile is in its correct place. That is, make sure that every card in a given pile contains a pair of patterns which are more similar than any of the patterns in any pair which follows it in the pile, and also that they are less similar than any of the patterns in any pair which precedes them in the pile. Do you understand what you are to do? You are to sort through the piles to make sure that every card is in its correct pile.

When *O*s had completed this portion of the experiment, *E* recorded on a special form the number of each card that was assigned to each pile. These records became the fundamental data of the experiment.

Procedure. *O*s were run singly with *E* present during the entire session. Two groups of 25 *O*s each were used. The *O*s were randomly assigned to Groups I and II. The combined group of 50 *O*s is labeled Group III.

Analysis of data. Groups I and II (N = 25 in each case) and the combined Group III of 50 *O*s were analyzed separately following the procedures outlined by Tucker and Messick.[14] This analysis resulted in a matrix of order N X N (I, II = 25 × 25; III = 50 × 50) which is factored by the method of principal components. Each rotated factor represents a dimension of viewpoint about perceived stimulus-similarity. For each viewpoint, estimates of psychological distances between the stimuli were computed following the Tucker and Messick procedure. This resulted in a 30 × 30 matrix of inter-stimulus distances for each viewpoint. These matrices were then analyzed by standard multidimensional scaling procedures to obtain the dimensions of perceived stimulus-variation represented by each.[15] These dimensions of perceived stimulus-variation represent the psychological dimensions used by the *O*s in making their judgments. It is next of interest to ascertain the relation of these psychological dimensions to various physical dimensions of stimulus-variation.

RESULTS

The characteristic roots of the three N X N matrices are presented in Fig. 2. Each root represents the amount of variance accounted for by a given factor. Factors retained for consideration are those with relatively large roots and which precede a noticeable break in the distribution of size of roots. 'Noise' roots are those which are relatively small and for which the successive differences in roots have stabilized at some small value. The 'noise' roots are those that distribute along the straight lines in Fig. 2; the 'real' roots are those that fall above these lines.

Fig. 2 shows that Group I contains at least four, and possibly five, dimensions, whereas Group II appears to have five dimensions. Group III also appears quite clearly to contain five dimensions. Inasmuch as Group

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[14] Tucker and Messick, An individual differences model for multidimensional scaling, *Psychometrika,* 28, 1963, 333-367.

[15] Samuel Messick and R. P. Abelson, The additive constant problem in multidimensional scaling, *ibid.,* 21, 1956, 1-16.

III, which is a combination of Groups I and II, has only five roots, it is apparent that the same viewpoints are represented in both subgroups. If this were not so, the distinct viewpoints from the two subgroups would all appear in the combined group, resulting in more than five significant roots. The similarity between Groups I and II is further strengthened by noting that the correlation between the distance estimates of each group is greater than + 0.90[16]. For these reasons, Groups I and II were combined for subsequent analysis.

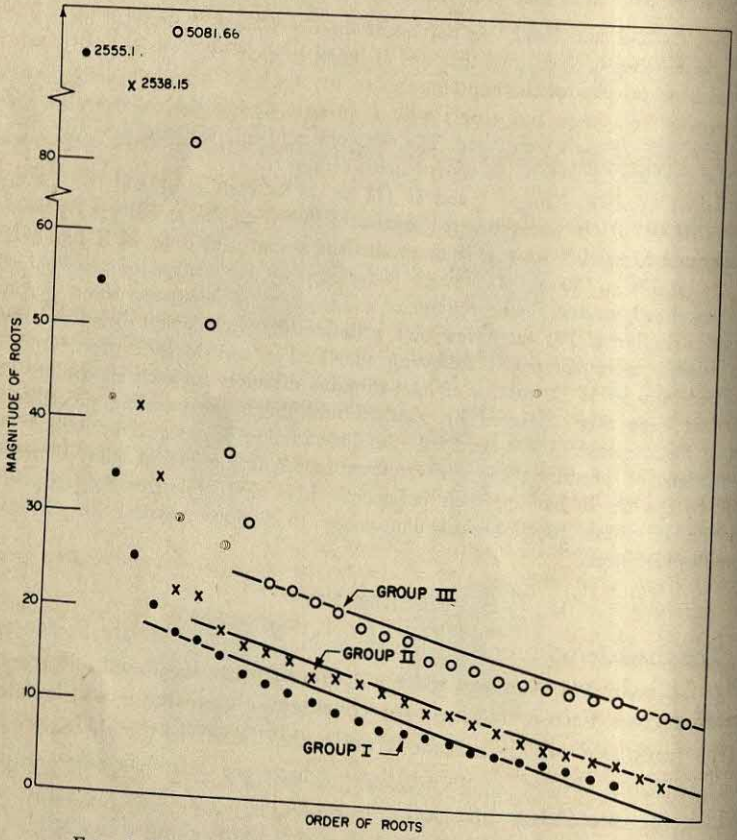

FIG. 2. CHARACTERISTIC ROOT-PLOTS FOR EACH GROUP

The matrix of inter-stimulus distances for each of the five points of view in Group III was analyzed by multidimensional scaling techniques, and the dimensions of the resulting perceived stimulus-spaces were rotated to the Equamax criterion of orthogonal simple structure.[17] Space does not permit the inclusion of the complete factor-plots; these data are, however, available from the authors. Viewpoint A has three dimensions; Viewpoint B has four; Viewpoint C has six; and Viewpoints D and E have five dimensions each. The number of significant dimensions (roots) was determined using the procedure described above. It will be remembered that individuals receive a score on each of the five viewpoint-dimensions. To obtain a rough estimate of the distribution of the viewpoints in the sample of *S*s, an individual was considered to have a particular viewpoint if his score on that viewpoint dimension was higher than his score on any of the other four viewpoint-dimensions. With this method of assignment, 14 *O*s scored highest on Viewpoint A, 7 on B, 14 on C, 10 on D, and 5 on E.

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[16] C. A. Silver and S. J. Messick, *Multidimensional Analysis of Visual Form*, 1963, 14.

Naming the dimensions. The 'Naming' of the dimensions was attempted by correlating the stimulus-rankings on each dimension with stimulus-rankings on various hypothesized criterion-scales.[18] First, a set of "hypotheses' was identified that were potentially related to the underlying dimensions of form-perception. Then, a numerical index was determined for each hypothesis to permit a ranking of the stimuli. The size of the rank-order correlation between the ranks on the hypothesized dimension and those obtained from the factor loadings were used as a 'test' of the adequacy of the hypothesis.

The hypothesis selected were as follows:[19]

(1) P^2/A, (perimeter squared divided by the area). This is primarily a measure of dispersion;

(2) *AV* (angular variability). *AV* is the mean change in degrees between successive angles of the figure;

(3) *C* (complexity) is computed from Attneave's regression equation:[20] $C = 5.46 \log_{10}T + 0.41S - 2.30$. It is however, primarily a measure of the number of sides of the figure. For the 30 stimuli used in the present research the rank-order correlation between *C* and number of sides was $+ 0.99$;

(4) *R* (rotation) was indexed by drawing a line through the figure the longest way possible. The deviation of this line from the vertical (in degrees) is the amount of rotation.

Rank-order correlation coefficients were used because of the skewness of the distribution of factor loadings. Due to the relative inefficiency of this statistic, higher significance levels ($\alpha = 0.01$) have been chosen.

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[17] D. R. Saunders, Integrating the implementation of quartimax, varimax, oblimax, and related rotational methods, *Phychol, Reports,* 10, 1962, 241.

[18] Silver, Landis, and Jones, *Measure of Information Transfer from Large Scale Displays,* 1965, 1-39; Silver, Jones and Landis, *Characteristics of Radar Operators,* 1965, 1-78.

[19] See Michels and Zusne, Metrics of visual form, *Psychol. Bull.,* 63, 1965, 74-86.

[20] Attneave, Physical determinants of the judged complexity of shapes, *J. exp. Psychol.,* 53, 1957, 221-227.

In the past, most multidimensional scaling studies have involved an analysis of the combined judgments for all *O*s. This amounts to an analysis of inter-stimulus distances derived from the unrotated first principal component of Group III. The amount of variances accounted for by this dimension is very large (90.17% of total) as may be seen from Fig. 2, and represents essentially the mean ratings. For purposes of comparison, interstimulus distances according to this composite or average viewpoint were also analyzed, and seven dimensions were obtained. The stimulus dimensions were interpreted by the procedures outlined above. For convenience, these factors will be referred to as "composite" dimensions.

Composite multidimensional space. Table I presents the Spearman rank-order correlations between the four hypotheses and the factor loadings. If only those Ps over 0.44 ($P < 0.01$) are accepted as useful for dimension identification, the following descriptions can be made: Dimension (1) is related to Attneave's complexity index, which is based on the number of turns in the figure; Dimension (2) describes horizontally oriented figures with minimal compactness; Dimension (3) may be described as highly dispersed figures with a large number of turns in any orientation; Dimension (4) is similar to Dimension (3) except that the relationship to compactness is somewhat stronger; Dimension (5) represents figures with a large number of turns each of which is quite different from the just preceding angle; Dimension (6) groups the figures with high dispersion, large angular variability and number turns; Dimension (7) is merely angular variability. The tendency for these figures to be oriented in a vertical dimension distinguishes this dimension from Dimension (5).

TABLE I

RANK-ORDER CORRELATIONS BETWEEN COMPOSITE DIMENSIONS AND HYPOTHESES

Factor	Hypothesis			
	P^2/A	AV	C	R
1	−.13	.40	.64†	−.21
2	.60*	.22	−.09	.51*
3	−.58*	−.38	−.62†	−.07
4	−.77†	−.36	−.63†	.27
5	−.43	−.51*	−.51*	−.12
6	−.53*	−.47*	−.60*	.05
7	−.13	−.50*	.11	−.38

* Significant at the 1% level.
† Significant at the 0.1% level.

Viewpoint dimensions: Table II presents the rank-order correlations between factor loadings and hypotheses. Again, for purposes of interpretation, only those correlations of 0.44 or greater ($P < 0.01$) are used to 'name' the dimensions. For convenience the factors will be characterized as the i-th dimension in the j-th viewpoint. Thus A-1 refers to the first dimension of Viewpoint A.

Viewpoint A is unique in that it consists of a small number of dimensions each of which is clearly interpretable. The correlations indicate that one dimension is 'dispersion,' another is angular variability, and the third is number of turns

Viewpoint B has only one dimension that correlated significantly with any of the hypotheses. This dimension (B-1) seems to be a combination of number of turns and angular variability with some suggestion of dispersion. None of the other three dimensions was interpretable. An inspection of the factor plots indicates, however, that the stimuli are all closely grouped around zero. For this reason, the judgments measured by these dimensions may be essentially random.

Viewpoint C contains six dimensions, each of which is relatively well-defined. The dimensions may be interpreted as follows: (1) dispersed many-sided figures; (2) vertical many-sided figures: (3) dispersed many-sided figures with irregular angles; (4) compact figures; (5) vertically oriented figures with a tendency to many sides; and (6) horizontally oriented and dispersed figures with some degree of angular variability.

Viewpoint D contains five dimensions, of which only four are interpretable. D-1 represents dispersed horizontal figures with large angular variability. The second dimension (D-2) seems to be measuring the number of sides in the figures. D-3 represents compact figures with very few sides and the angles of which are similar to each other. The relationship in D-4, on the other hand, which is similar to D-3, is much weaker and does not contain the restriction to equal-angled figures.

TABLE II

RANK-ORDER CORRELATIONS BETWEEN VIEWPOINT DIMENSIONS AND HYPOTHESES

Dimension	Hypothesis				Dimension	Hypothesis			
	P^2/A	AV	C	R		P^2/A	AV	C	R
A-1	.83†	.54*	.49*	.04	C-6	.37	.39	−.01	.51*
A-2	.32	.73†	.27	.40	D-1	.64†	.57*	.22	.49*
A-3	−.09	.32	.65†	−.24	D-2	−.12	.41	.64†	−.07
B-1	.40	.48*	.65†	.03	D-3	−.83†	−.46*	−.83†	.24
B-2	.16	.25	.16	.00	D-4	−.53*	−.29	−.48*	.03
B-3	.15	−.11	.01	.03	D-5	−.21	.06	−.22	−.12
B-4	.06	.12	−.13	.17	E-1	.66†	.34	.09	.40
C-1	−.69†	−.17	−.57*	.23	E-2	−.20	−.46*	−.77†	.25
C-2	−.14	.10	.49*	−.38	E-3	.06	−.06	−.28	−.07
C-3	−.59*	−.45*	−.65†	−.08	E-4	−.63†	−.08	−.40	.13
C-4	−.42	.05	.31	−.18	E-5	.39	.71†	.35	.16
C-5	.01	−.26	.38	−.56					

* Significant at the 1% level.
† Significant at the 0.1% level.

Viewpoint E, like Viewpoint D contains five dimensions of which only four are clearly interpretable. E-1 is primarily a dispersion dimension. E-2 is primarily number of sides. E-4 is dispersion, again, but with a relationship to number of sides which distinguishes it from E-1. E-5 is clearly angular variability.

In many cases, the correlation of a dimension with a particular hypothesis might be increased (and sometimes the marginal correlations of the dimension with alternative hypotheses might be simultaneously decreased) by rotating the stimulus-dimensions somewhat. Multiple-correlation and canonical correlation procedures could even be used to determine a rotation that would maximize the relation between some rotated dimension in the stimulus-space and each hypothesis.

Discussion

From the data presented it is clear that, although interpretation of the composite dimensions can be made, the meaningfulness of these factors as descriptive of any coherent population is in question. Regression equations such as those developed by Attneave can be based on the composite factors, but their application to any given group of *O*s is less than clear.[21] As a matter of fact, the construction of a single regression equation for the entire composite may be questionable because of the near equality of variance accounted for by each of the seven composite factors. It will perhaps be more fruitful to devote future research to specifying the perceptual characteristics of the subgroups.

The finding that no one measure of the complexity or geometry of a visual form will serve to describe the dimensions used by individuals is not surprising. It is also not surprising to find that *O*s will differ substantially as to the dimensions they use. It is, after all, what could have been predicted from the work on perceptual styles that has been accumulating over the past several years.[22] For all their sophistication in devising methods of measuring the informational content in a form, workers in the area of visual form-perception seem to have overlooked the fact of individual differences in perception and judgment.

The finding that none of the measures investigated can be said to be a better visual form-metric than any of the others is strengthened by noting that all four of the metrics tested accounted for some of the dimensions; indeed, by examining other metrics, it is possible that the interpretation of some of the factors would become substantially clearer. The four specifically tested in this study (complexity, angular variability, dispersion, and angle of rotation) did seem, however, to account for a majority of the dimensions. Of the four, complexity (number of turns) seems to account for more of the judgments than the others (the number of significant [$P < 0.01$] correlations for this metric is 10), while angle of rotation is significantly related in only three cases. Even though dispersion (P^2/A) and angular variability are related to complexity ($\rho = 0.49, 0.53, P <$

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[21] Attneave, Physical determinants of the judged complexity of shapes, *ibid.*, 53, 1957, 221-227.

[22] R. W. Gardner, P. S. Holzman, G. S. Klein, H. B. Linton, and D. P. Spence, Cognitive control: A study of individual consistencies in cognitive behavior, *Psychol. Issues*, 1, 1959, No. 4; R. W. Gardner, D. H. Jackson, and S. J. Messick, Personality organization in cognitive controls and intellectual abilities, *Psychol. Issues*, 2, 1960, No. 8; S. J. Messick and F. Fritzky, Dimensions of analytic attitude in cognition and personality, *J. Personality*, 31, 1963, 346-370; H. A. Witkin, R. B. Dyk, H. F. Faterson, D. R. Goodenough, and S. A. Karp, *Psychological Differentiation*, 1962, 1-418 (Wiley).

0.01, respectively), there are enough cases in which two of the three correlate non-significantly with a given dimension to make an operational distinction useful.

It is interesting to note that there is a subtle distortion in the main dimensions of the visual field as revealed by the occasionally significant rotational hypothesis. Koffka and others have suggested that objects oriented more toward the horizontal will be preferred.[23] The present data suggest that some individuals will group stimuli according to their orientation along both dimensions (horizontal and vertical) of the visual field. Whether or not, however, the units between stimuli grouped along the vertical as opposed to those along the horizontal are equal must remain for future experimentation.

The question of what are the dimensions of form perception can not be settled by a study such as has been reported here. The present study only indicates some of the dimensions; constraints induced by the selection of stimuli preclude determining whether or not there are more. The very fact that the Crook's figures vary in number of sides may have forced such a judgment on our *O*s, and it is therefore to be expected that complexity would emerge as the most potent dimension.[24] With some of the dimensions interpreted, it remains for later workers to identify the actual potency of the relevant factors. Such workers should use a larger sample of stimuli with "marker" dimensions (based on the results of the present research) carefully included. Because of the limits of time, the traditional multidimensional methods would prove impractical if the number of stimuli becomes much greater than 30. Once dimensions of form perception are identified, however, various simple stimulus methods could be adapted to investigate stimulus-placement on these dimensions and the relation of these dimensions to various stimulus properties.[25]

In addition, the viewpoint-groups identified in this study must remain without interpretation, since independent data on the personal characteristics and preferences of the *O*s comprising each group were not available. These data were not gathered in the present study because the main focus of interest was on the dimensions themselves. Since distinct viewpoints did appear, it is important however, that future research attempt to identify these differences by including measures of perceptual and cognitive styles and, perhaps, of personality and preferences.

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[23] Kurt Koffka, *Principles of Gestalt Psychology,* 1935, 275-280; Martti Takala, *Asymmetries of Visual Space,* 1951, 1-175.

[24] Crook, Gray, Hanson, and Weisz, *The Effect of Noise on the Perception of Forms in Electro-visual Display Systems,* 1959, 1-15.

[25] W. S. Torgerson, *Theory and Method of Scaling,* 1958, 1-460.

SUMMARY

Fifty women *O*s judged 30 black-on-white geometrical forms, using the multidimensional method of successive intervals. The data were analyzed by the Tucker-Messick individual differences model. Five distinct viewpoints were found, none of which produced the same stimulus space as was obtained by analyzing the average ratings. Attneave's complexity metric, which is essentially a function of the number of sides, was found to be related to more of the dimensions than any of the other three metrics tested (angular variability, dispersion, and rotation). The implications of these findings for research on the metrics of visual form were discussed and suggestions made for subsequent investigations.

LACK OF INHIBITION DURING INVOLUNTARY SACCADES

By JOHN KRAUSKOPF, VIRGIL GRAF, Institute for Behavioral Research, and KENNETH GAARDER, Clinical Neuropharmacology Research Center, NIMH

This paper is concerned with the question of whether vision is inhibited during involuntary saccades. Saccades are rapid eye-movements which shift the line of regard abruptly. Two classes are recognized: *voluntary saccades,* which occur during pursuit or when one intentionally shifts his gaze from point-to-point, as in reading; and *involuntary saccades,* which are those found when *O* attempts to fixate steadily on a single point. A camera moved in the same way while a picture is being taken would produce a blurred picture, and early investigators of eye-movements tried to determine why vision is not similarly blurred.

A good account of the arguments is given by Woodworth.[1] There were two main positions: Holt suggested that vision was inhibited during saccades,[2] while Dodge and Woodworth held that the blur was subliminal, supporting their argument with the observation that a blur could be seen if the stimulus were sufficiently intense.[3]

The matter rested there for about 50 years, and then a new series of studies revived the notion of inhibition during saccades. Ditchburn provided a somewhat limited demonstration of inhibition during involuntary saccades,[4] and more complete experiments by Volkmann[5] and Latour[6] yielded evidence that thresholds are slightly elevated during voluntary saccades. Volkmann's experiments, which provide good quantitative data, indicate an elevation of threshold on the order of 0.5 log-units. Since *O* had a dual task in Volkmann's experiments—moving his eyes according to instruction and making his judgments—the obtained difference in threshold may to some degree be conditioned by task-complexity.

Involuntary saccades occur without any special effort on the part of *O*. This case, therefore, constitutes a simpler situation in which to study the effects of sac-

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* Received for publication March 25, 1965.

[1] R. S. Woodworth, *Experimental Psychology,* 1938, 591-594.

[2] E. B. Holt, Eye-movement and central anaesthesia, *Psychol. Monogr.,* 4, 1903, (No. 17), 3-45.

[3] R. E. Dodge, The illusion of clear vision during eye movement, *Psychol. Bull.,* 2, 1905, 193-199; R. S. Woodworth, Vision and localization during eye movements, *Psychol. Bull.,* 3, 1906, 68-70.

[4] R. W. Ditchburn, Physical methods applied to the study of perception, *Bull. Inst. Phys.,* 10, 1959, 121-125.

[5] F. C. Volkmann, Vision during voluntary saccadic eye movements, *J. opt. Soc. Amer.,* 52, 1962, 571-578.

[6] P. L. Latour, Visual thresholds during eye movements, *Vis. Res.,* 2, 1962, 261-262.

cades *per se.* Ditchburn's experiment was intended as such a test. An *O* viewed an oscilloscope, and whenever he made a saccade, a brief stimulus was presented. It was reported that a second *O* saw the stimuli more than the first *O*, but since the oscilloscope-face has some persistance, the image must have been somewhat smeared across the first *O*'s retina. In the present experiment, a controlled flash, 0.5 m.sec. in duration, was used, a duration short enough effectively to eliminate smearing of the image. Furthermore, more refined psychophysical techniques were used to permit estimates of the reliability and confidence-limits of the results.

The basic experimental procedure was to present test-stimuli alternately during saccades or at some fixed interval after saccades; if there were inhibition, thresholds should be higher for targets presented during the saccades. Two indices of visual sensitivity were studied: measurement of the absolute threshold served as a representative detection task; and measurements of vernier-acuity were made because a task requiring the extraction of information from a complex display might show the effects of inhibition even if a simple task did not.

METHOD

The apparatus consisted of: (1) an electrical eye-movement recorder, (2) a saccade-detector, (3) timing circuits, and (4) a visual stimulator. Two systems for recording eye-movements were used. Both provided electrical signals which were direct analogs of the angular position of the eye in the horizontal plane. The first system involved the use of a tight-fitting contact-lens and has been described in an earlier publication.[7] This method was used in the absolute-threshold experiment. The second system—more convenient in that it did not require a contact-lens—was used in the vernier-threshold experiment.

The second system, which measures the location of the margin between the iris and sclera, is similar to others, but the greater sensitivity of our apparatus warrants some description of the details.[8] A diagram of the optics is given in Fig. 1. Light from a straight coiled filament lamp is imaged on the eye as shown in the insert so that part of the light falls on the iris and part on the sclera. A Wratten 87 infra-red filter (*IR*) eliminates enough of the visible radiation that *O* does not see light from the system, while a heat-filter (*HF*) removes enough of the far infra-red that *O* does not feel the impinging radiation. Since Lenses L_1, L_2, and L_3 all have *f/* numbers less than 2, a large proportion of the light radiated by the filament is imaged on the surface of the eye and a large proportion of that light which is reflected by the eye is directed to the photocathode of the RCA 930 photocell (*PC*). As the eye rotates in the horizontal plane, more or less light impinges on *PC*. The noise-level of the photocell-signal is less than the output-change produced by a movement of less than 2′ of arc. It should be emphasized that this method does not share all of the advantages of the contact-lens method. For example, lateral movements of the head contaminate records obtained with the edge-of-the-iris method, but not those obtained with the contact-lens method, provided that the measuring beam is collimated at the mirror. The drying of the surface of the eye which occurs between winks probably changes its reflectivity and thus may introduce artifactual drifts in the record. Wink-artifacts are more frequent, because the cornea is not bathed with fluid as it is when a scleral contact lens is worn.

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[7] John Krauskopf, T. N. Cornsweet, and L. A. Riggs, Analysis of eye movements during monocular and binocular fixation, *J. opt. Soc. Amer.*, 50, 1960, 572-578.

[8] W. M. Smith and P. J. Warter, Eye movement and stimulus movement: New photoelectro-mechanical system for recording and measuring tracking motions of the eye, *J. opt. Soc. Amer.*, 50, 1960, 245-250.

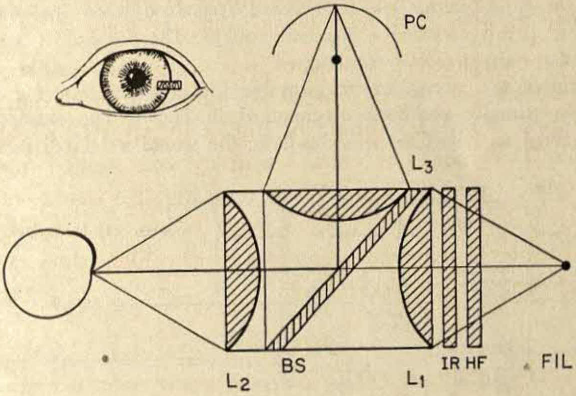

FIG. 1. APPARATUS FOR MEASURING EYE-MOVEMENTS
(Edge-of-the-iris method.)

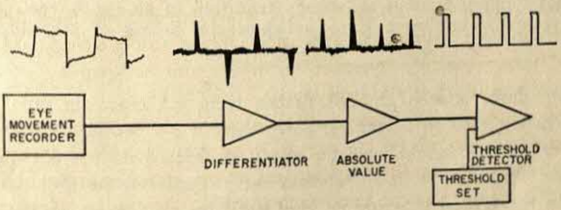

FIG. 2. SACCADE-DETECTOR
The waveform shown above each element is an artist's rendition of its output.

Since the purpose of the present experiments was to record the occurrence of saccades, only the last artifact presented problems, because the circuit detecting saccades did not discriminate between winks and saccades, but the distinction could be made easily by consulting the raw eye-movement records. It is highly unlikely that lateral head-movements abrupt enough to be confused with saccades can be made. Furthermore, when the records produced by the two methods are compared, the waveforms of movements identifiable as saccades are very similar. The advantages of the edge-of-the-iris method are (1) that no attachment is made to *O*'s eye, (2) a new *O* need not be fitted with a contact-lens, and (3) experimental sessions may be of greater length.

The saccades were detected by the circuit diagrammed in Fig. 2. If the electrical eye-movement records are differentiated with respect to time, the resulting signals give the instantaneous angular velocity of the eye. Saccades have durations which vary from about 15-30 m.sec., during which the eye achieves greatest angular velocities. Saccades therefore, appear in the differentiated records as brief positive or negative spikes, depending on their direction. The other eye-movements appear as low-level variations in the baseline. That saccades in either direction might be treated the same, the differentiated signal was passed through an absolute-value circuit. A threshold-detector which followed responded when the output of the absolute-value circuit exceeded a threshold-voltage. The threshold was so adjusted empirically that only saccades were detected.

The output of the saccade-detector was fed into a set of timing circuits which produced the stimulus-conditions diagrammed in Fig. 3. For example, the first saccade triggered an immediate stimulus-flash, the second a delayed flash, the third an immediate flash, and so on. All flashes were 0.5 m.sec. in duration. After a saccade which triggered an immediate flash, there was a period of 500 m.sec. during which further saccades could have no effect. A delayed flash was triggered by the next effective saccade. The delay between the saccade and the flash was 25, 50, or 200 m.sec. in the several experiments, and is shown as *X* m.sec. in the figure. After a saccade which triggered a delayed flash, no saccade could be effective for $500 + X$ m.sec. Two purposes were served by having these dead times. The 500-m.sec. period allowed *O* sufficient time to respond to each stimulus. The additional delay of *X* m.sec. following the delayed flash served to equate the minimal inter-flash-time preceding the two classes of stimuli. Without this feature, the immediate flashes would have impinged on the average on a more light-adapted retina.

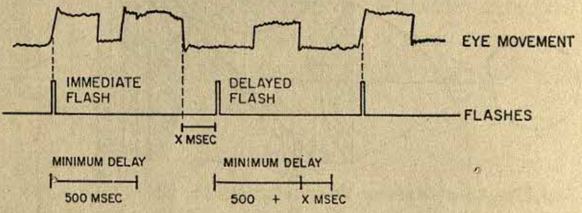

FIG. 3. TEMPORAL RELATIONS BETWEEN EYE-MOVEMENTS AND STIMULI

The upper trace is an artist's rendition of an eye-movement record. The lower trace indicates the timing of stimulus-flashes.

High-speed eye-movement records were used to check the timing of the immediate flashes. They were found to occur about 10 m.sec. after the onset of the saccade. On all saccades measured, the flash occurred well within the period of rapid movement, that is, on the straight portion of the rising trace.

In both experiments, the stimuli were generated by a Sylvania R 1131C glow-modulator tube. In Experiment I (absolute threshold), the cone of rays emerging from the tube was intercepted by a ground glass at a distance of 150 mm. from the source. A circular stop delimited a test-object whose diameter subtended 47′ of arc at the *O*'s eye. The viewing distance was 340 mm. The stimulus-luminance was varied by changing the current passing through the tube in 5 steps of 7.5 ma. from 25-55 ma. In Experiment II (vernier-threshold), a Maxwellian view was employed. The stimulus-configuration is shown in Fig. 4, and the various dimensions are given in the drawing. The lower line was fixed, while the horizontal location of the upper line was varied from trial to trial by means of a Grass oscilloscopic pen-motor. Five positions were used: 100″ left, 50″ left, center, 50″ right, and 100″ right. The settings were calibrated by examining a magnified image of the target with an eye-piece containing a micrometer. The retinal illumination of the circular field was 2 × 10^3 trolands. In both experiments, a thin orange ring 10′ in diameter served as a fixation-object.

In both experiments, stimulus-variation was controlled by a Western Union tape-transmitter. Punched tapes provided appropriate random stimulus-sequences. Responses were cumulated on counters and recorded individually on one channel of a Grass Polygraph, other channels of which were used to record the eye-movements and the output of the saccade-detector, and to monitor the occurrence of stimuli.

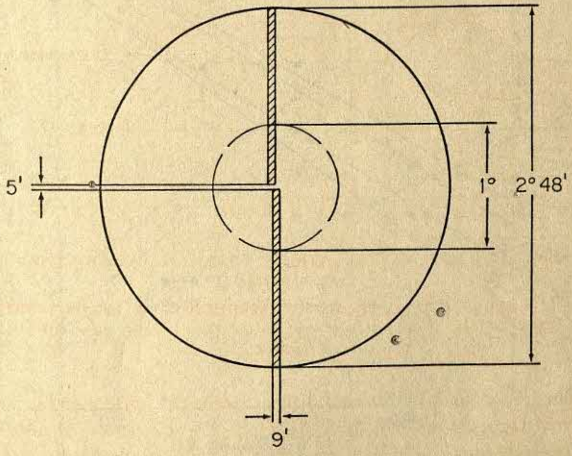

FIG. 4. DIAGRAM OF STIMULUS-FIELD IN EXPERIMENT II (Vernier-threshold)

After the necessary adjustments had been made, each session of both experiments was begun with a 5-min. period of dark-adaptation. In Experiment I, *O*'s task was to press a key whenever he saw a flash. A session consisted of 110 immediate and 110 delayed flashes, 22 of each kind at each luminance-level. One *O*, *KG*, was tested for 10 sessions each with delays of 25, 50, and 200 m.sec.; the other *O*, *VG*, was tested for 9 sessions at 50 m.sec. In Experiment II, *O*'s task was to signal whether the upper line appeared to the left or right of the lower line. No middle category of judgment was allowed. An experimental session consisted of 110 trials of both immediate and delayed stimuli, randomized among five levels. Both *O*s participated in 10 sessions with a 200-m.sec. delay used throughout.

Results. Results for the two observers in Experiment I with 50-m.sec. delay are shown graphically in Fig. 5. It can be seen that there is ilttle difference in the results for immediate and delayed flashes. The threshold for the immediate flashes is actually lower, although this apparent facilitation is not statistically reliable. The mean differences in threshold and the 99% upper confidence-limits for all conditions and *O*s are given in Table I. Only the mean difference for 25 m.sec. is statistically reliable, but the magnitude of the difference is only 0.028 log-unit.

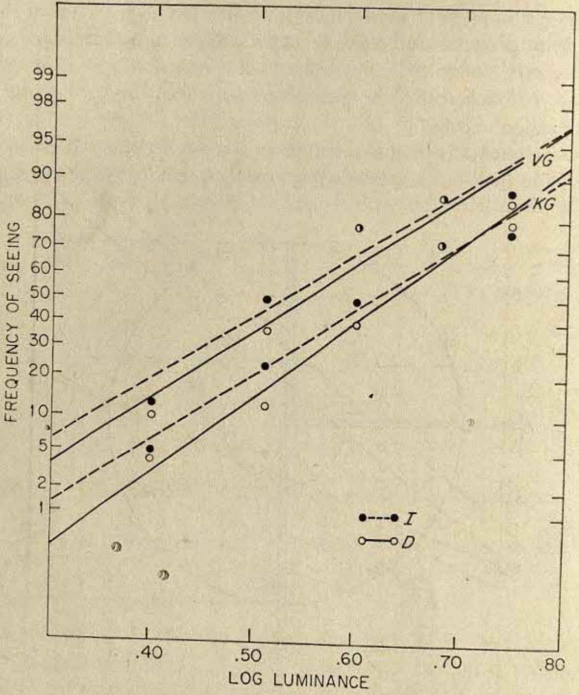

FIG. 5. RESULTS OF EXPERIMENT I
(Absolute threshold)

TABLE I

RESULTS FOR ABSOLUTE THRESHOLD

O	Delay (m. sec.)	Mean difference (log-units)	99% *UCL*
KG	25	+0.028	+0.072
KG	50	−0.013	+0.028
KG	200	+0.024	+0.114
VG	50	−0.020	+0.024

The results of Experiment II are shown graphically in Fig. 6. The lines were fitted by the method of least-squares. Again it is seen that there is little to choose between the two conditions. The vernier-threshold is defined as the reciprocal of the slope with the ordinate expressed in standard scores. The means, mean differences, and the 99% upper confidence-limits of the differences in vernier-threshold are given in Table II. The differences are not statistically reliable.

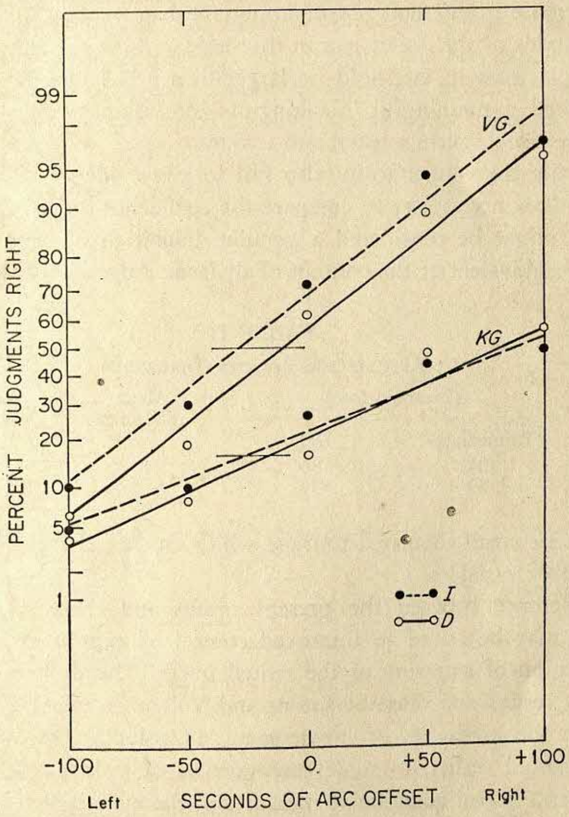

FIG. 6. RESULTS OF EXPERIMENT II
(Vernier-threshold)
The ordinate has been displaced downward for *KG*. The horizontal line indicates 50% judgment of 'right.'

Discussion. The results provide no support for the hypothesis that inhibition occurs during saccades. One of the four tests with the absolute threshold did show a statistically reliable change in threshold, but the mean effect was very small and only slightly larger than those found in the other three tests; that is, the statisical criterion is satisfied, not because of a change in the mean difference, but because of the reduced variability. There is, in fact, no evidence of a relationship between delay-time and mean difference in threshold. More important than statistical reliability are the magnitudes of the differences in threshold and their upper confidence-limits. An increase in threshold no larger than 0.114 log-unit can hardly be considered a meaningful 'blanking-out' of vision when the dynamic range of the visual system is taken into account.

The results for vernier-acuity also fail to give evidence of meaningful inhibition. It is not as easy to compare the confidence-limits of the results with what might be considered a genuine inhibition of seeing—that is, there is no equivalent of the concept of dynamic range— but the observed differences are small compared to those which can be produced by manipulation of other variables.

TABLE II

RESULTS FOR VERNIER-THRESHOLD

O	Threshold (sec)		Mean difference	Immediate-Delay 99% *UCL*
	Immediate	Delay		
KG	104	86	18	57
VG	59	57	2	22

The difference between the present results and those of Ditchburn apparently may be traced to improved control of exposure-duration and the elimination of smearing of the retinal image. The difference between the present results and those of Latour and Volkmann may be due to differences in the properties of involuntary and voluntary saccades *per se,* or, as suggested earlier, to some consequences of task-complexity. Volkmann presents useful quantitative measures of the magnitude of the effects of the various experimental conditions. At most, the thresholds change about 0.5 log-units, which is not, as she recognizes, what might be expected if vision were 'blanked out.' Volkmann's results are conditioned by the task the *O* has to perform as well as by whether the eye is executing a saccade or not. For example, the thresholds were generally lowest for the non-moving condition and higher for the conditions called Fix 1 and Fix 2. In all three conditions, the eye was stationary, but in Fix 1 and Fix 2 the test-flashes preceded or followed voluntary saccades. Volkmann interprets these differences in terms of a gradient of inhibition around the saccades, but there is no way to eliminate confounding of task-complexity and eye-movement condition. Volkmann recognizes that the differences she obtained might be conditioned by "the activation of nonspecific physiological systems which control the direction of attention."

The difference between the results obtained with voluntary and involuntary saccades may be due to some fundamental difference between these classes of eye-movement. For example, voluntary saccades generally are larger in amplitude and longer in duration. Further experiments with voluntary saccades of varying amplitude may serve to clarify the nature of the depression of vision which accompanies voluntary saccades.

ADVERBS AS MULTIPLIERS: SIMPLIFICATION AND EXTENSION

Donald W. Stilson, University of Colorado School of Medicine, and
Robert J. Maroney, V. A. Hospital, American Lake, Washington

Cliff has described a multiplicative rule for the combination of intensive adverbs (*e.g.* "very," "slightly") and evaluative adjectives (*e.g.* "good," "bad").[1] This rule has been verified, and it is of considerable interest in terms of what Osgood and Sebeok have called the "laws of word-mixture".[2] The scaling methods used by Cliff require formidable calculations and this might be expected to deter others from attempting to apply and extend his results.[3] In connection with the possibility of using simplified scaling, Cliff remarks that "some method such as assigning values by simply averaging the ratings given by the subjects would not generally be satisfactory in deriving the equivalent of a comparative judgments-scale. It would be so only if successive interval scaling would result in intervals of equal size."[4]

In spite of the fact that intervals of unequal size were found by Cliff, we are reporting an attempt to verify and extend the multiplicative rule for adverb-adjective mixture using mean ratings.

METHOD

The procedure involved four steps. A mean rating was obtained (a) for each adverb, (b) for each adjective, and (c) for each adverb-adjective combination. Finally, (d) the product of the mean rating for each adverb and the mean for each adjective were computed, and the linear regression of the mean ratings for adverb-adjective combinations on these products was examined. Thus, if C_i is the mean rating assigned adverb i, S_j is the mean rating of adjective j, and X_{ij} is the mean assigned the adverb-adjective combination (*e.g.* "slightly good," "very bad"), then the hypothesized relation is

$$X_{ij} = BC_iS_j + A,$$

where B and A are constants. This is equivalent to Cliff's formulation except that his method yielded values of the C_i such that $B = 1$.

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* Received for publication May 28, 1962. This work was done while the authors were at the University of Colorado, Boulder, Colorado.

[1] Norman Cliff, Adverbs as multipliers, *Psychol. Rev.*, 66, 1959, 27-44; Adverb-adjective combination in overseas groups, *Amer. Psychologist*, 15, 1960, 495.

[2] C. E. Osgood and T. A. Sebeok, Psycholinguistics: A survey of theory and research problems, Supplement to *J. abnor. soc. Psychol.*, 49, 1954, 1-203.

[3] Cliff used successive interval scaling and an analog of factor analysis for rectangular matrices.

[4] Cliff, *op. cit.*, 495.

Subjects. The *S*s in this research were students in introductory psychology classes and were predominantly college sophomores. The total of 115 *S*s was divided into a Pilot Group of 19 *S*s and six Experimental Groups containing 16, 16, 16, 17, 16, and 15 *S*s, respectively.

Questionary. All of the adverbs and seven of the 10 adjectives were from Cliff's lists. The adjectives "agreeable," "mediocre," and "tolerable" were added in an attempt to define more clearly the neutral point of the adjective scale. The words were shown in Column 1 of Table I.

In addition to these lists, every adverb was paired with every adjective to form a list of 69 adverb-adjective combinations (one pair was omitted through clerical error). This list contained the 10 unmodified adjectives, and 5 of the combinations occurred twice, for a total of 84 items.

TABLE I

MEANS OF ADVERB- AND ADJECTIVE-RATINGS

	Group of *S*s							
	Pilot ($n=19$)	I ($n=13$)	II ($n=15$)	III ($n=16$)	IV ($n=16$)	V ($n=15$)	VI ($n=12$)	Combined Groups ($n=87$)
Adverbs:								
Slightly	3.11	2.15	2.80	4.20	3.50	3.47	3.33	3.28
Somewhat	3.89	3.85	4.20	4.69	4.75	4.07	4.33	4.33
Rather	5.16	4.92	4.93	4.63	4.94	5.13	5.25	4.95
Quite	6.37	5.77	6.60	6.50	6.38	6.86	7.17	6.54
Decidedly	7.89	7.54	7.20	7.44	7.50	7.27	7.92	7.46
Very	7.21	7.46	7.40	7.50	7.69	7.47	8.00	7.57
Extremely	8.68	8.62	8.33	8.56	8.81	8.87	8.92	8.68
Adjectives:								
Evil	−2.68	−3.38	−3.73	−3.44	−3.75	−3.60	−3.42	−3.56
Disgusting	−2.42	−3.15	−3.07	−2.88	−3.06	−3.00	−3.25	−3.06
Bad	−2.16	−2.92	−2.60	−2.25	−2.63	−2.67	−2.82	−2.63
Mediocre	−0.58	−0.62	−0.73	−0.06	−0.25	−0.33	−0.33	−0.38
Average	−0.05	+1.15	+0.13	+1.83	+0.20	+0.47	+0.08	+0.66
Tolerable	−0.37	+0.08	−1.27	−0.69	−0.33	+0.13	+0.17	−0.35
Agreeable	+1.11	+1.54	+1.13	+1.75	+1.50	+1.87	+1.17	+1.51
Pleasant	+1.58	+2.54	+1.93	+1.94	+2.56	+2.27	+2.33	+2.25
Good	+1.89	+2.33	+2.27	+1.81	+2.25	+2.40	+2.33	+2.22
Charming	+2.32	+3.00	+2.53	+2.75	+3.00	+2.67	+2.50	+2.75

The order of the words in each list was random, but the combined list was subject to the restriction that neither the same adjective nor the same adverb occurred in succession. There were 3 orders of adverbs, 3 of adjectives, and 24 orders within the list of combinations. The three lists were assembled in each of the six possible orders. One list-order was administered to each of the six experimental groups.

For adjectives and adverb-adjective combinations, the written instructions asked *S* to rate "degree of favorableness" in describing a person on a 9-point scale. Parallel instructions required *S* to rate the "degree of strength" of each adverb.

RESULTS

Nine questionaries were eliminated due to failure to respond to a large number of items or a large number of marked inconsistencies in the ratings (*e.g.* one *S* reversed the scale for a full page). A total of 106 *Ss* entered into the analysis of the data.

Within each of the seven groups, the mean rating for each adverb and for each adjective was obtained. The middle value of the 9-point scale was arbitrarily chosen as the zero-point for adjective-ratings, so that the range of possible values for the adjectives is −4 to +4. For the adverbs, the scale-values ran simply from 1–9. These mean ratings are given in Table I.

Since all of the 10 adjectives and five of the adverb-adjective combinations were rated twice, it was possible to examine the stability of the mean ratings. The linear correlation between the means of the first and second ratings for Groups I–VI combined (n=87) was 0.997. The mean algebraic difference between the means of the first and second presentations did not differ significantly from zero ($p > 0.20$).

TABLE II

CORRELATION BETWEEN MEAN RATINGS FOR UNIVERSITY OF COLORADO STUDENTS ($n=87$) AND SCALE-VALUES OBTAINED BY CLIFF'S THREE GROUPS OF *Ss* (7 ADVERBS OR 7 ADJECTIVES ENTERED EACH CORRELATION)

	Wayne University ($n=218$)	Dartmouth College ($n=133$)	Princeton University ($n=186$)
Adverbs	.987	.998	.995
Adjectives	.951	.937	.921

To provide a test of the stability of results across populations, the linear correlation was obtained between our mean ratings (for Groups I–VI combined) and the scale-values for each of Cliff's three U.S. college samples. These correlations are shown in Table II, and a scatter plot of the relationship for the seven adjectives appears in Fig. 1. In the case of the adverbs, there is a very close relationship between our results and those of all three of Cliff's groups. For the adjectives, the relationship is somewhat weaker.

Since Cliff's values were presumably determined on a ratio-scale, and there was no assurance that our means were, we tested the hypothesis that the zero-points of our adverb and adjective scales for the combined group corresponded to the zero-points for Cliff's scales. These tests were made on the basis of the correlational analyses just described. In no instance could the null hypothesis be rejected at the 0.20 level of significance. If it is assumed that Cliff has generated ratio-scales, there is no evidence that our category-scales do not have a valid zero-point as well.

The primary focus of this study was upon the validity of the multiplicative rule for combining intensive adverbs and evaluative adjectives using mean ratings. To examine this, the mean rating for each adverb was multiplied by the mean for each adjective, and the linear correlation of these products with the mean ratings of the adverb-adjective combinations was obtained separately for each of the seven groups. These seven correlations and the correlation for the combined group are given in Table III. All of them are close to 0.95. The scatterplot of this relation for the combined group is shown in Fig. 2. The Xs in this plot are for combinations involving the adjective "tolerable."

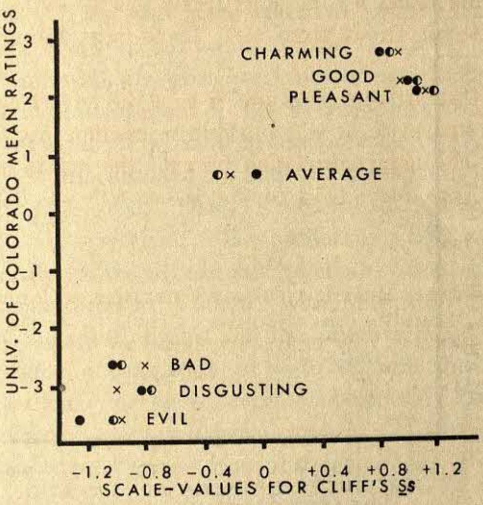

FIG. 1. RELATIONSHIP BETWEEN ADJECTIVES University of Colorado means against scale-values for Wayne University (Solid dots), Princeton University (Half dots), and Dartmouth College (x)

TABLE III

CORRELATIONS OF THE PRODUCTS OF THE ADVERB MEANS AND THE ADJECTIVE MEANS WITH THE MEAN OF THE RATINGS OF THE ADVERB ADJECTIVE COMBINATIONS

Group of *S*s

Pilot ($n=19$)	I ($n=13$)	II ($n=15$)	III ($n=16$)	IV ($n=16$)	V ($n=15$)	VI ($n=12$)	Combined Groups ($n=87$)
.967	.952	.938	.943	.937	.964	.970	.959

In addition to testing the multiplicative rule for *mean* ratings, its adequacy was examined for a few single individuals. To do this, one *S* was chosen randomly from each of the six groups, and the products of his

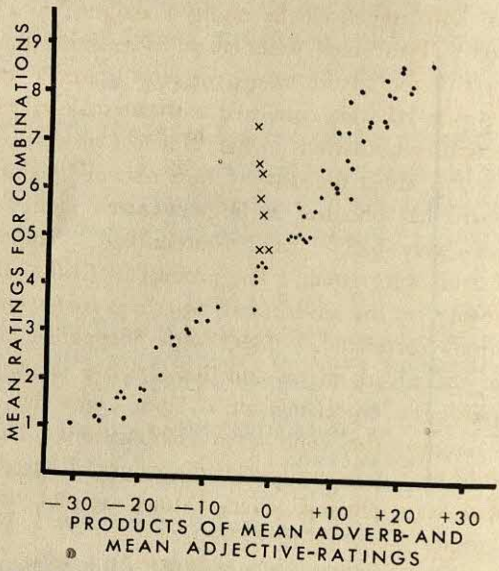

FIG. 2. CORRELATION OF THE PRODUCT OF MEAN ADVERB-RATINGS AND MEAN ADJECTIVE-RATINGS WITH THE MEAN RATINGS OF THE ADVERB-ADJECTIVE COMBINATIONS

Means are based on Experimental Groups I-VI combined. The X's are for combinations containing the word "tolerable"

TABLE IV

CORRELATIONS OF THE PRODUCT OF ADVERB AND ADJECTIVE RATINGS WITH RATINGS OF ADVERB-ADJECTIVE COMBINATIONS FOR EACH OF 6 RANDOMLY CHOSEN *S*s

Group from which *S* Chosen					
I	II	III	IV	V	VI
.842	.920	.947	.820	.905	.472

adverb and adjective ratings were correlated with his ratings of the adverb-adjective combinations. These correlations are shown in Table IV. Their mean (calculated using the Fisher transformation) is 0.86. A 95% confidence interval for the mean of this sample of six correlation coefficients has limits 0.676 and 0.944.

It is conceivable that the correlations of adverb and adjective products with the ratings of adverb-adjective combinations are ascribable in part to an artifact. All words and combinations were rated by *S* during a single session. Perhaps the rating of a single word (say "good") was peculiar to the individual and influenced the rating *S* assigned to a combination containing "good". Two steps were taken to examine this possibility. First, the products of the adverb means for our Group IV and the adjective means for Group III were correlated with the scale-values Cliff found for adverb-adjective combinations in his Wayne University sample. This correlation was 0.956, about the same as those obtained within our groups. Secondly, Sheppard has obtained in England mean ratings (on a seven point scale) for "very bad," "bad," "rather bad," "average," "rather good," "good," and "very good."[5] The product of Cliff's values obtained at Wayne University for the adverbs and adjectives corresponding to these seven combinations correlated —0.992 with Sheppard's mean ratings.[6] The products of the adverb means and the adjective means for our combined group yielded a correlation of —0.988 with the mean ratings Sheppard's *S*s assigned the same seven combinations.[7] Thus, since the multiplicative rule held quite well when the values of adverbs, adjectives, and combinations were obtained from distinct samples, the artifact described above cannot account for the results.

Other data reported by Sheppard permitted a test of the multiplicative rule for combining intensive adverbs and subjective estimates of monetary amounts. Sheppard reports the amount of money named as the weekly income of a man who was described as "very poor," "poor", "rather poor," "neither rich nor poor," "rather rich," "rich," or "very rich." If the log of the mean amount for "poor" is multiplied by our mean rating for "very," then the multiplicative rule can be used to predict the amounts Sheppard's *S*s assigned the term "very poor," similarly for "rather poor," "rather rich," and "very rich." The linear correlation between the predicted values and Sheppards observed log means is 0.973. Using Cliff's adverb scale-values, the analogous correlation is 0.976.

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[5] D. Sheppard, The adequacy of everyday quantitative expressions as measurements of quantities, *Brit. J. Psychol.*, 45, 1954, 40-50.

[6] The negative correlation is due to the fact that Sheppard's *S*s assigned small numbers to the favorable end of the rating scale.

[7] Since the unit of the scale of products of adverb and adjective means is not the same as the unit of mean ratings for adverb-adjective combinations, it was necessary to find the value which transformed an unmodified adjective mean rating to the unit of the product scale. Using the relationship between Cliff's adverbal scale-values and the mean ratings assigned adverbs by our six Experimental Groups combined, we found a mean adverb weighing of 5.967 on our scale to correspond to an adverb value of 1.000 on Cliff's scale. The prediction of Sheppard's mean ratings for the unmodified adjectives "bad" and "good" was made by multiplying the mean rating of "bad" and of "good" by 5.967.

Statements obtained by Sheppard of the distance one would walk corresponding to the verbal descriptions "very near," "near," "rather near," "neither near nor far," "rather far," "far," and "very far," when treated in the same way, yield a correlation of 0.964 using Cliff's adverb scale-values and 0.963 using our mean adverb ratings.

DISCUSSION

Although our data provide support for the multiplicative rule for both mean ratings and for individuals, it is clear in Fig. 2 that the adjective "tolerable" (represented by X's) is a maverick. Direct ratings of "tolerable" by itself or modified by "slightly" or "somewhat" are weakly unfavorable (negative), whereas it takes on positive values with stronger modification ("rather," "quite," decidedly," "very," "extremely"). Thus, the sign of the meaning of "tolerable" appears to interact with the strength of the intensive adverbs which weight it. This indicates that the multiplicative rule breaks down when applied to words whose meaning depends upon the word with which it is associated.[8]

The fact that the multiplicative rule could be applied to our data or to Cliff's to obtain rather accurate predictions of Sheppard's mean ratings for adverb-adjective combinations suggests that the multiplicaticative rule is relatively independent of the context set by instructions. This is because both Cliff's and our *S*s were instructed to rate adjectives according to their "favorability in describing a person," whereas Sheppard attempted to avoid providing any context for evaluation.

Although our data are limited in this respect, the use of intensive adverb scale-values as quantifiers of verbally determined estimates of income and distance suggests that such mixtures of word and number are acceptable to the human information processor.

The results indicate that the arbitrary zero points of our mean ratings agree rather well with those obtained using Cliff's more elaborate procedure and that our mean ratings come reasonably close to generating equal intervals. Were this not the case, the multiplicative rule could not be expected to fit the data at all well.

SUMMARY

Our results suggest that the multiplicative rule is applicable to most, but not all intensive adverb-evaluative adjective combinations, that it is not very sensitive to instructional context, that it is relatively insensitive to variations in scaling procedure, that it applies with moderate precision to the ratings of some individuals, and that it is quite stable among English-speaking *S*s.

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[8] A similar finding has been reported by F. J. Dudek (A comparison of scale values for adverbs determined by the constant-sum method and a successive intervals procedure, *Educ. psychol. Meas.,* 19, 1959, 539-548) for an intensive adverb.

UNCERTAINTY AND STAGE OF DECISION

By ARNOLD BINDER, Indiana University, BURTON R. WOLIN, ROSEMARIE WEICHEL, and STANLEY J. TEREBINSKI, System Development Corporation

Some writers have conceived of visual recognition in terms of the successive delimiting of the class of possible responses as discriminative cues are accumulated.[1] Binder used this conception in a task where the succession of cues and increasing specification of response-classes were under experimental control.[2] This made it possible to study the relationship between the level of ambiguity at the stage when a response was made and the personalities of the *S*s. He found a significant correlation between number of cues presented prior to the response and the amount of distrustfulness or suspiciousness on the part of the *S*s. Kiesler and King, on the other hand, did not find a relationship between perceptual cautiousness (waiting for more cues before responding) and general suspiciousness.[3] In a related experiment, Smock found that *S*s responded earlier (at levels of greater uncertainty) under 'stress' rather than 'security' conditions.[4] He concluded that individuals who are anxious may attempt to resolve ambiguity by early 'structuring or closure.'

The present experiment emphasizes a characteristic of the stimulus-situation rather than variables of context or personality. The particular characteristic was the relative frequency of presentation in the stimulus-series, which leads to a measure of stimulus-information.

Binder described a statistical model for recognition on the basis of information-theory.[5] Berlyne discussed the general applicability of an information-theory measure to those situations in which conflict is an important element.[6] Both of these approaches stressed the relationship between decision-, choice-, or reaction-time

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[1] Arnold Binder, A statistical model for the process of visual recognition, *Psychol. Rev.*, 62, 1955, 119-129; J. S. Bruner, On perceptual readiness, *Psychol. Rev.*, 64, 1957, 123-152.

[2] Binder, Personality variables and recognition response level, *J. abnorm. soc. Psychol.*, 57, 1958, 136-142.

[3] C. A. Kiesler and G. F. King, Individual differences in making perceptual inferences, *Percept. mot. Skills*, 13, 1961, 3-6.

[4] C. D. Smock, The influence of psychological stress on the "intolerance of ambiguity," *J. abnorm. soc. Psychol.*, 50, 1955, 177-182.

[5] Binder, *op. cit.*, 1955, 120-121.

[6] D. E. Berlyne, Uncertainty and conflict: A point of contact between information-theory and behavior-theory concepts, *Psychol. Rev.*, 64, 1957, 329-339.

and the amount of uncertainty in a stimulus-situation. This relationship between decision-time and information had been demonstrated experimentally.[7]

These results lead to the hypothesis that the greater the information in the stimulus-complex, the earlier the point at which attempted recognition will occur in a sequence of cues providing increasing specification. The information in the stimuli is measured by their relative frequencies of occurrence during paired-associate learning trials. Following learning to a criterion, component elements of the stimulus-patterns were presented in a fashion such that each succeeding presentation (with one exception) contained an additional differentiating cue.

METHOD

Subjects. Seventy-two *S*s were used. All were volunteers from among the employees of the System Development Corporation, recruited in response to a plea for *S*s which was published in the weekly newsletter of the Corporation. The *S*s were divided into three groups of 24, containing 12 women and 12 men.

Apparatus. The stimuli were presented in an apparatus using one-way mirrors. It was situated in a semi-darkened room, and *S* could see the stimuli only when they were directly lighted. The *S*s sat facing a 30 × 30-in. plywood panel mounted vertically on a table. In the center of the panel were the two mirrors, each 5 in. wide and 4 in. high. Behind each mirror, on *E*'s side, were boxes in which stimuli were placed. These boxes were so slotted on the bottom as to hold the stimulus-card horizontally. A second mirror at 45° to the line of regard presented the figures to *S* through the one-way mirrors. The stimuli were illuminated by tubular 40-w. bulbs, one in each box. These bulbs, together with a coating of white paint in the interior of the boxes, provided relatively even illumination of the stimuli.

Timing of presentation was accomplished by a cam-timer. Separate sequences were used for learning and testing phases of the experiment. During the learning, the stimulus-figures were shown in the left mirror and the required response in the right mirror; only the left mirror was used during the testing phase. A buzzer preceded each presentation. Then there followed, during the learning phase, a 1-sec. delay, the stimulus-figure for 2 secs. alone, both lights on for 2 secs., both lights off, and finally a 5-sec. delay before the buzzer for the next stimulus-response pair. During the testing phase, there was the 1-sec. delay, then the left light on for 3 secs., and a 1-sec. delay with the light off before the buzzer for the next presentation.

Stimulus-materials. The stimuli used during learning are shown in Column E of Fig. 1. These eight figures called for the following responses: E_1—Golf Flag; E_2—Seaway Pennant; E_3—Sail; E_4—Vacuum Cleaner; E_5—Tomahawk; E_6—Battle Axe; E_7—Hatchet; and E_8—Banner. The stimuli during the testing were potentially any of the 15 stimuli in Fig. 1. It will be seen that from left to right they contain progressively more differentiating cues.

The stimuli were drawn and then photographically reproduced on cards as mirror-images of the actual figures and words. The height of the figures was 3¼ in. The reduced figures at Levels A, B, C, and D (Fig. 1) were obtained by appropriately masking portions of the figures of Level E. Care was taken to insure that cues common to various figures appeared in the same location.

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[7] W. E. Hick, On the rate of gain of information, *Quart. J. exp. Psychol.*, 4, 1952, 11-26; Ray Hyman, Stimulus information as a determinant of reaction time, *J. exp. Psychol.*, 45, 1953, 188-196; F. C. Frick, Some perceptual problems from the point of view of information theory, in *Current Trends in Information Theory*, 1954.

Procedure. During the learning, the figures of Level E were presented, followed by their names. The sequence of trials differed for the three groups of *S*s. For Group I, each of the eight stimulus-response pairs was presented twice in a block of 16 trials. In the case of Group II, one of the eight stimuli was shown nine times per block, while the other seven were shown once each. The particular stimulus repeated nine times was rotated randomly over the whole group of 24 *S*s. For Group III, two of the eight stimuli were shown five times each, while the remaining six were shown once. The two more frequent stimuli were chosen either from E_1, E_2, E_3, E_4 or from E_5, E_6, E_7, E_8; that is, both of the more frequent figures came from the upper or the lower half of Level E as displayed in Fig. 1.[8]

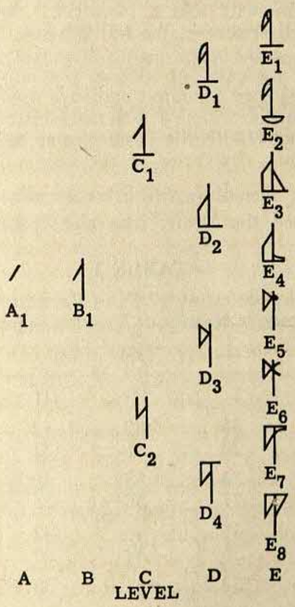

Fig. 1. Stimulus Figures Used During Learning (Column E) and During Testing (Progressively from Columns A to E)

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[8] These particular relative frequencies during learning for the three groups were chosen on the basis of two considerations; first, to have the same number of trials per block for the different groups and, secondly, to get a reasonably broad coverage of uncertainty values.

The order of presentation of the stimuli within blocks was randomized. Learning proceeded until a criterion of one errorless block of anticipations was reached.

The purpose of the testing phase was to determine at what stage in a sequence of incomplete or reduced figures, *S* would be willing to name the figure. Following the learning trials, these instructions were read to all *S*s:

> During this part of the experiment, I am going to choose one of the objects whose name you have learned. Rather than presenting the entire object at one time, I will present a series of clues that will make it increasingly obvious which object is being presented. These clues will appear in the left window. The right window will not be used during this part of the experiment. Your task this time is to call out the name of the object being presented as soon as you recognize it from the series of clues. After you have recognized an object and have responded with its name, a new series of clues will appear in which a different object will become apparent and to which you are to respond with the correct name as soon as you recognize the object.

The *S*s in all groups were then shown four sequences or trials of testing cues, each terminating when *S* named the figure. The eight possible sequences may be inferred by following the tree in Fig. 1 from left to right. The four sequences for an *S* were chosen such that one led to E_1 or E_2, one to E_3 or E_4, one to E_5 or E_6, and one to E_7 or E_8. Both the particular sequence from each pair and the order of the presentation among the four selected sequences were determined randomly.

TABLE I

TOTAL OF FIGURES SEEN BY 12 *S*S IN EACH SUB-GROUP BEFORE FIGURE IS NAMED IN TESTING SEQUENCE

Group	Sex	Test-Sequence				Average
		I	II	III	IV	
I	Men	35	40	42	41	39.5
	Women	28	37	32	41	34.5
II	Men	34	38	40	36	37.0
	Women	27	35	38	39	34.8
III	Men	35	40	42	51	42.0
	Women	36	38	39	40	38.3
Average of all groups		32.5	38.0	38.8	41.3	

RESULTS AND DISCUSSION

The score assigned to an *S* on a particular trial of the testing phase was the number of stimuli between 1 and 5, shown prior to his naming response. Since there were 4 tests or sequences, *S*'s total score could be between 4 and 20. The obtained range for these total scores was 5-19.

Table I shows a summary of the results of the 12 *S*s within each cell. A split-plot analysis of variance was performed on these scores. Only the *F* for trials is significant ($F = 10.36$, $p \leq 0.001$). That *S*s learn to wait over the four testing trials is not surprising. An early guess terminates the trial, and leaves the *S* uncertain as to whether his guess was correct.

The difference between sexes is quite sizable and consistent, althought the *F* falls short of significance. The three groups respond, on the average, at the same point in the sequence of tests.

It is true, of course, that a stimulus which was frequent during learning might or might not be the correct response during the test-sequence. Thus, for example, for Group II, a high-frequency response was correct up to Level D for only one sequence, and for two sequences a high-frequency response was incorrect beyond Level B. Therefore, the analysis of over-all scores lacked sensitivity. This was recognized in the design of the experiment so that both high frequency stimuli for Group III were included in the upper or lower half of Fig. 1. This made it possible to compare the responses in both Groups II and III for all cases where *S*s passed Level B without naming the figure. This allowed for a comparison on an intra-individual basis between paths leading to the half containing one or two high-frequency stimuli and the paths leading to the half with all stimuli of low frequency. The average points of response for the sequences entering the high and low frequency-halves were determined. A plus was assigned if the average score for the low frequency-paths was higher than the average for the high frequency-paths, and a minus sign if the other way around. An excess of plus over minus signs would support the hypothesis that more stimulus-information leads to earlier responding. The results for Group II were 7 pluses and 5 minuses, and for Group III 8 pluses and 6 minuses. These differences are, of course, far from significant.

To determine if the *S*s responded to the differential frequencies of presentation, an analysis was made of the frequencies of the various responses for Groups II and III. Ninety-six responses were made by the *S*s in Group II, and of these, 15 were names of the frequent stimulus (appearing nine times per block) and 81 were names of one of the other seven stimuli. Similarly, of the 96 responses for Group III, 27 were high-frequency names (one of the two stimuli appearing five times each per block) and 69 low frequency-names. The results of Binder and Feldman show clearly that the relative frequencies of responses to ambiguous cues (Levels A through D, Fig. 1) tend to equal the relative frequencies of occurrence of the full-cue objects (Level E) with which the responses were associated during learning.[9] If the *S*s in Group II responded with high-frequency names on the basis of an equal choice among the alternatives, we would expect the proportion to be 0.125. The obtained result of 0.156 differs little from that; comparable figures for Group III are 0.250 and 0.281. Binder and Feldman did not, of course, use repeated responses. Moreover, responses in the present experiment depended upon the test-sequence. For these reasons, a further analysis was made of high-frequency names chosen during the first sequence presented to each *S* and restricted to responses at Levels A or B.[10] For Groups II and III pooled, only 5 of 22 first sequence A or B responses were of high-frequency names. This obtained proportion of 0.227 may be compared with the proportion of 0.176 expected in the absence of any high-frequency preference. Matching would lead to a proportion of 0.588.

Subsequent experiments of Binder and of Feldman confirmed the matching tendencies found by Binder and Feldman.[11] The principal differences between the present experiment and the earlier ones are: (a) the population from which the *Ss* were selected—the other experiments used college students; and (b) the meaningfulness of the stimuli—the others used stimuli with no relationship to actual objects. The possbile effects of these (or more subtle) differences in producing the discrepant results will have to be explored in future research.

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[9] Arnold Binder and S. E. Feldman, The effects of experimentally controlled experience upon recognition responses, *Psychol. Monogr.*, 74, 1960, (No. 496), 1-43.

[10] Binder and Feldman, *op. cit.*, 3-22.

It does seem clear that the *Ss* in this experiment were not influenced in the test-responses by the varied frequency in the learning phase. On the other hand, neither did *Ss* respond with equal frequency in choosing among the names of the eight stimuli. Table II presents the frequencies with which the names of each of the eight stimuli were given, for each of the three groups and totally. It is perfectly apparent that the names "sail" and "banner" predominate; in fact 42% of all responses given consisted of these two. While the *Ss* did not respond more frequently in accord with the experimentally defined information, they used their prior cultural experience to reduce the uncertainty inherent in the test-performance. That is, either the ambiguous cues "looked more like" certain stimuli than others or the full-cue stimuli occur at differential frequencies culturally, or some combination of these and perhaps other factors influenced the choices.

TABLE II

FREQUENCY OF RESPONSE WITH EACH STIMULUS-NAME

Stimulus Name	Group I	Group II	Group III	Total
golf flag	14	14	14	42
seanay pennant	7	10	6	23
sail	26	20	16	62
vacuum cleaner	5	8	12	25
tomahawk	7	6	12	25
battle axe	2	3	6	11
hatchet	12	16	13	41
banner	23	19	17	59

It is also possible to relate the test-scores to the response-uncertainty, that is, to these differential frequencies of response. Responses were categorized as low-frequency responses (with high informational value), or high-frequency responses (with low informational value). If a response conveying little information will occur early in the recognition-sequence, the high-frequency responses should occur earlier than the low-frequency responses. For this purpose, the responses "sail" and "banner" were considered high frequency and all others low frequency.

Table III contains the average score for these high- and low-frequency responses under each of the three testing conditions. It was not possible to compare these numbers statistically, since they are averages within *Ss* over trials as well as over *Ss*. Conequently, to test statistically the hypothesis that high-frequency responses occur earlier in the sequences than low-frequency responses, intra-individual comparisons were made. For *Ss* who gave both high- and low-frequency responses, the analysis consisted of comparing the average point of high-frequency response with the average point of low-frequency response by means of the sign test. The results are given in the lower part of Table III. It should be pointed out that this effect is not the result previously found of longer delays during the later sequences. The responses to E_3 and E_4 did not occur more often during early test-sequences. The evidence is therefore quite convincing that reduced uncertainty of response as defined in this context is associated with earlier responding within a sequence of cues leading to increasing object-specification.

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[11] Binder, Supplementary report: Recognition responses to ambiguous cues following paired-associate learning, *J. exp. Psychol.*, 61, 1961, 263-264; S. E. Feldman, Probabilistic hierarchies to ambiguous "concept-classes," *J. exp. Psychol.*, 65, 1963, 240-247.

There is a difference between men and women in this experiment. This was evident, although not significant, in Table I. It is also shown by an analysis of error scores. The men made significantly fewer errors than the women. This is not surprising, since the average educational level of men at the System Development Corporation is higher than that of women. One is led to the hypothesis that a higher educational level leads to a tendency toward more certainty before responding. Sex and educational level are confounded in this experiment. They were not in Kiesler and King's experiment, in which there was no difference in response-tendencies between men and women.[12] This hypothesized relationship seems reasonable on intuitive grounds.

TABLE III

COMPARISON OF SCORE (STAGE IN TEST-SEQUENCE) OF HIGH- AND LOW-FREQUENCY RESPONSES

	Frequency	Group		
		I	II	III
Average scores	high	2.59	2.63	2.81
	low	3.59	3.23	3.61
Sign test	*N*	14	14	14
	p	.006	.029	.001

TABLE IV

ERROR DISTRIBUTION OVER STIMULI DURING LEARNING

Stimulus	Errors			
	Group I	Group II	Group III	Total
1	7	19	11	37
2	19	26	20	65
3	9	14	8	31
4	6	3	4	13
5	28	22	38	88
6	28	43	42	113
7	30	33	31	94
8	24	26	25	75

One other result is of interest—the distribution of errors over the stimulus-figures during learning. Table IV contains these errors for the various groups.

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[12] Kiesler and King, *op. cit.*, 5

It is evident that the preference for a response to E_3 and E_8 during testing cannot be explained as overlearning, since, using errors as a measure, these two responses do not differ from the rest.

The above comparison of high-frequency response vs. low-frequency response is similar to Hyman's analysis of the relationship between reaction-time to an individual stimulus and the probability of occurrence of that stimulus.[13] Although he found that the reaction-time to individual stimuli could not be predicted from the line relating average reaction-time and information associated with a set of stimuli, he did find that the mean reaction-time to frequent simuli (low information) was faster than to infrequent stimuli (high information). Newbrough obtained similar results in relating errors of recognition and uncertainty of both sets of and individual stimuli.[14]

Perhaps the results of the present experiment have significance for the question, discussed fully by Garner, as to whether the perceptual effects (including reaction-time) of information stem from stimulus- or response-uncertainty.[15] While the distinction between these uncertainties is by no means clear in many cases, there is considerable evidence that the informational effects of response-restriction are far more important than stimulus-restriction. The finding of the present experiment of no difference in response-scores among Groups I, II, and III, together with the finding of a significant difference in point of response between high and low frequency-response suggests further that response-uncertainty is the important variable.

SUMMARY

The present experiment investigated the relationship between different relative frequencies of presentation during the learning of paired-associates and the point of recognition in a sequence of cues derived from the learned figures and providing gradually increasing object-specification. Three groups of *Ss* learned the names of eight figures to a criterion and then were presented with sequences of abbreviated testing figures providing more and more cue-information relative to one of the learned figures. During the testing phase, each *S* was instructed to respond, in each of four sequences, with the name of the figure whenever he recognized it. The longer he waited before responding, the higher his score.

The three groups differed in the information or uncertainty of the learning trials, calculated on the basis of relative frequencies of occurrence during learning. No difference among the three groups in score was found during the subsequent testing. When the responses during testing were divided into two categories—names given with high frequency and low frequency—it was found that reduced uncertainty (high frequency) is associated with earlier responding in the sequence of cues.

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[13] Hyman, *op. cit.*, 188-196.

[14] J. R. Newbrough, Interaction between total stimulus information and specific stimulus information in visual recognition, *J. exp. Psychol.*, 55, 1958, 297-301.

[15] W. R. Garner, *Uncertainty and Structure as Psychological Concepts*, 1962, 35-38.

RANDOMIZATION BY HUMANS

GUSTAVE J. RATH, Northwestern University

The idea that humans are not good randomizers comes from psychophysics.[1] The conjecture that their attempts to randomize will show the inverse of the order they expect in the world arises from these studies. Alternative hypotheses may be found in Ross.[2] For example, if *S* expects to receive ones in a binary sequence with a high probability, then he will write zero with an equally high probability.

The experiments by Senders and Day were directed toward the evaluation of the assumption that humans when forced to guess in a psychophysical experiment will do so randomly.[3] Senders found that the assumption of randomness is met closely enough so as not to invalidate seriously many experimental techniques or methods of analysis, even though non-randomness clearly exists.

There has been an interest among psychologists and economists in evaluating game-theory as a model for human behavior.[4] While the greater part of the research has been directed toward the study of strategy in different games and in the problem of subjective utility, a question which is not commonly studied is: Can a player carry out a mixed strategy? A very simple class of mixed strategies consists of randomizing a set of pure strategies. In playing "Scissor, Paper, and Rock" or matching pennies, the optimum (game-theoretic) strategy would consist of choosing the alternatives randomly with equal probabilities.

The generation of random sequences using more than two alternatives has not been followed to the study of 10 (decimal) and 26 (alphabetic) alternatives. Early psychophysics did concern itself with more than two

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* Received for publication November 18, 1963. The programming for the reduction and analysis of this data was done by Francis DeKalb of the IBM Research Center.

[1] S. W. Fernberger, Interdependance of judgments within the series for the method of constant stimuli, *J. exp. Psychol.*, 3, 1920, 126-150; Leon Arons and F. W. Irwin, Equal weights and psychophysical judgments, *ibid.*, 15, 1932, 733-751.

[2] B. M. Ross, Randomization of a binary series, this JOURNAL, 68, 1955, 136-138.

[3] V. L. Senders and Ann Sowards, Analysis of response sequences in a setting a psychophysical experiment, this JOURNAL, 65, 1952, 358-374; Senders, Further analysis of response sequences in the setting of a psychophysical experiment, this JOURNAL, 66, 1953, 215-228; W. F. Day, Serial non-randomness in auditory differential-thresholds as a function of interstimulus interval, this JOURNAL, 1956, 69, 387-394; A descriptive analysis of the non-randomness of serial threshold data, this JOURNAL, 1957, 70, 238-242.

[4] Ward Edwards, The theory of decision making. *Psychol. Bull.*, 1954, 51, 380-417; M. M. Flood, in R. M. Thrall, C. H. Coombs and R. L. Davis (eds.), *Decision Processes*, 1954.

alternatives.[5] Three alternatives were studied by Lieberman, but he carried out no serial analysis.[6] These results may be interpreted to favor alternation, even though he didn't choose to. Placing *three* different symbols in a cell may be considered an alternation. Recently, Ross and Weiner explored a very large set of alternatives in using matrix and spatial patterns.[7] It seems appropriate to study the behavior in fairly large samples and with a higher number of alternatives.

METHOD AND PROCEDURE

Apparatus. The *Ss* were furnished with printed forms which contained in addition to identification locations, a grid of 25 by 10 rectangles on a sheet of 8½ × 11-in. paper. They were provided with a booklet containing 10 pages (2500 rectangles) and a pencil.

Subjects. Sixty volunteer college students served as *Ss* in the experiment.

Procedure. The *Ss* were arbitrarily divided into three groups of 20. Three conditions were established involving respectively the randomization of binary (0-1), decimal, and alphabetic characters. Each *S* served only under one condition; thus we shall call each group by the condition in which they served.

The *Ss* were instructed to write random characters. Randomness was explained through the use of an actual urn model, but only one draw was made from the urn in the demonstration. The *Ss* were paced by writing page numbers on a blackboard every 5 min. They were instructed to keep up to or ahead of the page number on the blackboard. They were further told that they could leave the experiment as soon as they had completed the booklet. The time to complete the booklet was recorded for all the *Ss*.

Tests. (1) The first test performed dealt with the equal probability of responses (engrams). The null hypothesis can be formally stated as follows:

$$H_o : P\ (R_t = J) = 1/N. \qquad\qquad i = j$$

where R_t is a random variable consisting of the *Ss* responses defined as follows for the three conditions

$$\begin{array}{lll} \begin{matrix} i \\ ; \\ j \end{matrix} & 1,0\ , & \text{in the Binary Case and } N = 2 \\ \begin{matrix} i \\ ; \\ j \end{matrix} & 0,1,2, \ldots 9\ , & \text{in the Decimal Case and } N = 10 \\ \begin{matrix} i \\ ; \\ j \end{matrix} & \text{a,b,c}, \ldots \text{z}\ , & \text{in the Alphabetic Case and } N = 26. \end{array}$$

(2) The second test applied to those *Ss* who were retained in the first test, that is, those not showing a bias in terms of single responses. Formally, using the previous notation, one would state the null hypothesis

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[5] M. G. Preston, Contrast effects and the psychophysical judgments, this JOURNAL, 48, 1938, 389-402.

[6] Bernhardt Lieberman, How people predict three balls fall into three cells, *Psychol. Rep.*, 13, 1963, 711-716.

[7] B. M. Ross and S. G. Weiner, On making random patterns, *Percept. Motor Skills*, 17, 1963, 587-600.

$$H_o : P_s \; (R, R_{s+o}) = P \; (R_s) \; P \; (R_{s_{1+}}).$$

The effect of this analysis and the other higher order analysis is to remove the lower order biases and test only the bias at the level.

The one *S* who was not rejected by the two previously states tests was tested for the equiprobability of his third order of trigram frequencies. The logic followed was the same as for the second-order test.

A 95% level of confidence was used in all tests of statistical significance.

RESULTS

The engram or first order frequencies were tested for all the *S*s by Chi Square. Four out of 20 Binary *S*s responded with substantially equiprobable responses. The remaining 16 departed significantly from 0.50. The data from all the decimal and alphabetic *S*s deviated significantly. Considering diagram or second order frequencies only one out of the remaining four successful binary subjects did not allow the rejection of the hypothesis equiprobability. That single subject was rejected at the trigram level.

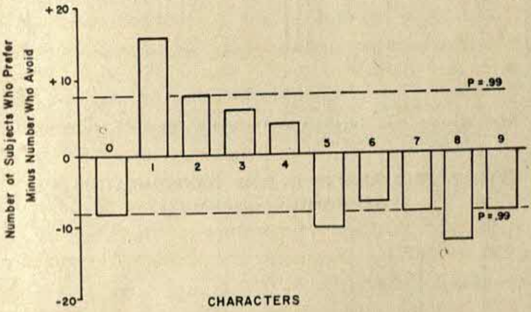


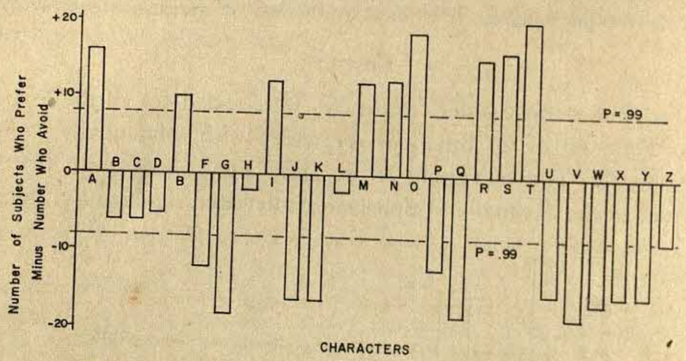
FIG. 1. PREFERENCES IN THE RANDOMIZATION OF DECIMAL CHARACTERS

To explore further the biases that were exhibited, the first order frequencies or engrams were examined for preferences. Eighteen of the 20 binary *S*s preferred one over zero. The decimal preferences are shown in Fig. 1, which shows that 1 and 2 are significantly preferred and 0, 5, 7, 8, 9 are significantly avoided. The alphabetic preferences are even more interesting.

The eight most frequent letters in the English language (*e, t, a, o, n, r, i,* and *s*) are preferred and the twelve letters, *f, g, j, k, p, q, l, u, v, w, x, y,* and *z* significantly avoided. A Spearman rank-correlation was carried out between the preferences for the letters and the ranks by frequency of the letters in the English language giving a correlation of 0.84. Fig. 2 shows the alphabetic preferences.

The preferences of alternation versus repetition in the binary case was tested by grouping together 00 and 11, and grouping 10 and 01. Correcting for the engram (first order) biases, one finds that 16 out of 20 *Ss* avoid repetition and prefer alteration.

A more complete procedure is needed to analyze the decimal and alphabetic cases. If we take the number of units difference in the natural order of enumerating the characters, we have a measure going from 0 to *N*-1, where *N* is the number of characters. Therefore, examples of difference 0 would be 11, 99, *aa,* or *ff*. A difference 1 would be 12, 54, 67, or *ab, fe,*



Note: Spearman Rank Correlation of Preferences and Ranks of Frequencies of Letters in English = .84

Fig. 2. Preferences in the Randomization of Alphabetic Characters

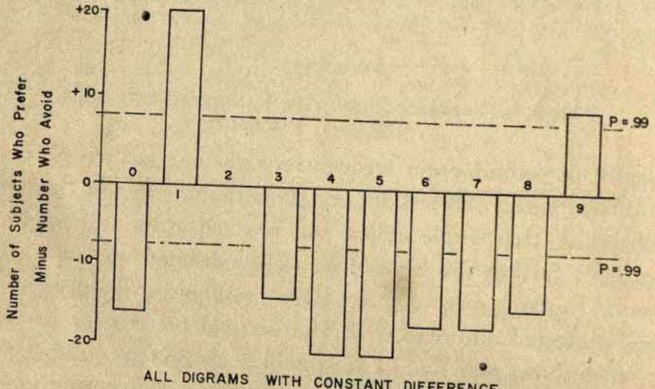


Fig. 3. Frequency of Repetition of Decimal Characters in Terms of Number of Steps in Sequence Separating Repeated Characters

jk, *yz*. Pooling the frequencies for all the data having the same difference, we can compare it by a Chi square with the expected frequencies, having corrected for the first order effects. We find, as illustrated in Fig. 3 and 4,

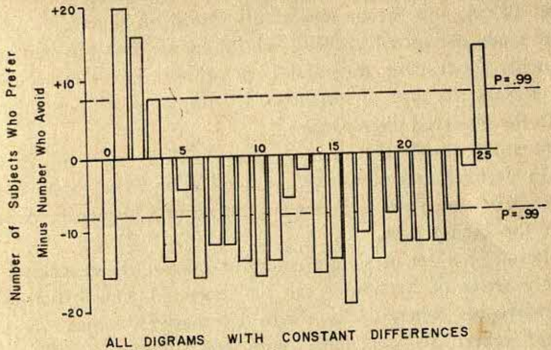


FIG. 4. FREQUENCY OF REPETITION OF ALPHABETIC CHARACTERS IN TERMS OF NUMBER OF STEPS IN SEQUENCE SEPARATING REPEATED CHARACTERS

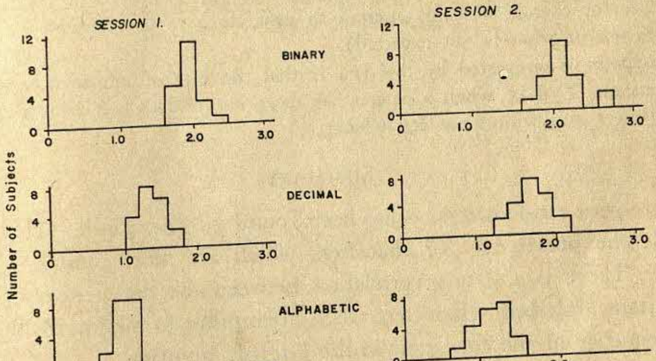


FIG 5. RATE OF CHARACTER PRODUCTION FOR THREE SETS OF CHARACTERS
Each Session Required Production of 2500 Characters

that there is a great avoidance for difference 0 (repetition) and a great preference for characters adjacent in their natural sequence.

The rate at which the *S*s produce the characters decreases as the number of possible characters increases. The actual distributions of rates are illustrated in Fig. 5.

DISCUSSION

Aside from supporting the widely found and accepted knowledge that humans are not good randomizers, what has been found?

Substantial agreement with the avoidance of repetition in a two or three choice situation exists between this study and the studies of Fernberger, Arons and Irwin, Zlotowski and Bakan, and the theoretical discussion of Skinner.[8] There is disagreement with the study conducted by Ross, who does not find the avoidance of repetition.[9] In a further experiment, Ross and Levy support Ross's previous conclusions.[10] According to Bakan, the type of instructions, equipment, set, and method of analysis may lead to the observed discrepancies.[11]

The present study shows that the previous findings with binary sequences can be extended to decimal and alphabetic randomization. The *S*s do avoid repetition and prefer symbols which are adjacent in the natural sequence. Adjacency is like alternation in the binary case.

The formulation of a set of simple rules for human choice-making has not been advanced, but a series of hypothesis can be generated. One hypothesis is that the first order preferances when dealing with alternatives, which are common in a culture, should reflect *S*'s experience with the alternatives. This hypothesis is generated by the high correlation between *S*'s alphabetic character preferences and the frequencies of the characters in the English language. Another hypothesis is that *S*s expect repetition in the world and do not expect change (the Cournot hypothesis in economics).[12] This is supported by the avoidance of repetition and preference for change when *S*s attempt to generate a random sequence (what the world is not or what is not expected).

An hypothesis suggested by the data is that the rate of information-generaiton is a constant. That is, when a person has more alternatives to randomize, he will do it slower and with more redundancy.

SUMMARY

*S*s are poor randomizers, as has been found in other studies. Their biases include the preference for characters which are more common in their culture. There is a strong correlation between the preferences of the *S*s for certain alphabetic characters when attempting to randomize them and the frequency of the characters in the English language.

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[8] Fernberger, *op. cit.*, 126-150; Arons and Irwin, *op. cit.*, 733-751; Martin Zlotowski and Paul Bakan, Behavioral variability of process and reactive schizophrenics in a binary guessing task, *J. abnorm. soc. Psychol.* 66, 1963, 185-87; B. F. Skinner, The process involved in the repeated guessing, *J. exp. Psychol.*, 30, 1942, 492-503.

[9] Ross, *op. cit.*, 136-138.

[10] B. M. Ross and Nissim Levy, Patterned productions of chance events by children and adults, *Psychol. Rep.*, 4, 1958, 87-124.

[11] Paul Bakan, Response-tendencies in attempts to generate random binary series, this JOURNAL, 73, 1960, 127-131.

[12] A. A. Cournot, *Mathematical Principals of the Theory of Wealth (Bacon Translation)*, 1929, Ch. 18.

The finding that the *S*s avoid repetitions and prefer alternation can be extended to decimal and alphabetic randomization by redefining alternation as the choice of an adjacent character in the natural sequence.

Lastly, the rate of character generation is negatively correlated with the number of alternatives to be randomized.

TWO SOURCES OF DISTRACTION IN INCIDENTAL LEARNING

By WILMA A. WINNICK,
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Winnick and Lerner studied the effects of three kinds of external distraction on intentional and incidental learning.[1] Although all three distractions effected a slowing down of intentional learning, no comparable effect was found on the amount of recall for irrelevant material shown simultaneously with the material to be intentionally learned. The apparent immunity of incidental learning to distractive effects was viewed as possibly due to the special characteristics of incidental learning. This type of learning, occurring as it does at the same time as other learning or with attention directed elsewhere, may be so highly selective as to occur even in the presence of distraction.

On the other hand, the contrast shown by the effect of distraction on intentional learning and its lack of an effect on incidental learning may be a function of the experimental procedure. In the study cited, distraction was introduced only during intentional learning, not during the test of incidental recall; this raises the possibility that the findings reflect differential effects of performance.

The present study, therefore, in its first experiment, attempted to determine whether the resistance of incidental learning to distraction would be maintained with distraction introduced during the attempt to recall the irrelevant material, as well as during the actual learning. Should incidental learning prove unaffected by distraction during both learning and recall, its immunity would be demonstrated at both the learning and the performance levels.

A second experiment is also reported which, in contrast with the first, introduced a source of distraction inherent in the learning situation. Johnson and Thomson found incidental learning to occur when the incidental task (recall of nonsense-syllables being memorized by a partner) was a

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[*] Received for publication November 9, 1964. Supported by a grant from the Graduate Division of the City University of New York. The data in Experiment I were collected by Mrs. Lana Stein and are reported in a thesis prepared by her in fulfillment of the requirements for the degree of Master of Arts from Queens College of the City University of New York.

[1] W. A. Winnick and R. A. Lerner, Intentional and incidental learning under distraction, this JOURNAL, 76, 1963, 683-686.

distraction for the simultaneous intentional learning of other syllables by *S*.[2] The nature of the task, however, was such that no control measure (*i.e.* scores from a 'no distraction' group) could be obtained to evaluate the amount of incidental learning. In the present study, the distraction arose from an incongruity between the intentional and incidental materials to be learned. It was predicted that increased attention would be required to learn the intentional material with a consequent reduction in the amount of incidental learning.

EXPERIMENT I

Method: (1) Subjects. Four groups of 16 *Ss* each were composed by random assignment of students in introductory psychology.

(2) Experimental design. A 2 × 2 factorial design was used, comprising two treatment groups (distraction, no distraction) during intentional learning and two similar treatment groups for the test of incidental recall.

(3) Apparatus and materials. A list of 12 girls' names was exposed on a Lafayette memory drum and served as the relevant material for intentional learning. Each name was typewritten in a 2¼ by ¼-in. rectangle of color, the color providing irrelevant material in temporal and spatial proximity to the relevant. The names and colors were those used by Winnick and Lerner.[3] The 'distraction' material consisted of a recording of *World News of 1959* narrated by Walter Cronkite and played through earphones on a Wallensak tape recorder.

(4) Experimental procedure. The method of anticipation was used for the intentional learning; the names were exposed at a 2-sec. rate with an 8-sec. inter-trial interval. Learning was continued to the criterion of one errorless trial. Immediately after the completion of intentional learning, the *Ss* were shown a chart with the six colors across the top and the names in serial order down the side. They were instructed to match names with colors; the number of correct matches was the incidental recall-score.

Results and discussion. Table I indicates that groups exposed to distracting materials during intentional learning required a greater number of trials to criterion than groups learning without distraction. The means of the two distraction-groups (13.25 and 13.69 trials to criterion) were close to identity, as were the means of the two no-distraction groups (6.00 and 5.88), indicating equation on intentional learning of the subgroups that were to receive varying treatments during the test of incidental recall. Analysis of variance revealed a significant difference between the distraction–no-distraction groups ($F_{1,60} = 91.9$; $p < 0.001$).

Both serial positional curves and mean error-rates for intentional learning were compared for the two groups to determine whether the distraction exhibited drive-properties. Any such properties might be revealed in the greater rate of error and more peaked serial positional curves under distracting conditions, both indications of the strengthening of initially strong incorrect responses by increased drive. Although mean error rates

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[2] Ronald Johnson and Calvin Thomson, Incidental and intentional learning under three conditions of motivation, this JOURNAL, 75, 1962, 284-288.

[3] *Op. cit.*, 684.

TABLE I

MEAN SCORES FOR INTENTIONAL LEARNING AND FOR INCIDENTAL RECALL
(Experiment I)

	Conditions during intentional learning			
	No Distraction		Distraction	
Conditions during incidental recall	Trials to learn int. material	Inc. recall	Trials to learn int. material	Inc. recall
No distribution	6.00	2.38	13.25	1.75
Distribution	5.88	2.38	13.69	3.00

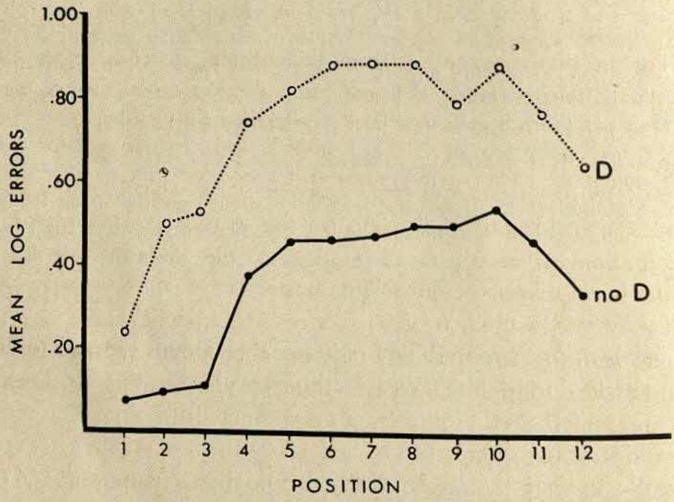

FIG. 1. SERIAL POSITION-CURVES FOR INTENTIONALLY LEARNED MATERIAL WITH AND WITHOUT DISTRACTION
(Experiment I)

were identical under 'distraction' and 'no-distraction' (mean = 1.9 errors per trial), the two serial positional curves, as shown in Fig. 1, were somewhat different in shape.

The serial positional curves have been plotted using mean log errors (overt errors plus omissions). The logarithmic values convert the possible multiplicative influence of experimental variables into an additive curve.[4] To evaluate differences between these curves a two-way mixed analysis of variance (experimental treatment, serial position) was carried out for the data plotted in Fig. 1. This analysis found significance in both main effects, for treatments ($F_{1,62} = 50.03$; $p < 0.001$) and positions ($F_{11,682} = 44.45$; $p < 0.001$) and in the treatments by positions interaction ($F_{11,682} = 64.09$; $p < 0.001$). The significant interaction, revealing as it does a difference in shapes of the curves for the two treatments, may be taken as indicative of drive-characteristics of distraction.

The data in Table I show little difference in the amounts of incidental material recalled among the four groups. The mean numbers of colors recalled for the two subgroups learning intentional material without distraction were identical: 2.38 for both groups, one tested under distraction, the other tested without distraction. Distraction, then, introduced during the test of incidental recall had no effect on test scores, where there had been no distraction during intentional learning. A loss on the recall test for the distraction group would have been indicative of a performance effect. For the two subgroups learning intentional material under distraction, a small nonsignificant difference in incidental recall scores was obtained; the groups tested without distraction having a mean of 1.75, the group tested with distraction a mean of 3.00 colors recalled.

An analysis of variance of the incidental scores showed no significant differences in incidental recall scores for the treatments or for their interaction. An analysis of covariance was carried out in order to correct the incidental recall scores for differing numbers of trials in intentional learning; none of the residual treatment *F*s or interaction *F*s were significant. Incidental learning has thus been shown to be unaffected by distraction introduced either during the course of intentional learning or during the test of incidental recall.

EXPERIMENT II

The purpose of this experiment was to study the effect of a distraction that arises in the learning situation itself. To create such an arrangement, the learning materials used in Experiment I (colored, girls' names) were paired with the names of different colors.[5] It was predicted that such paired-associate learning would by its very nature be more difficult than either colored names of girls paired with non-color words or uncolored names of girls paired with color names. Furthermore, it was predicted that the incidental learning of the association of the irrelevant colors with the girls' names would be less in the first situation described than in the second. It seemed reasonable to expect that such incidental learning must suffer as a consequence of its incongruence with the intentional learning. Such a prediction seems to stem either from a common-sense interpretation that incongruence hurts learning, or from the belief that a funnelling of attention away from the irrelevant material may be necessary in order for intentional learning to occur.[6]

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[4] M. H. Glanzer and S. C. Peters, Re-examination of the serial position effect, *J. exp. Psychol.*, 64, 1962, 258-262.

[5] This experimental arrangement is based on an hypothetical experiment described by B. J. Underwood, *Psychological Research*, 1957, 165.

Method: (1) Subjects. In all, 45 introductory psychology students served as *Ss*, 15 in each of the three experimental groups. In addition, 10 *Ss* from Group I and six from Group II had to be dismissed when they failed to learn the material within 20 trials. Learning materials for the groups were: Group I, girls' names on colored background paired with conflicting color names; Group II, girls' names on colored backgrounds paired with adjectives; Group III, girls' names on plain background paired with color names.

TABLE II

MEAN SCORES FOR INTENTIONAL LEARNING AND FOR INCIDENTAL RECALL (Experiment II)

Group	Intentional learning: trials to criterion	Incidental learning: number recalled
I	15.00	4.00
II	17.53	6.00
III	14.33	—

(2) Apparatus and materials. Ten of the girls' names from Experiment I were exposed in five different sequences on a long-arm Lafayette memory-drum and served as stimulus-items in all three groups. For Group I, these names were typed against a 2¼ by ¼ in. rectangle of color (irrelevant material) and were paired with conflicting color names (*e.g.* 'Ingrid' against a red background, paired with the word 'yellow'). Group II learned the same names on colored background, paired with adjectives (*e.g.* 'Ingrid' against a red background, paired with the word 'hateful'). Group III had the same pairs as Group I, but without any irrelevant colors.

(3) Experimental procedure. Standard paired-associate learning procedure was used, with pairs exposed at a 4-sec. rate including a 2.2-sec. exposure and an 8-sec. interval. Learning was continued to the criterion of one errorless trial, after which the *Ss* in Groups I and II were shown the chart used in Experiment I and asked to match colors and names for the test of incidental recall.

Results and discussion. Table II shows the mean trials to criterion and mean errors in the three groups and incidental recall scores for the two groups (I and II) for which the incidental recall test was appropriate. That Group I, for whom irrelevant colors differed from the relevant color words, took fewer trials (though not significantly so) to learn than Group II is contrary to what had been expected. Analysis of variance of both the trials to criterion and the error data revealed no significance among the three groups on intentional learning. In evaluating these data, it must be recalled that some selection of the *S*s has taken place: 10 having been dropped from Group I, six from Group II.

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[6] D. H. Kausler and E. P. Trapp, Motivation and cue utilization in intentional and incidental learning, *Psychol. Rev.*, 67, 1960, 373-379.

With intentional learning suffering no effect in Group I from the potential distraction of the irrelevant colors, the incidental recall-scores themselves showed a slight dimunition; this was not unexpected and would seem to be a consequence of increased attention to intentional learning caused by the distraction posed by the incidental material. A *t*-test comparing the scores in Groups I and II (4 and 6 colors recalled, respectively) indicated this small difference to be significant ($t_{28\ \mathrm{df}} = 2.82$; $p < 0.01$). With a covariance analysis correcting incidental scores for the small difference in intentional learning scores, the significance of this difference was wiped out.

In evaluating these incidental learning scores, the selection of the *S*s must again be mentioned. It seems quite possible that the 10 *S*s dropped were those having difficulty with intentional learning because of interference from the irrelevant material. Put otherwise, these may have been the *S*s who were experiencing conflict between the relevant and irrelevant material and were unable to concentrate on the relevant by attempting to ignore the irrelevant colors. On the other hand, Group II also represents some selection of *S*s, six having been dropped; why these *S*s failed to learn in 20 trails is not clear. Both groups, however, represent *S*s selected from among those appearing for the experiment; in Group I these may be the better concentrators, while the characteristics of the *S*s in Group II are not so easily identified.

Discussion. The present study has demonstrated in the first experiment reported that incidental learning, in contrast to intentional learning, remains unaffected by an external distraction, introduced either during exposure to the material or during the test of its recall. The second experiment found that a source of internal distraction had no effect on intentional learning and only a slight effect on incidental learning. These findings are suggestive of differences in the nature of intentional and incidental learning.

There seem to be two possible explanations for the apparent immunity of incidental learning to either external or internal sources of distraction. First, the lack of an effect of distraction on incidental learning may be due to the fact that this is the kind of learning that occurs under the most distracting of conditions—a distraction inherent in the type of learning—with attention directed toward the learning of other materials. Since incidental learning typically occurs under such conditions, the imposition of further distraction does not substantially reduce the amount learned. Its imposition during the test of recall, of course, makes the recall conditions more similar to original learning; on the other hand, where distraction was present during learning but was absent during recall, this alteration of conditions had no more effect than where distraction was present during recall but not during learning. In other words, both the actual learning of incidental material and the performance involved in the recall of the material have been shown to be unaffected by distraction.

A second, possibly related, explanation might emphasize the high degree of selectivity of incidental learning. Incidental learning, under the conditions studied is likely to occur for stimuli having, for a variety of possibly idiosyncratic reasons, some initial associative possibilities. Such initial differentiation of pairs may reflect such strong associations as to be resistant to any type of interference. This, of course, is the kind of explanation applied by Postman and Adams to the finding that exercise, though strengthening intentional learning, does not increase the amount of incidental learning and to the finding that incidental learning is subject to less intra-serial interference than is intentional.[7] This study has further demonstrated that incidental learning is less subject to interference from factors extraneous to the learning situation as well as to distraction built into the learning itself.

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[7] Leo Postman and P. A. Adams, Studies in incidental learning: VI. Intraserial interference, *J. exp. Psychol.*, 54, 1957, 153-168; VII. Effects of frequency of exercise and length of list, *ibid.*, 56, 1958, 86-95.

AN ACQUIRED DRIVE BASED ON CONFLICT

By R. A. CHAMPION and GRETA ALLEN, University of Sydney

An acquired drive has been defined as a learned response which has the same effect on other responses as deprivation and strong stimulation.[1] Although the hypothetical response of fear may be a component of the innate response to noxious stimuli, it has also been described as an acquired drive because it may come to be elicited by a previously neutral stimulus through learning, and because other responses are then activated by this stimulus.[2] The technique used to study the acquisition of fear has been that of classical conditioning; a neutral stimulus such as a tone is repeatedly presented in conjunction with a noxious stimulus like an electric shock, the conditioned and unconditioned responses being taken as fear and pain, respectively.

It has been suggested that frustrative nonreward and conflict may act as sources of acquirable drive, just as noxious stimulation does in the case of fear.[3] Wagner has shown that neutral stimuli paired with frustration in rats take on reinforcing and energizing properties;[4] frustration being defined as the withholding of a previously given reward. The following study was designed to discover if a neutral stimulus paired with conflict will also come to elicit a response with drive-properties, conflict being defined as the simultaneous presentation of two or more stimuli eliciting incompatible responses. In other words, the aim was to determine if conflict, like noxious stimuli and frustration, has drive effects which can come to be elicited by a neutral stimulus through classical conditioning. Care was taken to ensure that the method of generating conflict did not involve noxious stimuli, so that any evidence for acquired drive could not simply be interpreted in terms of fear. The technique used to detect the acquired-drive effects was that of the probe stimulus, it being

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* Received for publication August 24, 1964.

[1] J. S. Brown, *The Motivation of Behavior,* 1961, 140, 144.

[2] Brown, *op. cit.,* 144.

[3] Abram Amsel, The role of frustrative nonreward in non-continuous reward situations, *Psychol. Bull.,* 55, 1958, 102-119; N. E. Miller, Liberalization of basic S-R concepts: Extensions to conflict behavior, motivation, and social learning, in Sigmund Koch (ed.), *Psychology: A Study of a Science,* 2, 1959, 198-212.

[4] A. R. Wagner, Conditioned frustration as a learned drive, *J. exp. Psychol.,* 66, 1963, 142-148.

assumed that a response elicited in the presence of the acquired drive would be potentiated by it.[5]

Method: (1) Subjects. The *Ss* were 30 undergraduates and 10 office workers, allocated to an experimental group (9 men, 11 women) and a control group (10 men, 10 women).

(2) Apparatus. The *S* was placed in a sound-reduced room separate from that containing *E* and the main apparatus. Words were projected on a 23 × 17-in. milk-glass screen 5 ft. in front of *S* at eye level, in colored letters $1\frac{1}{2}$ in. high on a white background. The *CS* was a 2000 ~, 85db. tone of 200 m.sec. duration delivered to *S* through headphones. The stimuli used to produce conflict consisted of the following words in color: 'red' in blue, 'yellow' in blue, 'blue' in red, 'yellow' in red, 'red' in yellow, and 'blue' in yellow. Three other stimuli were used for habituation and as probes: 'red' in red, 'blue' in blue, and 'yellow' in yellow. Each word was exposed for 200 m.sec. The reaction-time of *S* was measured with an electronic chronoscope. It started with the onset of each word presentation and was stopped by pressure on a 3 × 1 in. horizontal panel on the left arm-rest of *S*'s chair.

(3) Procedure. The *S* was instructed to press the panel and say each word aloud as soon as it appeared on the screen, having been warned that a tone would be heard from time to time. The first trial for all *Ss* was a pretest probe, consisting of a presentation of the stimulus to be used as *CS* (tone) followed 3.5 sec. later by 'red' in red. All the *Ss* were then given 79 presentations of the habituational stimuli, that is, 'red' in red, 'blue' in blue, or 'yellow' in yellow, in random order with the restriction that no one word occur more than three times in succession. For the members of the experimental group a training trial was inserted after 10, 23, 34, 44, 57, and 69 habituational trials; each training trial consisted of a presentation of a tone (*CS*) followed by the conflict stimulus (*UCS*) after a 0.5-sec. interval. The *Ss* of the control group were presented with the conflict stimulus alone at the same six stages, and they also had six presentations of the tone, but the latter stimuli occurred at random in the whole series of trials with the restriction that tone and conflict stimulus never occur on succeeding trials. Finally, all *Ss* were given a post-test, which was identical with the probe-trial used as a pretest. It consisted of the tone followed by the neutral word 'red' in red. The intertrial interval was 15, 20, or 25 sec. according to a prearranged random schedule. No ready signal was given at any stage.

Results. The *Ss* were arranged in 20 matched pairs on the basis of their mean performance on the first 10 habituational trials. The results of the probe-trials before and after training are shown in Fig. 1, the measure being mean latency of panel-pressing to the stimulus-word. Both the experimental and control groups showed a significant decrease in mean response latency ($t = 4.69$ and 3.26 respectively, $p < 0.01$ for 19 *df*), but the change was significantly greater in the experimental group ($t =$

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[5] J. S. Brown, H. I. Kalish, and I. E. Farber, Conditioned fear as revealed by magnitude of startle response to an auditory stimulus, *J. exp. Psychol.*, 41, 1951, 317-328. Wagner, *op. cit.*, 142-148.

2.26, $p < 0.05$ for 19 *df*). A comparison of the mean scores for experimental and control groups on each of the two tests showed that while the difference was not significant before training ($t = 1.38$, $p > 0.10$ for 19 *df*), the superiority of the experimental group was statistically significant after training ($t = 2.14$, $p < 0.05$ for 19 *df*); the greater absolute difference between means on the pretest was offset by greater variability in performance at that stage.

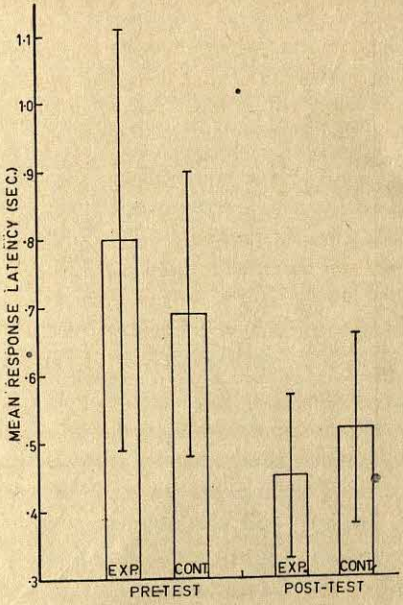

FIG. 1. MEAN RESPONSE LATENCIES FOR THE EXPERIMENTAL AND CONTROL GROUPS ON THE PRE- AND POST-TESTS

The vertical lines show the variability of the scores as ± 1 *SD*).

Performance on conflict trials is represented in Fig. 2; these trials involved the presentation of tone and conflict stimulus for the experimental group (training) and of the conflict stimulus alone for the control group. The mean reduction in response latency from the first to the last conflict trial was significantly greater in the experimental group ($t = 2.72$, $p < 0.02$ for 19 *df*).

Discussion. The mean latency of the panel-pressing response to the neutral word ('red' in red) decreased more in the experimental than the control group from pretest to post-test probe-trials, suggesting that the tone (*CS*) had come to elicit a response with energizing properties, through being paired with conflict stimuli (*UCS*s) for the experimental *S*s. The temporal interval between the tone (*CS*) and the neutral word (probe) was set at 3.5 sec. because of evidence that the latency of the energizing effect of fear is of that order,[6] and on the assumption that the analogous property of an acquired drive based on conflict would have a similar latency. It was therefore not anticipated that the experimental and control groups would necessarily show differential performance during training, the interval of 0.5 sec. between tone (*CS*) and conflict word (*UCS*) being chosen because of its general efficiency in promoting conditioning. The obtained difference (Fig. 2) may simply reflect the increasing effectiveness of the tone as a warning signal, which was available only to the experimental group, and this interpretation is supported by the improved performance of the control group from pre- to post-test, with tone present. If it be allowed, on the other hand, that the conflict words (*UCS*) acted as a form of probe-stimulus, then the training data are compatible with the interpretation given to the test-data, and the results for the experimental group suggest that a stronger effect would have been achieved with prolonged training. In particu-

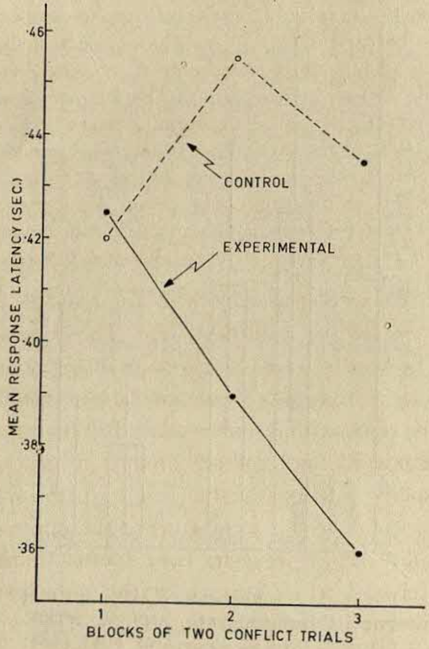

FIG. 2. PERFORMANCE ON CONFLICT TRIALS

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[6] R. A. Champion, The latency of the conditioned fear response, this JOURNAL, 77, 1964, 75-83.

lar, the rationale of the technique of the probe-stimulus implies in this context that the latency of the energizing effect of an acquired drive based on conflict may be as short as 850 m.sec. (*CS* probe-stimulus interval plus latency of panel-pressing; see *Fig.* 2, third block of trials for experimental group).

It may be suggested that there is an obvious and preferable alternative to an explanation in terms of the hypothesis under test. It may simply be said that the *S*s have learned techniques appropriate to conflict situations, so that there is transfer of training to the present case, and that the superior performance of the experimental group is merely due to the fact that the tone serves as warning of the impending conflict. To be more specific, it may be argued that there is generated in the *S*s of the present study a set to attend only to the letters and not the color on conflict trials, and that this set becomes activated by the tone. Following the example of Brown, Kalish, and Farber,[7] this suggestion is taken as describing a possible mechanism for the predicted energizing effect rather than an alternative explanation of it. Finally, it may be noted that evidence for an acquired drive based on conflict suggests a way in which conflict may persist long after the actual conflict situation has passed, and points to a factor which may be as useful as fear in explaining the origin and persistence of many abnormal behaviors.

Summary. Forty *S*s were presented with color names printed in letters either of the named hue or a different hue. The *S* pressed a panel and read the word. The words served as probe or habituation and as conflict stimuli, respectively. A tone was presented 3.5 sec. before a probe stimulus as pre- and post-tests, with 79 intervening habituational trials, *S*s being allocated to experimental and control groups in matched pairs on the basis of mean response latency over the first 10 trials. At six prearranged points, the experimental groups were given a training trial, consisting of the tone (*CS*) followed 0.5 sec. later by a conflict stimulus (*UCS*); the control group received the *UCS* alone at the same points and the *CS* alone an equal number of times at other stages. The experimental group showed a greater reduction in response latency for panel pressing from pre- to post-test, suggesting the existence of an acquired drive based on conflict.

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[7] Brown, Kalish, and Farber, *op. cit.,* 326.

SCALE OF WEIGHT FROM FRACTIONATION AND MULTIPLICATION DATA

RAM G. CHATTERJEA, Brown University, and BANDANA DASGUPTA, D.H. Training College, Calcutta

Since the publication of a scale for weight by Harper and Stevens, several studies have been reported with less than complete agreement about the correct form of the scale.[1] Most of these studies have used either fractionation or a constant-sum method. To rule out certain sources of bias in these methods, it seemed desirable to use the method of multiplication as well. In this paper, we shall compare *VEG* scales constructed from halving and doubling procedures.

The study by Guilford and Dingman used a method calling for ratio judgments.[2] Two standard weights were used and each standard was presented with all comparison weights, the set ordered roughly in geometric progression. These authors found that subjective weight was a power function of physical weight with an exponent of 1.2. Baker and Dudek also used ratio judgments, in particular, the constant-sum version of this method, and showed that scales constructed by different methods did not agree.[3] Warren and Warren pointed to the disturbing influence of the size-weight illusion.[4] A study using fractionation with a varying presentation of the comparison weights produced a *VEG* scale differing slightly from earlier ones.[5] Canter and Hirsh used several methods and concluded that fractionation, certainly if used alone, is not a suitable method for constructing a weight scale.[6] The recent discussions of Stevens and of Torgersen clarify some of the procedural problems involved and suggest the use of a combination of methods as employed in this paper.[7]

Method: Subjects. Thirty students, 17 men and 13 women, graduate students in

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* Received for publication July 10, 1963. This paper is based on the second author's Master in Science thesis submitted to Calcutta University, 1962, and has been written in its present form by the first author.

[1] R. S. Harper and S. S. Stevens, A psychological scale of weight and a formula for its derivation, this JOURNAL, 61, 1948, 343-351.

[2] J. P. Guilford and H. F. Dingman, A validation study of ratio judgment methods, this JOURNAL, 67, 1954, 395-410.

[3] K. E. Baker and F. J. Dudek, Weight scales from ratio judgments and comparisons of existent weight scales, *J. exp. Psychol.*, 50, 1955, 293-308.

[4] R. M. Warren and R. P. Warren, Effect of relative volume of standard and comparison object on half heaviness judgment, this JOURNAL, 69, 1956, 643.

[5] R. G. Chatterjea and Ramanath Kundu, Construction of a psychological scale of weight from fractionation data, *Brit. J. Psychol.*, 51, 1960, 61-65.

[6] R. R. Canter and Jerry Hirsh, An experimental comparison of several psychological scales of weight, this JOURNAL, 68, 1955, 649.

[7] S. S. Stevens, Problems and methods of psychophysics, *Psychol. Bull.*, 54, 1958, 177-196; W. S. Torgerson, *Theory and Methods of Scaling*, 1962, 100.

psychology, participated in the doubling experiment, while 10 others, 7 men and 3 women, acted as *S*s in the halving experiment. The *S*s ranged in age from 21–23 yr.

Materials. Weights were constructed from pharmacist's pill boxes of equal shape and size. The set of weights was made up of five standard stimuli (40, 80, 120, 160, and 200 gm.). The comparison-weights for each standard weight are shown in Table I.

Procedure. Basically the procedure was the same as reported in an earlier paper.[8] In the doubling experiment, the comparison-weights were presented first in a haphazard manner and again serially. The haphazard presentation consisted of presenting to the *S*s one standard weight followed by all the comparison-weights listed in Table I. Thus a weight of 40 gm. would be followed by comparison-weights 60, 80, through 440 gm. When judgment of a weight twice as heavy was obtained with 40 gm., the next standard weight would be presented along with the other comparison-weights, leaving aside the one selected by the *S*. The sequence continued with other standard weights. In the serial presentation, each standard was followed by four comparison-weights only. Thus the 40 gm. standard was followed by comparison-weights of 60, 80, 100, and 120 gm. In the fractionation experiment the mode of presentation was restricted to the serial presentation as described above.

TABLE I

STANDARD AND COMPARISON WEIGHTS

Standard (in gm.)	Comparison weights (in gm.)							
	multiplication				fractionation			
40	60,	80,	100,	120	20,	25,	30,	35
80	140,	160,	180,	200	40,	50,	60,	70
120	220,	240,	260,	280	60,	75,	90,	105
160	300,	320,	340,	360	80,	100,	120,	140
200	380,	400,	420,	440	100,	125,	150,	175

The instructions to the *S* in the doubling experiment were that he was required to find the comparison-weight that "feels twice as heavy as that of the standard" while in the fractionation method he was asked to find the comparison-weight that "feels half as heavy as that of the standard."

Each standard was presented three times to each *S* in irregular order.

Results. The mean of three judgments of each *S* for each standard was determined. Then the median judgment of the 30 *S*s in the doubling experiment, and of the 10 in the fractionation experiment was calculated for final analysis. These are shown in Table II. The median judgments in serial presentation do not differ greatly from those with the haphazard presentation in the multiplication data. Hence, separate treatment of the serial and haphazard presentations was considered unnecessary.[9] The judgments of serial presentation will be considered henceforth.

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[8] Chatterjea and Kundu, *op. cit.,* 61.

[9] This is in line with the argument put forward earlier. Chatterjea and Kundu, *op cit.,* 62.

The psychophysical relation between the judgments of double and half the standard weight appear roughly linear. The *VEG* scale was derived from the data on the double judgment in the following manner. The weight that was judged twice as heavy as 100 gm. was found to be 188.7.

TABLE II

MEDIAN JUDGMENTS FOR EACH STANDARD WEIGHT OF COMPARISON-WEIGHTS THAT ARE TWICE AS HEAVY OR HALF AS HEAVY

Standard (in gm.)	Double judgment		Half judgment
	serial presentation	haphazard presentation	serial presentation
40	71.79	77.34	28.67
80	139.87	140.20	54.33
120	234.67	235.33	81.08
160	312.32	290.77	108.89
200	398.90	360.70	119.67

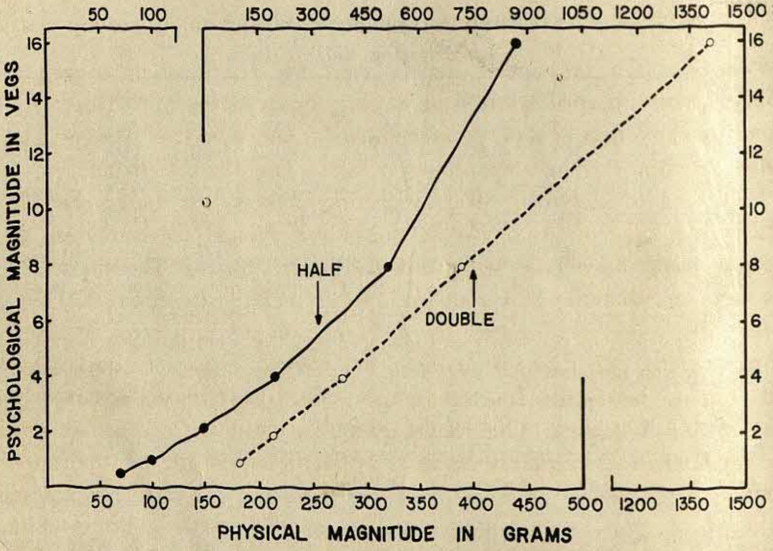

FIG. 1. *VEG* SCALES PLOTTED ON LINEAR COORDINATES
Straight line denotes scale obtained from fractionation data while dotted line shows the scale derived from multiplication data.

Now we define the unit of subjective weight as the weight experienced when one lifts 100 gm., and call it 1 *VEG*. Since a weight of 188.7 gm. is judged twice as heavy, we assign 2 *VEG* to the physical weight of 188.7 gm. In like manner, we can locate the physical weights in grams corresponding to subjective weights in *VEG*s for other values. The *VEG* scale constructed in this way for the double judgment is shown in Fig. 1.

Following a similar procedure, we are able to construct another scale from the fractionation data. Since a physical weight of 66.9 was judged to be half as heavy as that of 100 gm., we assign 0.5 *VEG* to the corresponding to physical weight of 66.9 gm. This scale is also shown in Fig. 1.

The fact that the data fall close to a straight line on a log-log plot indicates that apparent weight in this case is a power function of stimulus-magnitude. The least-square solution for the half and double judgment data yielded the following exponents:

$$V = 1.27\ W^{1.01} \text{ for double judgment}$$
$$V = 0.77\ W^{1.04} \text{ for } \tfrac{1}{2} \text{ judgment}$$

where V is subjective weight in *VEG*s and W is stimulus-magnitude in grams.

The exponents shown above were calculated from the judgments obtained from serial presentation, both for fractionation and multiplication data.

Discussion. The exponents derived from the fractionation as well as from the multiplication method are slightly greater than 1.00. This corresponds to the findings of several studies summarized by Stevens and Galanter, where the slope was shown to have a range of 1.13 to 2.07.[10]

The trend of judgment in fractionation data is just the reverse obtained from multiplication. While the half heaviness judgments are all overestimated, the double judgments are underestimated. This fits to the statement that halving and doubling operate in opposite direction.[11] Empirical observation revealed no sex differences in the judgments obtained. Moreover, the pill boxes containing the weights being of equal shape and volume, this factor did not have the effect on judgment reported by Warren and Warren.[12] The results, therefore, lead us to conclude that the psychophysical functions obtained by fractionation and multiplication methods are similar.

Summary. The experiment was designed to compare *VEG* scale constructed from fractionation and multiplication judgment data. It was found that the subjective weight is a power function of physical weight whether the fractionation or multiplication method is used.

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[10] S. S. Stevens and E. H. Galanter, Ratio scales and category scales for a dozen perceptual continua, *J. exp. Psychol.*, 54, 1957, 377-411.

[11] Stevens, The measurement of loudness, *J. acoust. Soc. Amer.*, 27, 1955, 821.

[12] Warren and Warren, *op. cit.*, this JOURNAL, 643.

APPARATUS

A DEVICE FOR PACED AND SELF-PACED VISUAL DISPLAYS

By David Arenberg and Walter W. Surwillo, National Institutes of Health

Recent developments in theories and methods in the area of verbal learning require precise timing of the intervals during which various aspects of the material are displayed and of the period during which *O* is supposed to respond. For some purposes it is also desirable to present self-paced trials in which the timing depends upon *O*'s performance on each item. Self-paced trials may be distributed throughout the entire acquisition-sequence for measures of intermediate learning as well as for measures of final learning and retention.

Self-paced trials and precise timing of intervals are provided by card-changing devices which are commercially available, but such devices introduce some minimal interval of time between successive items to allow the material to be changed.[1] This interval of time is undesirable for many experimental purposes, especially if inter-item interval interacts with other independent variables. A system which uses a motion-picture projector (with synchronous motor) and films with appropriate numbers of frames to achieve precise timing of the material has proved quite satisfactory for some purposes in this laboratory. Although no interval of time between successive items is required for this system, it is not feasible to use it for self-paced trials.

For studies of paired-associate learning, a device was designed which permits the presentation of self-paced trials at any time during acquisition and which provides for precise timing of the intervals during which the material is displayed. No minimal interval is required between successive items, and the sequence of items can be readily modified. The device consists of three basic units: slide-projectors, cam-timer, and control-panel.

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[1] The Cardmaster (Hunter Mfg. Co., Iowa City), for example, is a card-changing device with two shutters. Although it has considerable operational flexibility, nearly 2 sec. are required for each change of cards during which time both shutters are closed.

Projectors. Projectors are used in pairs with the number of pairs depending upon the length of the unrepeated sequence of items during acquisition and the capacity of the projectors.[2] One of each pair projects the stimulus on the screen; the other displays the stimulus together with the appropriate response. An extra projector is used to project the stimuli for self-paced trials; this same projector also serves to illuminate the screen when no material is being projected. The projector used in the self-paced trials is operated manually by a remote switch.

Cam-timer. The cam-timer programs all events that occur during an acquisition-sequence, and the time required for a single rotation of the cam-shaft is the duration of a single item. Cams operate their respective microswitches which, in turn, light the lamps in the projectors or trigger the mechanism (in the projectors) which changes slides. As an example, suppose *E* wants the stimulus exposed for 1.00 sec., a blank interval of 1.00 sec., and finally the stimulus with its response exposed for 2.00 sec. One cam is set to hold its microswitch closed for 1.00 sec., and the latter is connected in series with the lamp of the projector (P_1) that contains slides of a stimulus. A second cam is also set for 1.00 sec. and is positioned on the cam-shaft in such a way that the beginning of this 1.00-sec. interval coincides with the end of the previous one. The microswitch is connected in series with the lamp in the extra projector (P_2) which contains a blank slide. A third cam is set for 2.00 sec. and is positioned on the cam-shaft so that the beginning of this interval coincides with the end of the previous one. The microswitch governed by this cam is connected in series with the lamp of the projector (P_3) that contains slides of a stimulus with its response. The cam-shaft is driven by a synchronous motor at the rate of one revolution every 4.00 sec. Two other cams are set to close their microswitches for a brief interval of time, and the latter are connected to the mechanisms that change the slides in P_1 and P_3. These cams are so positioned in the cycle that the slides in a projector are changed when its lamp is extinguished.[3]

Control-panel. The control-panel serves several control functions. One switch provides power to the motor of the cam-timer and is closed during paced trials and opened at a specific phase of the cycle to permit presentation of self-paced trials. For *E* to know which projector-lamp is lighted without the necessity of looking at the screen, a pilot-lamp for each projector is mounted on the control-panel. Two-position toggle switches are mounted above the pilot-lamps corresponding to P_1 and P_3. Only if these switches are in the 'on' position are the lamps in the projectors and the corresponding pilot-lamps lighted when the appropriate micro-switch of the cam-timer is closed. The 'off' position defeats the operation of the cam-timer. Similar switches with 'on' and 'off' positions serve the same function for the slide-cycle mechanism of each projector. Another 'two-position' switch is also mounted on the panel for each projector. When the latter switch is in the 'run' position, the cam-timer is connected for normal operation; in the 'calibrate' position, the cam-timer is connected to a 0.01-sec. chronoscope which permits *E* to check or adjust the settings of the cams.

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[2] It is most convenient to use projectors with circular slide-trays of large capacity and with a shorting plug in the circuit of the lamp to permit easy access to the wiring. The Carousel Model 570 (Eastman Kodak Co., Rochester, N. Y.) which is used in this laboratory is no longer available, but the Carousel Model AV-900 can be used instead. Zoom lenses are used in all projectors to facilitate matching the size of the projected images.

[3] Two different cam-timers have been used. One was assembled in this laboratory from components; the other was obtained as a complete unit from Meylan Stopwatch Corp., New York. To improve the precision of the latter, the original motor and gear-assembly was replaced by a heavier synchronous motor that was directly coupled to the cam-shaft. Unfortunately, the increased precision is gained at the cost of a loss in flexibility because the motor must be changed to change the duration of paced items. The major advantage of the Meylan unit is the manner in which cams are connected to the cam-shaft. This feature permits cams for two contiguous intervals to be easily and accurately synchronized. Other types of cam-timers are available (for example, Bristol units from Philadelphia Electronics). A unit with several extra cams and switches should be selected so that delays are not encountered when a component needs to be replaced.

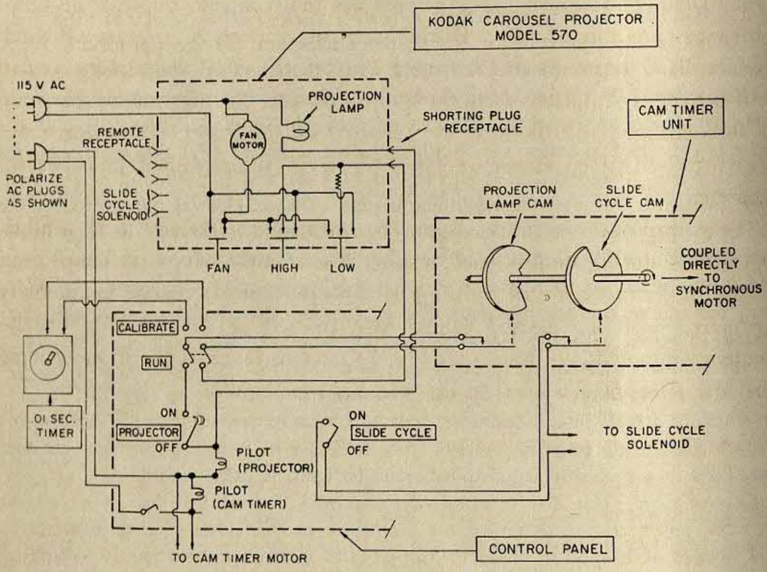

FIG. 1. CIRCUIT FOR CONTROL OF ONE PROJECTOR
The heavy dashed lines designate the three basic units of which the device is composed.

Fig. 1 shows the switches and the manner in which they are wired as well as the connections among the three basic units. Note that the circuit is shown for only one projector. The wiring for the others is identical with the exception of two differences in P_2, the projector used for self-paced trials. Since P_2 is not used to project material during paced trials, the circuitry for changing the slide is omitted. The toggle switch which is mounted on the control-panel, and which controls the lamp in the projector, has a third position that is wired so that the lamp in P_2 remains lighted during self-paced trials, regardless of the position of the cam in the cam-timer. To extend the number of paced items that can be handled by the device without changing slide-trays, an additional pair of projectors is used and extra switches are added to the control-panel to permit a shift in control operations from one pair of projectors to the other. All projectors are conveniently arranged on open shelves which permit access to both front and rear of the projectors.

Operation of the device is quite simple. For paced items, the switch which supplies power to the motor of the cam-timer is turned 'on' to initiate, and 'off' to terminate, a sequence of items. For self-paced items, this switch is turned to the 'off' position. In addition, the switch that controls the lamp in P_2 is moved to the position which leaves the lamp illuminated continuously, and the switches that control lamps in the other projectors are turned 'off'. During self-paced trials *E* presses a switch as soon as *O* responds to each item. This switch is on the remote-control unit which is supplied with the projectors and by pressing it the slide in P_2 is changed and the next item appears on the screen. Transition from paced to self-paced items and *vice versa* is accomplished quickly and smoothly.

Two empirical findings should be mentioned. After 1,650 'on-off' operations during preliminary testing, the standard type of lamp used in the projectors was still lighting with no perceptible change in intensity compared with an unused lamp. Also by setting the intensity of the lamps in P_1 and P_3 to 'low' (see Fig. 1), and in P_2 to 'high,' it was found that the after images seen on the screen were neglible.

THE PRIMATE BEHAVIOR LABORATORY OF BROWN UNIVERSITY

By ALLAN M. SCHRIER, Brown University

The Primate Behavior Laboratory, an addition to the Walter S. Hunter Laboratory of Psychology, was completed during 1964.[1] It comprises the front half (about 5,000 sq. ft.) of the fourth floor of Hunter Laboratory. The remaining half is occupied by ventilating and elevator machinery for the rest of Hunter Laboratory and a strip of flat roof around the perimeter, on which platforms have been built for storing extra monkey cages.

Roughly a third of the Primate Laboratory is research space, another third animal-colony rooms, and the rest is primarily corridors, offices, and shop and work-rooms. Before describing these three areas, it seems worthwhile to mention some features of construction common to them.

All internal partitions are Lelite block, a lightweight material similar to cinder block. Outside of the colony rooms, the concrete slab floor is covered with rubber tile. All rooms are air-conditioned and there is also some humidity-control, primarily in the direction of an increase to about 50% during the heating season. None of the air is recirculated by the ventilating system. Except for the colony rooms, temperature and humidity are controlled by zones. (In other areas of Hunter Laboratory, temperature and humidity have often been uneven from room to room as a result of zone control. This has not been as great a problem in the Primate Laboratory, perhaps in part because the zones are smaller.) To reduce the transmission of noise, there are baffles in the ventilating ducts between rooms, doors located at three places in the corridors, weatherstripping around the door of each room, and, except for the colony rooms, Fiberglas acoustic tile on the ceilings.

Research space. Research is conducted in some of the rooms opening off corridors A-4 and D-4 (see Fig. 1). Corridor D-4: Rooms 431 and 435 are used for dis-

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* Construction of this laboratory was supported by a matching grant (RC-1125) from the Health Research Facilities Branch, DRFR, National Institutes of Health, U.S. Public Health Service. The equipping of the laboratory was supported by research grant MH-07136 from the National Institute of Mental Health, U.S. Public Health Service. Harold Schlosberg, Fred Stollnitz, and Morris L. Povar were of great help during the planning and construction of the laboratory.

[1] Harold Schlosberg, The psychological laboratory of Brown University, this JOURNAL, 71, 1958, 768-776.

criminative-learning. Each contains a Wisconsin-type apparatus (WGTA).[2] Room 433 serves this area as a storage place for WGTA discriminanda, spare parts for the WGTAs, data sheets, food pellets, etc. Corridor A-4: Room 465 is used for work on visual psychophysics. The monkeys are tested in a cubicle 4.5 ft. wide, 6.5 ft. long, and 7.5 ft. high, the remainder of the room being devoted to control, recording, and optical apparatus. Room 467 is designed for electrophysiological research; it contains a large electrically-shielded, sound-resistant enclosure and a laboratory counter and sink. Room 472 is a suite of six small rooms, five of which are used for free-operant or discriminative-learning research involving automatic programming and recording. Room 472A, for example, is large enough for two operant chambers and enclosures for large macaques, plus the necessary control and recording equipment on three relay racks. The sixth room, 472C, is used for storage of electrical supplies and equipment and as an electrical shop. The sink at the end of the Room 472 corridor has proved quite useful as a source of water for the rooms nearby for such purposes as rinsing of equipment not contaminated by animal wastes.

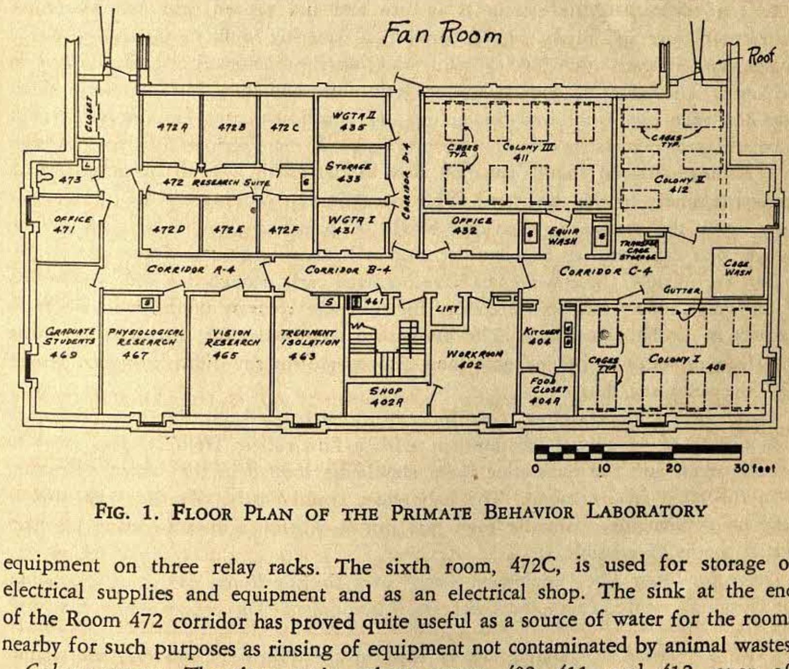

FIG. 1. FLOOR PLAN OF THE PRIMATE BEHAVIOR LABORATORY

Colony rooms. The three main colony rooms, 408, 411, and 412, open off corridor C-4. Each room has independent control of temperature and humidity and has at least 12 changes of air per hour. The colony rooms are very brightly illuminated by numerous waterproof fluorescent fixtures, controlled by a separate time switch for each room. The walls of these rooms were painted and then coated with a waterproof finish. The concrete floors were treated with a waterproof sealer.

The cage layout in Rooms 408 and 411 is as indicated in the floor plan; each cage rack holds six macaques in individual cages, three above and three below. The cage layout in Room 412, which is used for small monkeys, is roughly as indicated in the floor plan, but varies depending on the size of the cage racks that happen to be in use. The total colony capacity is about 132 medium-size monkeys, such as rhesus macaques *(Macaca mulatta)*. This figure could be increased by about 50 per cent for small monkeys, such as squirrel monkeys *(Saimiri sciureus)*.

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[2] A. M. Schrier, A modified version of the Wisconsin General Test Apparatus, *J. Psychol.*, 52, 1961, 193-200.

A single sloping refuse pan serves the upper cages in each rack, and another pan the lower cages. The animal droppings are flushed from the cage-rack pans into gutters several times a day by the caretakers using hoses. The gutters in each of the rooms flush automatically at least every 2 hr. and the contents pass into drains which also flush automatically every few hours. Automatic flush-type drains are the only means of providing full protection against odors from drains in regular use in animal quarters.

An automatic watering system is in use and has proved quite successful. A system of copper and plastic piping connects a watering fount in each cage with a storage tank in each room.[3] A constant water level is automatically maintained in each tank. The watering founts develop leaks now and then, but the water flows from the cage pan right into the gutters. (If flush-type pans are not used, such automatic watering systems may not be practical.) A more serious problem has been an occasional clogged fount. The best way to deal with it is to have the founts checked regularly by the caretakers. Otherwise, the first warning of this trouble will come from the monkey's behavior; it will stop eating dry foods, such as the biscuits that constitute its regular diet, and will show a general depression in behavior. Fortunately, these symptoms have occurred early enough that only restoration of the water supply has been necessary for complete recovery of the monkeys from the effects of the dehydration. The savings in time and effort involved in filling and cleaning watering bottles more than compensate for any difficulties encountered with the automatic devices.

Room 463 is a quarantine room for new or sick monkeys. The most desirable arrangement of an animal laboratory is with a flow-pattern from "clean" areas to "dirty" areas, and the quarantine room should be located in the "dirty" direction from the main colony rooms.[4] The Laboratory could not be planned with such a desirable system; the quarantine room was put at as great a distance from the main colony rooms as possible.

Other facilities. The "equipment wash" area outside Room 411 has large sinks for cleaning transfer cages and other small equipment. The cage-washing machine, in Corridor C-4, is large enough to allow a whole rack of monkey cages to be rolled into it; it saves many hours of hand scrubbing. Room 402 is a general-purpose work room, containing a desk calculator, an IBM card punch, locker space, a small desk for undergraduates and one for a full-time research technician. The shop, 402A, is used for minor repairs and construction, and does not replace the much larger departmental shops in the basement of Hunter Laboratory. Room 404 is a food preparation room with sinks, a refrigerator-freezer unit, and cabinets for general storage, and 404A is used primarily to store bags of monkey biscuits. Rooms 432, 469, and 471 now provide office space for two research associates and three graduate students. Any of these rooms, but particularly 432, may be used for research in the future. Room 461 is a janitor's closet, and 473 is a toilet.

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[3] The low pressure watering founts used in the Primate Laboratory are manufactured by H. W. Hart Manufacturing Co., Inc., P.O. Box 3456, Glendale 1, California. The "Harada Valves" of Hardco Scientific Corp., 6811 Grace Ave., Cincinnati, Ohio 45227, are similar to the Hart founts. The "Lixit" valve, made by ATCO Manufacturing Co., 1106 Hardman Ave., Napa, California, is used in some laboratories and operates under normal line pressure.

[4] *Guide for Laboratory Animal Facilities and Care* (Rev. 1965), U.S. PHS Publication No. 1024, purchasable from Supt. of Documents, U.S. Government Printing Office, Washington, D.C. 20402, provides much information that would be valuable in planning both primate and nonprimate facilities; it also contains a valuable selected bibliography on the care and management of laboratory animals. Also see *Standards for the Breeding, Care, and Management of Laboratory Primates: I.* Macaca mulatta, 1965, prepared by the Institute of Laboratory Animal Resources, NAS-NRC, Washington, D.C. 20418.

After over a year of use of the Primate Laboratory, we have only one major criticism. Noise is not controlled as well as we would like. Although the steps taken to reduce noise, which were mentioned earlier, appear to have been effective, there still appears to be transmission of sound directly through the lightweight blocks used for partitions and, perhaps most important, by the single slab that constitutes the floor of the Laboratory. In almost all other respects, we have found that it is an excellent place for behavioral research on monkeys.

A SYSTEM FOR RECORDING THE ORDER AND DURATION OF AN ANIMAL'S PATH IN A RUNWAY

J. M. BENJAMIN, JR., R. M. RAUCH, Biophysical Electronics,
S. J. HARRIS, Wells College, and SOLOMON WEINSTOCK, Brooklyn College

The apparatus described here was developed to investigate the behavior of rats in straight alleyways. More specifically, the equipment represents a semi-automated system for obtaining a record of the animal's path in running down an alleyway and also a record of the duration of successive locomotor movements.

The apparatus involves a 20-channel 'contact' recorder in which one of the animal's rear paws serves to key a thyratron circuit when the paw makes contact with the runway floor. This is accomplished by constructing the runway floor of tinned copper bus bars embedded in wood at ⅛ in. intervals. Alternate bars are connected to grid and ground of a thyratron circuit. When the animal's left rear paw, which has been coated with silver paint, makes contact with adjacent bars, a thyratron circuit is completed. Resistance in the animal's other paws is too high to complete the circuit.

To obtain a record of the position of the animal's paw during each contact, the runway is divided into a checkerboard of ¾ in. by 2 in. sections. Contact within one of these sections operates two elements of a 20-element paper tape recorder. One of these elements codes the position of the paw in the length and the other in the width of the alleyway. Thus, the animal's path may be plotted from the records of the location of his paw on successive steps.

In addition to activating the paper tape recorder, contact with the floor causes a 1000 ~ signal to be recorded on one channel of a two channel magnetic tape recorder. During intervals between contacts the 1000 ~ signal is recorded in the second channel. By playing back each of the channels through an electronic counter with a print-out, the duration in msec. of each successive contact and of each stride of the animal's paw is recovered.

A variety of response-characteristics can be analyzed from the data obtained. Positional data can be used to evaluate between trial-variability in an animal's path and variability about a straight line from start to the goal. Duration of contact and stride may be used to detect momentary pauses in the animal's run or variability of response-times within and between trials.

Recording system. A block diagram of the recording and playback systems is given in Fig. 1.[1]

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* From the Behavioral Laboratory of Lehigh University. Funds were provided by a research grant, M-2286, from the National Institute of Mental Health, and by a grant from the Institute of Research, Lehigh University.

[1] More detailed circuit diagrams may be obtained by writing to the senior author at Biophysical Electronics.

A 1000 ~ oscillator drives both the thyratrons and the time-recorder. Power for the thyratrons is supplied by the 1000 ~ source through a 75w. power amplifier. This amplifier, using EL34 tubes, develops 120-140 v. The 20 thyratrons are fired by the grids in the runway, and they in turn operate relays capable of switching high voltages.

Each relay activates one electrode of a 20-electrode paper-burn recorder. When a relay is closed it connects to the appropriate electrode of the paper-burn recorder a transformer supplying 2300 v., 60 AC through a 60,000 Ω current-limiting resistor. Thus, a burn mark is made on a moving paper tape which travels between the electrodes and a common ground bar. The paper-burn system was adopted in order that photo-electric readout of the information could be added in the future.

The thyratron-grid is biased beyond its critical voltage by a network of resistors and a 130-v. DC supply. Leads are brought from the runway to the thyratron-grids.

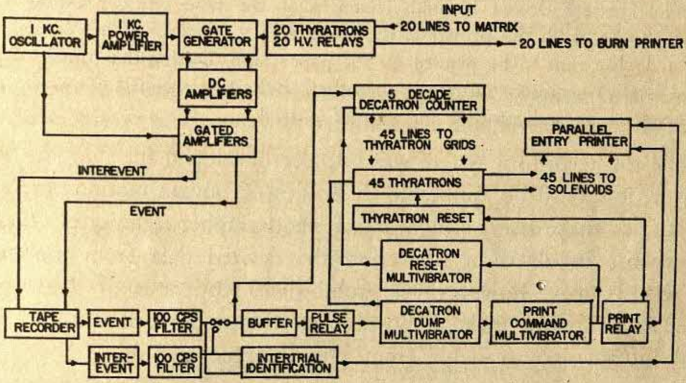

FIG. 1. CIRCUIT DIAGRAM OF THE APPARATUS

The impedance of the grid network is such that 50-100,000 Ω from grid to ground will cause the thyratron to fire. Grid-potentiometers make it possible to correct for individual thyratron-characteristics.

When a thyratron fires current flowing in the 1000 ~ supply generates a gate signal which is rectified and filtered. The filter has a time-constant of about 0.5 msec. This *DC* envelope is amplified and clipped and presents a clean positive and a negative going rectangular wave when a 1000 ~ current is flowing. These signals gate a 1000 ~ signal alternately to the two channels of a magnetic tape-recorder (Magnecord M-90).

Since the same master oscillator is used for all recording functions, the accuracy of the information placed on the magnetic tape will depend only on the constancy of the 1000 ~ oscillator. Acetate tape is used and recording is at a speed of 7.5 in. per sec.

Playback system. During playback, the magnetic tape transport is driven by a small gearhead synchronous motor which drives the capstan flywheel at about 0.75 in. per sec. This reduction in playback speed was adopted in order to make it possible to use an ordinary parallel entry printer rather than a more costly high speed printer for the print-out of the time-data. Since the recovery of the data from the tape involves counting pulses, the speed of playback has no effect on accuracy.

'Event' (contact) and 'interevent' (stride) durations are obtained by playing the tape back twice. A selector switch picks the appropriate channel. The signal from the tape is filtered through a 100 ~ L-C filter and then goes to a Decatron counter with five decades in the counter up to 100,000 cycles of the timing signal may be counted. At the end of each signal a relay opens which prints the count and resets the counter. The system is then ready to receive the next signal.

Time constants are adjusted so that interevent times of less than 40 m.sec. are disregarded when event times are being counted. Similarly, when counting interevent times, event times of less than 4 m.sec. do not cause a print. Since the ratio of playback speed to recording speed is 1:10 these recorded times correspond to actual times of 400 m.sec. during playback. Thus the printer is assured of a minimum of 400 m.sec. to recover after each print.

A logic element driven by both channels of the tape recorder causes a print command to be given the printer in the absence of a signal on either channel. This causes a double zero to be printed on the paper tape. During the course of a trial a signal is always present on one or the other of the two channels. Thus the double zero serves as an indicator of the end of a trial.

While the recording system was initially developed for studying animal behavior, it should be applicable in studies of human motion. Frequently such studies make use of high speed photographic techniques. The time and expense involved in recovering the desired data from motion picture records may, however, be prohibitive. Apparatus of the type described here should make it possible to obtain most, if not all, of the useful information available from photographic records.

The present instrument has the advantage that precise measurement of the duration of each of several movements may be recorded. This permits analysis of response-variability, as well as the average duration of component movements. Furthermore, the use of the paper tape recording permits the investigation of movement sequences in which the order of occurrence of successive responses is not fixed.[2]

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[2] K. U. Smith and R. F. Wehrkamp, A universal motion analyzer applied to psychomotor performance, *Science,* 113, 1951, 242-244.

ALL-ELECTRIC RECORDING IN A STRAIGHT ALLEY

By CECIL C. BRIDGES, Texas Christian University

Brown and Hoopes have reviewed previous attempts at continuous recording of an animal's position in a straight alley and have developed an improved device which may be described as a see-saw runway with a strain-gauge to measure deflecting force.[1] While their device represents a considerable improvement over former techniques, it has several minor disadvantages: strain-guages are delicate and expensive, the system must be calibrated for the weight of each animal used, and vertical jumps are recorded as horizontal motion.

The present device uses an entirely electronic approach and avoids these difficulties. The circuitry of the device is diagrammed in Fig. 1. The vertical bars represent the interlaced bars of a grid floor in a runway. One set of bars is connected to a common bus-bar, while alternate bars are connected between a series of 15,000 Ω resistors. The series of resistors is connected across the output of a 20-v. regulated power supply. The device functions as a simple voltage divider, with the moving animal acting as the variable contact of a potentiometer. More specifically, the feet of the animal provide a path for current to flow from the series of resistors to the bus-bar. The voltage varies in small equal increments between the ends of the series of resistors. The voltage conducted to the bus-bar depends on the point at which the feet of the animal contact the

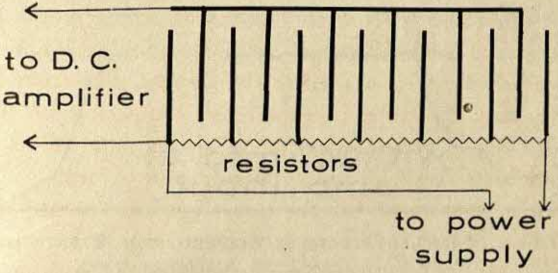

FIG. 1. SCHEMATIC DIAGRAM OF THE DEVICE

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[1] J. S. Brown and J. J. Hoopes, A device for recording a rat's position in a straight alley, this JOURNAL, 78, 1965, 130-133.

grids. A Sanborn DC amplifier is connected to a Sanborn DC recorder to provide a continuous record of this varying voltage level over time. With 52 resistors, each of the voltage increments represents less than 2% of the total range, and the point at which the animal contacted the grids can be determined within 1 in. Because of the high input impedance of the amplifier, current flow through the animal is only a few microamps and white rats do not appear to respond to this current flow at this voltage. When observing a white rat, one sees that the tail is often dragged across the grids, but this produces only a negligible change in the record because the dry rat-tail is such a poor conductor. By holding the rat and pressing its tail against grids damp with rat urine, a spurious localization can be produced, corresponding to the position at which the tail shorted the grids. Even under these circumstances however, if the tail is merely passively dragged across the grids, no noticeable change in the record results. Possibly, the bristles of the rat's tail prevent grid contact.

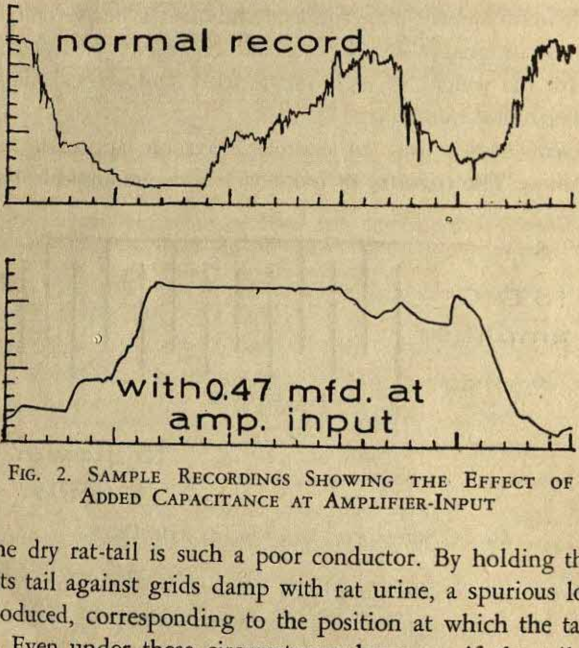

FIG. 2. SAMPLE RECORDINGS SHOWING THE EFFECT OF ADDED CAPACITANCE AT AMPLIFIER-INPUT

The present runway was built of plexiglas with the grid-floor composed of thin brass brazing rods spaced on half-inch centers. The runway is approximately 52 in. long with 52 15,000 Ω resistors in series providing a load of 780,000 Ω. Because current flow is negligible, it would be possible to substitute dry batteries for the regulated power supply. With an increase of the gain of the amplifier, a considerably lower voltage across the series of resistors could be employed, but the 60 ∼ hum associated with room wiring is more likely to be picked up with high amplification. Fig. 2 is an example of the records obtained with this device.[2] The records presented here represent two different passages of the same animal through the runway. The upper record was made with the apparatus as described and the lower record was made with low-pass filtering. The divisions on the abscissa represent intervals of approximately 1 sec. and the divisions on the ordinate represent location in units of approximately 5 in.

In trial runs using white rats satiated for water and food, suprising individual consistencies in the graphic record of the exploratory locomotion of these animals was observed, consistencies which were not at all apparent to the observer who simply watched the animals.[3] This invariance in exploratory behavior as well as certain problems of approach-avoidance are currently being investigated using this device.

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[2] A 0.47-microfarad capacitor across the input of the DC amplifier can be used for low-pass filtering. This capacitor will smooth out the small rapid jumps in the record. This makes the record considerably easier to read if one is interested only in the general position of the animal, but sacrifices information about foot-movements.

[3] Pereboom has previously noted regularities in exploratory behavior in a much longer runaway. (A. C. Pereboom, Continuous recording of spontaneous runway behavior: A normative and methodological study, *Psychol. Rep.*, 11, 1962, 223-237.)

AN ELECTRICAL DRINKING SPOUT FOR SMALL ANIMALS

By WILLIAM J. MUNDL, McGill University

An electrified drinking spout to administer mouth shock as punishment has been successfully used in passive avoidance experiments.[1] The task of counting the number of shocks the animals received was, however, found tedious and very careful observation was necessary to judge correctly 'pseudo-responses,' *i.e.* those made when the animal comes in close approach to the spout and retracts without having touched it. The device described here greatly simplifies collecting of data, and extensive use has shown it to be reliable.[2]

The circuit arrangement of shock source and electronic signal processing is shown in Fig. 1. The shock current is held nearly constant by using a current source of 240-V. A.C. and a high series resistance; thus changes in contact resistance between animal and floor and, also, between animal and drinking spout have only a small influence on the magnitude of shock current. When touching the metal spout, the animal completes a current path from shock source over the primary winding of transformer T1 to ground (floor of test-enclosure), thereby inducing a voltage in the secondary winding. This voltage is amplified by Q1, a common emitter stage. A 1,000 Ω resistance in the emitter loop provides for stabilization of the operating point. Diode CR1 short-circuits large positive transients which originate in the transformer winding when the contact between spout and ground is broken. The build-up of negative transients is eliminated by the diode action of base and emitter of Q1. The output of Q1 is peak-to-peak rectified and clamped to ground by diodes CR2 and CR3, resulting in negative waves which then trigger the Schmitt module.[3] The output of the trigger starts the timing cycle in the monostable multivibrator which is adjusted to approximately 150 m.sec. by capacitor C2. This pulse is applied to the driver stage Q2 which actuates an electromechanical counter.[4] Diode CR4 suppresses large transient voltages caused by the inductance of the counter-coil. A power supply provides regulated +6 v. and —6 v. for the amplifier and the circuit modules, and unregulated

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* Supported in part by grant M-1475 from the National Institute of Mental Health, and by grants from National and Medical Research Councils and the Defence Research Board of Canada.

[1] Phyllis Kasper, Attenuation of passive avoidance by continuous septal stimulation, *Psychon. Sci.*, 1, 1964, 219-220; Louis Pellegrino, The effects of amygdaloid stimulation on passive avoidance, *Psychon. Sci.*, 2, 1965, 189-190. Cf. also J. H. Hill and Eliot Stellar, An electronic drinkometer, *Science*, 114, 1951, 43-44; P. T. Young, Continuous recording of the fluid-intake of small animals, this JOURNAL, 70, 1957, 295-298; P. T. Young and W. M. Kappauf, Apparatus and procedures for studying taste preferences in the white rat, this JOURNAL, 75, 1962, 482-284.

[2] The apparatus was used by Pellegrino as part of work for a Doctoral dissertation.

[3] Circuit modules are of Philips manufacture.

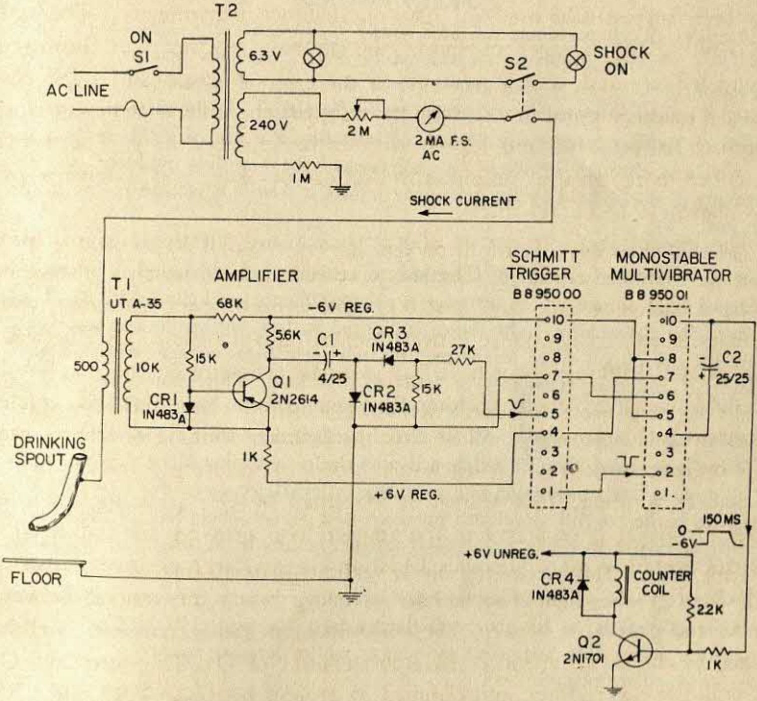

FIG. 1. SCHEMATIC DIAGRAM OF SHOCK-SOURCE AND COUNTING CIRCUIT
Capacitor C1 governs the response time of the counting circuit.

+6 v. for the counter. Switch S2 in the shock circuit is a silent, low capacitance type.[5]

Since test set-ups vary with experiments, the following changes in the circuitry can be implemented in order to obtain maximal reliability of the apparatus. Sensitivity is increased by reducing the emitter-resistor of Q1, or vice versa, the required sensitivity being determined by the shock current. Further, the circuit response time can be shortened by reducing C1.

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[4] Elmeg Type Q23, RA.

[5] Federal A. C. Switch. Type 1424.

APPARATUS NOTE

AN INEXPENSIVE ELECTRONIC SWITCH FOR AUDIO FREQUENCIES

Electrical switch transients can be a source of annoyance or error in many audio frequency circuits. Problems engendered by such transients may range from an unwanted click in an output transducer at the onset of a pure tone signal to a spurious command signal in a sensitive transistor circuit. At the same time, accurate length of signal is frequently needed, necessitating the use of a timer, the output of which is through a relay. This aggravates the transient problem, since the changing magnetic field in the relay may induce a transient voltage in the leads to the load.

The solution of this problem lies in electrical separation of the transient producer from the controlled circuit. One method to achieve such separation is by use of a switched light source which activates a photoconductor or light-sensitive semi-conductor. The choice of a light source is limited to low voltage incandescent lamps, since the use of gas filled lamps requires relatively high voltage, with consequent danger of inducing transients in leads to the load. This choice, in turn, precludes the use of photoconductor devices and photosensitive transistors, since thermal filament lag in light output will be directly reflected in their signal output.

What is required, then, is a light-activated device with immediate turn-on/turn-off ("all-or-none") characteristics and requiring no auxiliary power for operation. Such a device is the recently developed photosensitive silicon controlled rectifier or light switch, a relatively cheap and easily obtained example of which is the General Electric X2A. This device maximally switches current from Anode (A) to Cathode (C) when light of an intensity determined by a bias voltage from Cathode to Gate (G) falls on its photo-sensitive surface. Increase of light intensity above triggering value does not effect switching, while decrease below triggering value causes immediate turnoff.

An inexpensive electrically silent switch for audio frequencies of DC to 1000 *CPS* at peak voltages up to 25 and maximal current up to 0.22 a. is shown in the accompanying schematic diagram. Two GE X2A light-activated switches provide full-wave passage of current when illuminated by the 6-v. bulb, which in turn is activated through a switch or timer contacts. Half-wave passage may be accomplished with a single GE-X2A. The 50K potentiometer between Cathode and Base serves as a sensitivity control. Lowering the Gate to Cathode resistance lowers the sensitivity to light. Linear potentiometers should be used; miniature printed circuit board types are suggested (see Fig. 1).

It is suggested that this unit be constructed on a phenolic chassis in a relatively light-tight metal case and that two-conductor shielded cable be used, with shield bonded to the case, timer (if used), amplifier chassis, and signal generator chassis (if used). This will assist in shunting to ground any 60 cycle artifact. The light activated switches should be mounted in standard transistor sockets, to prevent heat damage in soldering the leads.

For adjustment, with the switch between the desired load and a sine wave input, and the lamp illuminated, potentiometers are started at low resistance and raised while observing output on an oscilloscope. When triggering sensitivity is reached, a sine wave will be observed on the scope tube face. Care should be taken not to exceed the voltage and current ratings of the light activated switches. Ambient temperature of the X2As should not exceed 100°F.

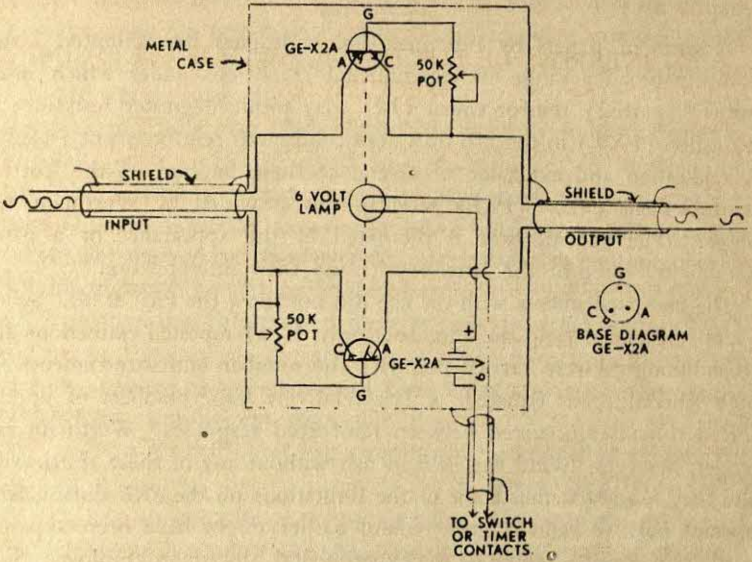

FIG. 1. SCHEMATIC DIAGRAM OF THE ELECTRONIC SWITCH

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NOTES AND DISCUSSIONS

A NOTE ON RESISTANCE TO EXTINCTION IN THE FISH

A series of papers by Bitterman and colleagues has delimited a successively broader range of experimental conditions under which intermittent (partial) reinforcement (*IR*) may produce greater resistance to extinction (*EXT*) in the fish than does continuous reinforcement (*CRF*).[1] A replication and extension of several of these findings on the 'partial-reinforcement effect' (*PRE*) recently was provided by Wertheim and Singer.[2] The present note is prompted by the appearance of a paper questioning some of the latter results and their interpretation.[3]

The first experiments with the fish did not show the *PRE* at all.[4] Subsequent studies did yield the effect, but only if (a) repeated extinctions and reconditionings were carried out, (b) the number of reinforcements for both schedules was equated, or (c) relatively large numbers of unreinforced responses occurred between reinforced responses.[5] Wertheim and Singer, however, found the *PRE* in fish without any of these restrictions, and they suggested that some of the limitations on the *PRE* during free-operant positive reinforcement found earlier might have been apparent partly as a result of some of the experimental procedures employed. Such procedures included the utilization of relatively short sessions of extinction-testing, and the measurement of extinction-performance during a single session, with the critical consequence that a comparison of repeated measures of the time-course of extinction (both within individual *S*s and between *S*s) was excluded. In support of their suggestion, Wertheim and Singer first replicated the finding that initial resistance to extinction was much greater after *CRF* than after *IR*. The significance of this difference was tested by comparing the rate-changes from the last day of training to the first day of extinction for each *S*, using a *t*-test for the difference between correlated means.[6] Much more importantly, however, this difference was clearly visible in the individual daily response-rate functions and in the cumulative records.[7] The earlier findings were then extended by the demonstration that after the first day of extinction the response rate was greater after *IR* in all four *S*s, even though the number of reinforcements during *CRF* and *IF* was not equated.

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[1] Jerome Wodinsky and M. E. Bitterman, Partial reinforcement in the fish, this JOURNAL, 72, 1959, 184-199; Wodinsky and Bitterman, Resistance to extinction in the fish after extensive training with partial reinforcement, *ibid.*, 73, 1960, 429-434; R. C. Gonzalez, R. M. Eskin, and M. E. Bitterman, Extinction in the fish after partial and consistent reinforcement with number of reinforcements equated, *J. comp. physiol. Psychol.*, 55, 1962, 381-386.

[2] G. A. Wertheim and R. D. Singer, Resistance to extinction in the goldfish following schedules of continuous and variable interval reinforcement, *J. exp. Anal. Behav.*, 7, 1964, 357-360.

[3] Gonzalez and Bitterman, Partial reinforcement and the fish, this JOURNAL, 78, 1965, 136-139.

[4] Wodinsky and Bitterman, *op. cit.*, 184-199.

[5] Gonzalez, Eskin, and Bitterman, *op. cit.*, 381-386; Further experiments on partial reinforcement in the fish, this JOURNAL, 76, 1963, 366-375.

Gonzalez and Bitterman note a possible ambiguity in the analysis of these results, since greater resistance to extinction after *IR* might be expected solely because a greater number of reinforced responses preceded the post-*IR EXT* than preceded the post-*CRF EXT*. If the reasoning of this argument were correct, however, then it would also be expected that *IR* would produce a greater resistance to extinction even on the first day of *EXT;* further, this difference in favor of post-*IR* extinction responding should be most marked on the first day of *EXT*. As may be noted from the results reported above, neither of these expectations is in agreement with Wertheim and Singer's data.

The results of several other experiments also indicate that the number of reinforced training trials is not a factor in determining resistance to extinction once an asymptotic performance level has been reached.[8] After stable responding has been achieved, principal determinants of resistance to extinction include such variables as the reinforcement schedule, and the sequence of schedules, just prior to extinction. For example, Sutherland, Mackintosh, and Wolfe found that, in the rat, resistance to extinction was progressively less under the following conditions, respectively: 60 trials *IR*, 100 trials *CRF* followed by 60 trials *IR*, 160 trials *CRF*, 60 trials *CRF*. (The last two groups were virtually identical, however, differing only in the *SD*.) The conditions for the second and fourth groups are analogous to those employed by Wertheim and Singer; it is therefore of interest to note that the functions relating response-strength to time in extinction for these two groups of rats are identical in form to those obtained by Wertheim and Singer in the fish.

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[6] Quinn McNemar, *Psychological Statistics*, 2nd ed., 1955, 108; cf., Gonzalez and Bitterman, *op. cit.*, 136-139.

[7] Wertheim and Singer, *op. cit.*, Figs. 2 and 3, 359.

[8] H. M. Jenkins, Resistance to extinction when partial reinforcement is followed by regular reinforcement, *J. exp. Psychol.*, 64, 1962, 441-450; John Theios, The partial reinforcement effect sustained through blocks of continuous reinforcement, *ibid.*, 64, 1962, 1-6; N. S. Sutherland, N. J. Mackintosh, and J. B. Wolfe, Extinction as a function of the order of partial and consistent reinforcement, *ibid.*, 69, 1965, 56-59; S. E. Sperling, Reversal learning and resistance to extinction, *Psychol. Bull.*, 63, 1965, 281-297.

Prior to the attainment of an asymptotic performance-level, resistance to extinction in, for example, the pigeon and in the rat increases with negative acceleration as the number of reinforcements during initial conditioning increases.[9] A study by Gonzalez, Eskin, and Bitterman, using a repeated-measures design, demonstrated a similar result in the fish but failed to replicate this finding in the rat.[10] Unfortunately, these latter experimenters reported only the total number of extinction-responses for groups of *S*s, a measure that would obscure possible trends, *e.g.* in the (likely) direction of more rapid extinction and re-acquisition during repeated conditionings and extinctions in the rat. Since the functions for individual *S*s may be obscured irreversibly by the use of grouping techniques, the results of such studies are not readily comparable to those from experiments in which the behavior of individual *S*s is the principal focus of analysis.[11] For example, it might well have been that the differential extinction-patterns reported by Wertheim and Singer also were present in many of the *S*s studied by Bitterman and colleagues, but that averaging obscured these patterns.

It thus seems apparent that in order to provide a comprehensive analysis of factors determining performance during conditioning or extinction, it is necessary to consider the sequential characteristics of the successive events (such as responses, pauses, and reinforcements) that may occur. For example, in the experiment by Wertheim and Singer, the *IR*-schedule that was used (a variable-interval schedule with a mean inter-reinforcement interval of 50 sec., producing a 4:1 average ratio of responses to reinforcements) generated a relatively constant free-operant response-rate during conditioning; the *CRF* schedule did likewise. Because the momentary response-rates during each conditioning schedule were highly stable prior to the extinction-test, the changes in rate during extinction were readily discernible in all four *S*s. These stable sequential characteristics of the behavior of the individual *S*s during conditioning were critical in the development of Wertheim and Singer's argument for a fundamental similarity among some of the resistance-to-extinction characteristics of fish and higher organisms. During conditioning, for example, the cumulative response records indicated a high day-to-day stability of performance, as well as the presence of characteristic schedule-specific, within-sessions sequences of responses and pauses. Furthermore, not only was the over-all rate of response-decrement in extinction less after *IR* than after *CRF*, but the comparative schedule-differentiating, moment-to-moment sequences of responses and pauses were quite similar to those observed under like conditions in organisms higher on the phylogenetic scale.[12]

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[9] Eliot Hearst, Resistance-to-extinction functions in the single organism, *J. exp. Anal. Behav.*, 4, 1961, 133-144; C. T. Perin, Behavior potentiality as a joint function of the amount of training and the degree of hunger at the time of extinction, *J. exp. Psychol.*, 30, 1942, 93-113; S. B. Williams, Resistance to extinction as a function of the number of reinforcements, *ibid.*, 23, 1938, 506-552.

[10] Gonzalez, Eskin, and Bitterman, Alternating and random partial reinforcement in the fish, with some observations on asymptotic resistance to extinction, this JOURNAL, 74, 1961, 561-569.

[11] Murray Sidman, A note on functional relations obtained from group data, *Psychol., Bull.*, 49, 1952, 263-269; W. A. Deterline, Operant discrimination reversals: Comparative data, *J. exp. Anal. Behav.*, 3, 1960, 247-253; Hearst, *op. cit.*, 133-144.

While the findings of Wertheim and Singer may be generalizable to other experimental paradigms, the relevant experiments have yet to be performed. One principal implication of these results, however, is that the conditions requisite for the *PRE* in fish are less limited than has previously been supposed. Greater limits thus are placed upon the generality of statements concerning behavioral differences between fish and higher classes of animals. Although the results of Bitterman and colleagues have been interpreted to support the hypothesis of such a class-difference, it is apparent that the case for this interpretation has yet to be proven. (It also may be that Gonzalez and Bitterman do not now unequivocally reject the more conservative position. Thus, while in an early paper it was concluded that the *PRE* may be obtained in fish "*only* with the equation of reinforcement," more recently it was asserted that the effect may appear "*even when* the total number of training sessions and the total number of reinforcements in the same" (emphases added).[13]

At the present time, experimental evidence does not permit the definition of a set of necessary conditions for the *PRE* in fish, probably because of the wide range of experimental situations in which the dimensions of the effect may be studied, and because of the many different techniques of analysis that may be applied to the data.

Stanford University School of Medicine GEORGE A. WERTHEIM

A REPLY TO WERTHEIM

In the foregoing note, Wertheim repeats the assertion that he and Singer "found the *PRE*" in fish without equating number of reinforcements and thereby demonstrated "that the conditions requisite for the

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[12] Wertheim and Singer, *op. cit.*, Fig. 3, 359; F. S. Keller and W. N. Schoenfeld, *Principles of Psychology*, 1950, 70; C. B. Ferster and B. F. Skinner, *Schedules of Reinforcement*, 1957, 346.

[13] Gonzalez, Eskin, and Bitterman, *op. cit.*, 386, Gonzalez and Bitterman, *op. cit.*, 136.

PRE in fish are less limited than has previously been supposed."[1] What Wertheim and Singer actually found was greater resistance to extinction after 24 sessions of continuous reinforcement *plus* 24 sessions of *VI*-reinforcement than after 24 sessions of continuous reinforcement alone. Certainly there was a substantial difference in number of reinforcements under the two conditions, but what Wertheim seems unable to grasp is that the direction of the difference deprives the result of any significance for the problem of partial reinforcement.

In comparing resistance to extinction after partial and consistent reinforcement, it is impossible to equate both number of trials (or responses) and number of reinforcements. When reinforcements are equated, the partial animals make more responses. Under these conditions, both the fish and the rat show greater resistance after partial reinforcement, but such a result is difficult to interpret; it may be due to inconsistency of reinforcement, or it may be due to the greater frequency of response in the partial animals, or to secondary reinforcement on unreinforced training trials. Only when trials are equated can greater resistance to extinction in the partial animals be attributed with confidence to inconsistency of reinforcement, because frequency of response is the same in the two groups and the consistent animals have more reinforcements. It is under these conditions that greater resistance to extinction after partial reinforcement has been found in the rat but not in the fish, and the experiment of Wertheim and Singer (in which none of the critical variables was equated) does not alter this fact. With more sessions, more responses, and more reinforcements in the partial case than in the consistent, their experiment tells us nothing at all.

Why Wertheim and Singer should have chosen to do such a pointless experiment is difficult to understand. Citing results obtained with the rat, Wertheim argues that the *CRF*-sessions which preceded the *VI*-sessions were not responsible for the difference in resistance to extinction, but why then were they given? Because the devil finds work for idle hands? The argument from results for the rats implies the assumption, of course, that rat and fish are alike with respect to the way in which they are affected by reinforcement. It does not seem to trouble Wertheim that he must assume the two animals to be alike in order to justify the design of an experiment which is said to provide evidence that they are alike. Our own evidence that the fish may be more sensitive than the rat to frequency of reinforcement[2] is badly muddled by Wertheim. Failing to distinguish between initial resistance to extinction and asymptotic resistance to extinction, he sees our experiment, incorrectly, as a failure to replicate certain earlier results with the rat, and he responds to the imagined discrepancy from a handy store of Skinnerian homilies. He concludes with a dark hint (documented with two quotations torn heedlessly from context) that despite our professed indifference to the Wertheim-Singer results we may have been led by them to alter our position on the problem of partial reinforcement in the fish. Here again he is mistaken.

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[1] G. A. Wertheim, A note on resistance to extinction in the fish, this JOURNAL, 78, 1965, 138-141.

Bryn Mawr College M. E. BITTERMAN
R. C. GONZALEZ

THE COFFEE CUP ILLUSION

The illusion described here is one which is so striking to me that for a number of years I have felt unwilling to call it to the attention of other people, except in an informal way, on the grounds that it must surely have been observed many times by many people. I have, however, yet to find anyone who, when shown, exhibits anything other than surprise. Under the circumstances to be described, an image of a familiar object whose angular size increases and which is, in fact, moving closer to the *O*, is seen as being sharply diminished in size, and, as moving away from the *O*. Thus, far from exhibiting constancy, or even the "law of visual angle," it is a prime example of inconstancy. Although a coffee cup is not essential to the existence of the effect, it is true that it works very well. Further, when one is drinking coffee the kind of stimulus-object for which the effect seems to work best is almost always present.

If one looks at the reflection in a cup of coffee of an overhead light on a dark or poorly illuminated ceiling, the surround of the light is so dark as to be virtually invisible. The darkness of the coffee, I think, also helps to suppress any well-structured surround of the light. The *O* should make his first observation at such a viewing distance from the cup that the image of the light nearly fills the cup. If, then, the head is smoothly and rapidly moved closer to the cup, the apparent size of light is very much reduced and its apparent distance very much increased. The effect is striking and it may be repeated frequently without any loss of the illusion.

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[2] R. C. Gonzalez, R. M. Eskin, and M. E. Bitterman, Alternating and random partial reinforcement in the fish, with some observations on asymptotic resistance to extinction, this JOURNAL, 74, 1961, 561-569.

The magnitude of the change is very large. The light seems to shrink to a quarter or a fifth of its original size and to move a distance which may be ten times that moved by the observer in approaching the cup. Here we have an actually increasing visual angle with an apparent decrease in size, and an actually decreasing distance with apparent increase in distance. Two questions are raised by the illusion. One is the question of why the light looks smaller. The other, of why it looks farther away. Of course, as the head approaches the cup, the visual angle of the light increases. The visual angle of the cup increases even more rapidly and the *ratio* of *angular 'light'* to *angular 'cup'* decreases markedly, however, as shown in Fig. 1.

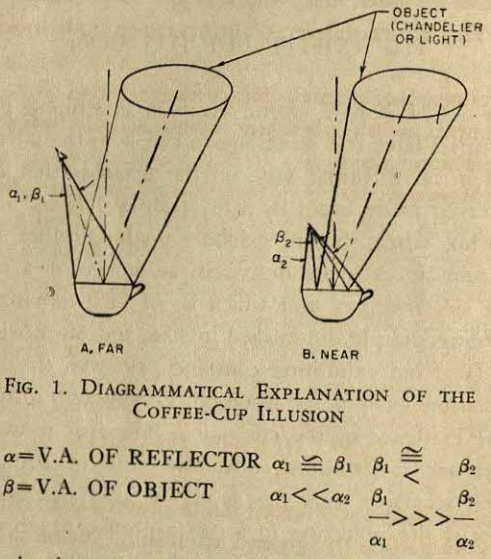

FIG. 1. DIAGRAMMATICAL EXPLANATION OF THE COFFEE-CUP ILLUSION

α=V.A. OF REFLECTOR $\alpha_1 \cong \beta_1$ $\beta_1 \overset{\cong}{<} \beta_2$

β=V.A. OF OBJECT $\alpha_1 << \alpha_2$ $\frac{\beta_1}{\alpha_1} >>> \frac{\beta_2}{\alpha_2}$

The illusion is *dynamic*. It appears when the *O* moves. Observation from any fixed position, the 'near' point for example, does not give the impression that the light is smaller than it should be. The impression apparently depends on the relative rate of change of the angles involved.

The cup is seen as being of constant size. It is clearly visible and in a well-structured surround, and constancy holds for it even with change in distance of the *O*. The effect apparently arises from a mechanism similar to that suggested by Hering as described by Boring, in which the apparent size of a distant window, when the attention is directed to the hand in front of the face, varies with the distance of the hand.[1] *Here,* however, the size-change does not demand that the attention be directed to the near object, the coffee cup, in order that *it* be seen as constant.

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[1] E. G. Boring, *Sensation and Perception in the History of Experimental Psychology*, 1942, 292.

The light, therefore, must appear smaller when it fills less of the cup, and indeed, since the image of the light increases but slightly whereas the cup increases a great deal, the light can appear perhaps as small as one-tenth of its original size, depending upon how close to the cup the *O* moves and how far away he was to begin with. The problem of distance is a difficult one. One is always tempted to say simply that something that looks smaller must have moved away and therefore it is smaller. On the other hand, since the visual angle of the light is not constant, but in fact increases slightly, one could also imagine that it would appear to be much closer in order that, with a larger visual angle, it can appear to be smaller. This is not, however, the case; the light looks smaller and farther.

I have ascertained that a small pocket-mirror serves as well as a coffee cup if the ceiling is not too brightly illuminated. It has the further advantage of not having waves.

Cambridge, Mass. JOHN W. SENDERS

A SEARCHLIGHT ILLUSION

An optical illusion is experienced while observing the stationary beam of a searchlight located on the ground in Flushing Meadow Park, Borough of Queens, New York City. The searchlight, a guide for aircraft pilots, is about 3.5 miles southeast of La Guardia Airport. The beam is parallel, of uniform brightness throughout its length, and maintains a fixed inclination of about 70° to the horizontal. Although actually tilted toward the airport, the beam appears to tilt toward the *O* who stands facing the searchlight from any surrounding location. The beam seems to point overhead, either directly or somewhat to the right or left, depending on *O*'s position, but always well within his quadrant. This illusion is independent of his distance from the searchlight, or of weather conditions.

Let *O*'s position be at 0°, as shown in Fig. 1, when the beam tilts (actually) directly away from him. If α is his angular position measured counter-clockwise around the searchlight, β the angle of inclination of the beam, and θ the *apparent* angle between the beam and the vertical as viewed from position α, it is readily shown that

$$\theta = \text{arc tan } (\sin \alpha \,.\, \text{cotan } \beta).$$

For the present purpose, we include only values of θ between 0° and ±90°, and call θ negative when tan θ is negative; α is positive and varies, in the conventional manner, from 0° to 360°, while β is positive and constant. Considered in this manner, positive and negative values of θ are a measure of the magnitude of the beam's deviation to the right and left, respectively, in its real or apparent tilt toward O. When θ is zero, it points directly overhead, actually ($\alpha = 180°$) or apparently ($\alpha = 0°$).[1]

The illusion is not visible from the air. Three airline pilots and one first officer, questioned separately, stated that they saw only a vertical lightbeam when approaching or departing from the airport at altitudes of 1,500-2,000 ft.

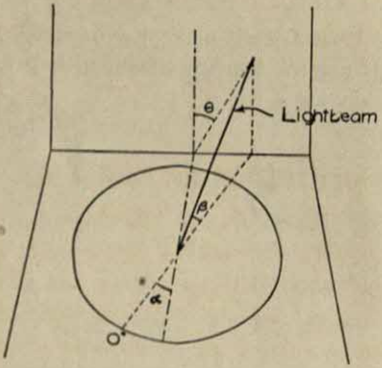

FIG. 1. A SEARCHLIGHT ILLUSION

A suggested explanation of the illusion is that the O, who is ground-based, sees the stationary lightbeam against a background of the night sky, which offers no cues regarding the true direction of the beam. He sees the sky, however, as a surface, rising toward him from the horizon and passing overhead. These two factors combined lead him to see the beam as a projection on the sky. Hence, the beam will always appear to tilt toward him when he faces its source from any encircling position.[2]

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[1] See, for example, E. G. Bilham, An azimuthal method of measuring cloud height with a searchlight, *Nature*, 159, 1947, 677-678, for a somewhat related equation.

[2] I am indebted to Dr. George Gourevitch, New York University School of Medicine, for this explanation.

Several other optical illusions associated with lightbeams emanating from searchlights or revolving beacons[3] have been noted in the past, but they bear no resemblance to the phenomenon described above.[4]

Isaac Albert Research Institute, **GILBERT GOLDSTEIN**
Jewish Chronic Disease Hospital, Brooklyn, N.Y.

AN ACKNOWLEDGMENT

The portrait of Henri Piéron, which appears as the frontispiece of this number, was taken about 1951 when Piéron was seventy years old, thirteen years before his death. It was kindly supplied by Professor Paul Fraisse, Piéron's younger colleague and successor as Director of the Institute of Psychology at the University of Paris. The signature comes from the files of Professor Karl M. Dallenbach and is unusual in being nearly illegible, for Piéron usually relied on his typed name for identification, when necessary, of the hieroglyph so well known to his correspondents.

E. G. B.

Henri Piéron: 1881-1964

Born July 18, 1881, Piéron died on November 6, 1964, at the age of eighty-three years. In 1900 he read his first scientific paper to the IVth International Congress of Psychology in Paris, and after that he kept on publishing articles, studies, and critical reviews with inexhaustible energy. His role has been fundamental in the development of scientific psychology in France and also important in the international scene. In his introductory lecture of 1907 (published in 1908) at the École Pratique des Hautes Études, Piéron was already maintaining that the subject matter of psychology is behavior (*comportement*), a position which he had attained independently of Watson, whose famous pronouncements on this matter are dated from 1913 to 1919.

The son of a teacher of mathematics, Piéron started out (as became quite usual in France for future psychologists) with a *licence* and an

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[3] Mathew Luckiesh, *Visual Illusions: Their Causes, Characteristics, and Applications*, 1922, 160 ff.; S. S. Beggs and J. M. Waldram, Searchlights, *London Illumi. Engin. Soc.*, 1948, 108-110.

[4] Georges Colange and Yves Le Grand, Observation de l'image atmosphérique d'unphare, *Comptes Rendus*, 204, 1937, 1882-1884; Louis Dunoyer, Sur les illusions d'optique provoqués par un phare tournant, *Comptes Rendus*, 205, 1937, 867-869; G. ten Doesschate and F. P. Fischer, A propos des illusions d'optique provoquées par d'unphare tournant, *Annales d'Oculistique*, 176, 1939, 103-108.

[*] Translated from the French by Mollie D. Boring.

agrégation in philosophy, but he also obtained a *doctoratès sciences* in physiology with a thesis on sleep. He then worked with Alfred Binet and succeeded him in 1911 as head of the psychological laboratory at the Sorbonne. He also collaborated for some time with Pierre Janet at the Salpêtrière. Named to the École Pratique des Hautes Études in 1907, he founded in 1920 (with the assistance of Janet, Dumas, and Delacroix) the Institut de Psychologie of the University of Paris and remained its director for a long time. Then, in 1928, he founded the Institut National d'Études du Travail et d'Orientation Professionnelle, which he also directed. It was in 1923 that he was elected to the Collège de France and to the chair in physiological psychology that he occupied until his retirement.

He was always an active participant in the International Congresses of Psychology, as well as the psychotechnical congresses after they had been founded. He played an important role in the foundation and later in the organization of the International Union of Scientific Psychology. He was its first president.

Not only has the main tenet of Piéron's psychology always been that the proper subject of our science is conduct or behavior but, further, that every particular psychological explanation always leads, when it is sufficiently precise, to a physiological explanation. At the time of the presentation of his scientific work (for his candidacy in the Collège de France), Piéron used for an epigraph this statement of Johannes Müller's: *Nemo psychologus nisi physiologus.*

In no way does that aphorism imply, in Piéron's thinking, a pure and simple reduction of psychology to physiology, for he would then have been obliged to abandon psychology and become a physiologist. The principle means, rather, that, since behavior is a 'total' reaction of the organism, whereas physiological processes are 'partial' reactions, we cannot really understand the total reaction without knowing the details of the partial reactions. For example, in studying sleep and proving that 'hypnotoxin' taken from a sleepless dog will put a normal dog into deep sleep, Piéron proved that under ordinary living conditions the normal dog acquires an anticipatory rhythm that helps it to avoid the intoxication beforehand, but that it succumbs once the intoxication reaches a critical level. This anticipatory 'conduct,' psychological in nature, is thus superimposed upon the normal physiological reactions of intoxication, and we have to realize what these physiological factors are before we can understand the dog's behavior.

This principle of relating psychology to physiology without reducing it to physiology led Piéron to choose as his special field of study the borderline between the two domains, especially the 'physiological psychology' of sensation. After numerous studies of differential thresholds, of Fechner's logarithmic relation and kindred topics, and after considerable original research on color vision, Piéron provided us with a splendid synthesis of all this research in his compendious work: *La sensation, guide de vie* (Gallimard, 1945, 1-420).

The two significant characteristics of these studies, in addition to what we have noted in respect of physiology, are: (1) that sensations are never considered separately, but always as they are set in the context of a total integration and thus are involved with the totality of behavior; and (2) that within this context of integrated behavior, the subjective impressions of the experimental subjects may not be neglected. As early as 1913, in an article reviewing "Le domaine psychologique," (*Année psychol.*, 19, 1913, 1-26), Piéron maintained "that there is no opposition between introspection and objective psychology." when introspection deceives us, there is still a problem of behavior to understand why, and the study of this problem, of the inadequacy of the subjective report, is itself instructive for objective psychology. In other words, consciousness is not a mirror but an interpretation (true or false), and this interpretation as such poses problems for behavior.

Another consequence of Piéron's interest in the link between psychology and physiology was his constant concern with relating human psychology to animal psychology. He was convinced that every general law in human psychology is common both to animal and man, and, after having developed his ideas on memory (*L'évolution de la mémoire,* Flammarion, 1910), in 1948 and 1958 he synthesized his ideas under the title *De l'actinie à l'homme: Études de psychophysiologie comparée* (Presses Universitaires de France).

Although Piéron's principal activities were thus directed upon psychophysics and comparative psychology, he nevertheless exhibited enormous activity in many other fields. We must in particular cite his contributions to applied psychology. In 1949 he published his *La psychologie différentielle,* which was the first in a seven-volume *Traité de psychologie appliqée* that he edited for the Presses Universitaires de France.

His interests in differential psychology were soon supplemented by his research on tests and on psychological examinations in general, which resulted in the publication *Examens et docimologie* (1963). "Docimology," as he defined it, is an objective and scientific study of the different methods of testing that have to do with examinations in the schools as well as being the standardized and rigid evaluations the tests are supposed to be. Along this line, he supplied a sharp criticism of the examinations in the schools by showing both the arbitrary and the chance factors that intervene when one submits the same problems or the same candidates to different examiners.

In addition to his original work, Piéron also contributed, especially as editor of *l'Année psychologique,* a considerable number of bibliographical analyses and critical studies. He read everything that came out in psychology, and his colleagues never could understand how he managed to become so well informed, while continuing to pursue his own personal research and devoting considerable time to the direction of the institutions he had established or helped to establish. He was, moreover, a delightful and very loyal friend, who always found time to receive and stimulate his colleagues in their mutual enterprises.

In a word, Henri Piéron was one of psychology's great servants.

University of Geneva JEAN PIAGET

Kurt Goldſtein: 1878—1965

Kurt Goldstein was born on November 6, 1878, in Kattowitz, Upper Silesia, to a non-orthodox Jewish family. He received his early education at the local public school and, when the family later moved to Breslau, he attended the Humanistische Gymnasium. After one semester at Breslau University, he went on to Heidelberg to pursue philosophy and literature, but returned the following year to study medicine, particularly psychiatry.

At that time the academic path to mental disease lay through neuroanatomy and neurophysiology. Goldstein found himself attracted to the pathological laboratory. Karl Wernicke, the chairman of the Department of Psychiatry, a psychiatrist who was also interested in symptomatology and post-mortem relationships, first directed Goldstein's attention to the problems of aphasia which later became for him an enduring preoccupation.

Goldstein received his M.D. in 1903, during the year in which he published five papers besides his dissertation. In 1904, he joined the *Senkenbergische Neurologische Institut* in Frankfurt-am-Main, as assistant to Ludwig Edinger, who became a most influential figure in Goldstein's career. Subsequently, Goldstein spent a year each with Hoche and Oppenheim. In 1906 he joined the staff of the Psychiatric Clinic of the University of Königsberg, where he devoted himself to the study and treatment of psychiatric patients. He used this opportunity for careful observations of patients, publishing on a variety of neurological and psychiatric topics. One early paper foreshadows some of the basic distinctions that underlie current differential diagnostic testing.

In 1914, Goldstein accepted Edinger's invitation to join him in Frankfurt as his first assistant. They both expected Goldstein to return to laboratory investigations, but he had by now acquired a taste for clinical work and a strong belief in its usefulness. Soon he had organized a hospital and research institute for brain-injured soldiers, the famous *Institut zur Erforschung der Folgeerscheinungen von Hirnverletzungen,* in association with Adhémar Gelb, a student of Stumpf's.

The Institute became a clinical center for diagnosis, treatment and rehabilitation of neurological and psychiatric military casualties. Its early publications dealt with bullet injuries to the central nervous system, war neuroses and hysterical symptoms. For Goldstein the real problems of the patients constituted the ultimate justification of the systematic studies undertaken. Advanced students and junior colleagues from psychology, medicine, and related fields soon joined this ongoing research activity.

Goldstein's systematic studies fall into four major phases. The earliest concerns the relationship between circumscribed cortical injuries and sensory or motor defects. Next come the problems of perceptual disturbances and agnosia and with them the initiation of the 16 papers in *Psychologische Analysen hirn-pathologischer Fälle,* of which the majority are concerned with the visual perception of patients with damaged optic tracts or occipital lobes.

Goldstein expanded these studies during the 'twenties into the area of cerebellar function and its relation to tonus. It was in this period that he organized his findings into the chapters on experimental techniques for the study of cutaneous sensations, diseases of the brain and meninges, the cerebellum, localization on the cerebral cortex, and similar topics which he wrote for a number of important handbooks.

Among his studies one had lain dormant for a decade: the problem of aphasia. His first paper on the topic had appeared in 1906, and eight others had been published prior to his return to Frankfurt. Now, nine years later, he prepared a paper on the nature of amnesic aphasia, and he followed it by a step-by-step analysis of the condition.

The late 'twenties also witnessed Goldstein's growing interest in psychotherapy, his critical examination of Freud's writings, and his attempt to embody into biology what he learned from neurological and psychiatric patients.

Concurrently his academic career ran on with editorships and society affiliations. Presently Goldstein succeeded Edinger as Professor of Neurology at Frankfurt. Simultaneously he became Director of the Neurological Institute, while remaining Director of the special Institute for brain-injured soldiers. He held various associate editorships and through Volume 17 remained as one of the founding editors of *Psychologische Forschung.*

In 1930, he accepted the Directorship of a large new municipal hospital for neuropsychiatry in Berlin together with a professional affiliation in the Department of Neurology and Psychiatry at the University of Berlin, with the proviso that he could continue association with his Brain Injury Institute. With high hopes he settled into this outstanding position in Germany, but history intervened.

Goldstein was briefly jailed when Hitler came to power in 1933, then released on condition he leave the country. The Rockefeller Foundation supported him for a year in Amsterdam. The Dutch were most friendly and he spent his time dictating what was to become his magnum opus, *The Organism.* The basic draft of this complex book was completed in five or six weeks, leaving his typist in a state of exhaustion. In 1934 it was published in Holland, but in German, under the title *Der Aufbau des Organismus.* The English translation appeared in America (1939) with the title *The Organism: A Holistic Approach to Biology Derived from Pathological Data in Man.* It included a foreword by Karl S. Lashley. It has since gone through many printings, has been published in paperback, and translated into other languages (among them French, Spanish, Russian, and Japanese). It deals with methodological questions as much as with facts, and it presents critical assessments of many major theoretical trends of its day and our own.

When he arrived in New York City in 1935, Goldstein entered the private practice of neurology and psychiatry. In 1936 he was appointed Clinical Professor of Neurology and Lecturer in Psychopathology at Columbia University. He worked for a time at the Psychiatric Institute, later becoming chief of the newly organized Laboratory of Neurophysiology at Montefiore Hospital. Once again he was joined by a psychologist, Martin Scheerer, with whom he examined patients, taught medical students and psychologists, directed some Ph.D. dissertations, and published the monograph titled *Abstract and Concrete Behavior: An Experimental Study with Special Tests.*

During the winter of 1938-1939, he was invited by Harvard to deliver the William James Lectures which were published as *Human Nature in the Light of Psychopathology*. In 1940, he accepted a five-year grant from the Rockefeller Foundation for the post of Clinical Professor of Neurology at Tufts College Medical School. He spent mornings at the laboratory of the Boston Dispensary, afternoons in private practice and writing. In addition to a steady stream of articles, he published in 1942 *After Effects of Brain Injuries in War: Their Evaluation and Treatment,* and, in 1944, with Eugenia Hanfmann and Maria A. Rickers-Ovsiankina, the monograph *Case Lanuti: Extreme Concretization of Behavior Due to Damage of the Brain Cortex.* It was based on work carried out at the Worcester State Hospital. In 1948, he also completed major sections of *Language and Language Disturbances,* a treatise on aphasia.

Although Goldstein had become an American citizen in 1940, from the beginning he had an intensely personal involvement in the progress of World War II. He had many friends in America and was grateful to the country where he and so many others had found asylum first, and then a new home, but it was still for him a home in exile.

The Tufts appointment ended in 1945. Goldstein was sixty-seven, and by the rules of academic life should have retired, but this was unthinkable for him temperamentally and impossible financially. He returned to private practice in New York City and various teaching activities at the City College and the New School for Social Research. In his late seventies, he began to commute once a week to Brandeis University.

In 1958, the larger world joined in the celebration of Goldstein's eightieth birthday. Several generations of students and friends assembled to present him with a special collection of manuscripts. The Hebrew University sent a beautiful scroll composed in his honor, and the University of Frankfurt awarded him the Doctor Honoris Causa.

Goldstein's last years were often lonely. His wife died in 1960 and many of the friends of his generation were gone: Gelb, Koffka, Wertheimer, Cassirer, Einstein, Lashley, Scheerer, and finally Heinz Werner. He had published over 200 articles and books and completed his personal contribution to the *History of Psychology in Autobiography* in 1965. Now he complained of tiring easily and felt himself getting old. Finally he suffered a stroke which left him paralyzed on the right side and unable to speak or move his right arm, and died three weeks later on September 19, 1965, in New York City.

Goldstein came to psychology from the field of psychiatry, more specifically neurology. His emphasis on the importance of psychological factors was a major contribution to neurology, but his view of psychology as biologically based even more important. His greatest empirical accomplishment was his life-time work with brain-injured persons, especially the aphasics. His major contribution in respect of theory was the organismic point of view. Despite his considerable affinity with the gestalt group, Goldstein always stressed the total response of the organism more than the nature of the perceptual field organization. For this reason he developed such concepts as the "catastrophic reaction" to marked anxiety which he found as characteristic of the brain-injured. He believed that this reaction is a natural response to the disparity between the person's capacities and the demands made upon him, a discrepancy that prevents the self-realization which is the goal of adaptive behavior and for which an "abstract attitude" is necessary. Instead he found "concrete attitudes" in the brain-injured and in schizophrenics. He worked persistently and rigorously to delineate this field of the abstract and concrete. Such thinking served as the core of the "organismic" point of view which his name has been, and will continue to be, so intimately associated.

National Institutes of Health DAVID SHAKOW

BOOK REVIEWS

Edited by T. A. RYAN, Cornell University

The Problem of Social-Scientific Knowledge. By WILLIAM P. MCEWEN. Totowa, N.J., The Bedminster Press, 1963. Pp. xxix, 590. $12.50.

In this book, a philosopher writes an exposition of the epistemological basis of modern social science. It is an ambitious undertaking. He analyzes the opinions and caveats of well-known representatives of psychology, sociology, anthropology, political science, economics, and history. Psychology is treated as two disciplines, characterized as individual and social psychology His principal source materials for the latter are the writings of Newcomb and Murphy; for the former, the writings of J. W. Allport, Tolman, Hull, and M. H. Marx.

The author addresses himself to such questions as: what is meant by social-scientific knowledge? and, how is it acquired? From a broad philosophical perspective, he tries to coördinate epistemology and the methodology of social science. He begins from the assumption of epistemological dualism; namely, that scientific knowledge is mediate knowledge; that the datum is not identical with the object to be known. Early in the book he makes clear the usually-overlooked distinction between the knowledge-situation and the meaning-situation. The knowledge-situation is the locus of the methodological questions of the social science enterprise, the meaning-situation is the locus of the epistemological questions. The knowledge-situation specifies the kind of knowledge that may be acquired; the meaning-situation specifies the method of acquiring beliefs and the criterion for testing them. The priority of the meaning-situation is most often neglected by social scientists who have taken 19th century physical science as the ideal model.

A significant portion of the book is devoted to an explication and defense of the epistemological-methodological distinction. A third basic conception—the value-situation—completes a paradigm for describing various perspectives in the search for knowledge, among them uncritical non-operationism (*e.g.* mysticism), naïve pragmatism, naïve-impressionism, critical non-operationism (*e.g.* pure empiricism), and two versions of operationism. An example of the use of the paradigm follows (page 78):

Value-situation:	What is the dominant motive of the pure empiricist? The desire for indisputable information about particular factual situations without reference to any *a priori* system of universal and necessary principles.
Meaning-situation:	What kind of knowledge is acquirable? Quantitatively analyzed sense perceptions of particular occurrence.
Knowledge-situation:	By what method and criterion is warranted belief acquired? By inductive inferences from observations of sense-data which are verified by experimentally testing their correspondence with facts under controlled conditions.

Although McEwen makes few original contributions about the place of values in the scientific enterprise, he does offer a two-fold classification that clarifies some of the controversial issues: radical relativism, and situational relativism. The former position declares that no objective knowledge is possible. Interpretations of any social scientist are relative to his values and are, in principle, not distinguishable from the interpretations of non-scientific claimants. Situational relativism recognizes the part played by valuations, but attempts to take into account personal bias through reflectively forming sentences which fit the requirements of the meaning situation: "The situational relativist assumes that within the specifiable limits and in accordance with the specific requirements of a given epistemological perspective testable claims can be constructed and verified as reasonably accepted to a community of reflective minds" (page 59).

As an aid to defining social-scientific knowledge, McEwen describes a "synoptic model" of behavior data. An expansion of the multi-dimensional models of Tolman and Parsons, this model has little originality nor (for the modern psychologist, at least) utility. The counsel to take into account historical, cultural, social and personality variables is too well known to require further emphasis. That many of our colleagues continue to work as if conduct could be understood as the operation of a small number of antecedents is more a reflection of scientific specialization than of restricted models.

Parts IV and V of the book would be useful to the budding social scientist interested in examining the basis of scientific inquiry. Here are detailed accounts of the epistemological pattern of the reflective process and of the methodological pattern. The latter is a detailed elaboration of the hypothetico-deductive method. The former emphasizes the need for constructing postulates in the meaning-situation. Five postulates are set down: reality; probability; system; causality; and coherence. The significance of these postulates for each of the social sciences is given.

Although helpful in systematically describing the meaning-situation, the author has not taken advantage of recent substantive work. As indicated above, the sources of the author's analysis of social science are the opinions, preferences, styles and viewpoints of widely-read writers. The significance of the causal postulate would have been more emphatic if the author had considered the distinction between rule-following explanations of behavior and causal explanations as noted by Peters in his little book on motivation. The use of Freudian metaphors, similarly, suggests a failure to look at constructs that are based on other metaphoric origins.

The author discusses the nomothetic-idiographic antinomy but fails to show how it is illuminated by applying the epistemological-methodological distinction. Allport, in favoring the use of idiographic methods, makes clear that his valuation of the individual directs him to ask certain questions emphasizing uniqueness. The methods of assessing uniqueness are conditioned by the meaning-situation. Other writers begin from a value-situation that emphasizes the virtue of nomothetic, law-like statements for predicting conduct of persons within expected margins of error. The empirical work of Meehl, Holt, Hoffman, Hammond, and the reviewer would have provided the author with a more up-to-date set of substantive illustrations than the opinions of elder statesmen.

Similarly, in discussing the postulate of coherence, the author would have been well-advised to incorporate the recent discussions of validity—the place of congruent validity and of predictive validity in the epistemological scheme.

I have mixed evaluations about this book. On the one hand, it deals with important problems some of which are ignored and neglected by psychologists (and others) in the frenetic search for truth through following the implications of the laboratory model of science. Further, it shows that the social sciences have a common set of epistemological problems. On the other hand, the book tries to do too much. To treat every principle and postulate in each of the social sciences limited the author to examining the work of a few theorists in each field. Although the opinions of these theorists are worthy of consideration, they are meager representations of the actual work being done. Admittedly, it would have been a Herculean task to analyze the epistemological bases of the significant research in all fields of the social science. Perhaps McEwen's book will stimulate others to attempt similar epistemological studies of more restricted scope—of one segment of social science at a time.

University of California,
Berkeley

THEODORE R. SARBIN

A Theory of Data. By CLYDE H. COOMBS. New York, John Wiley and Sons, Inc., 1964. Pp. xviii, 585. $14.95.

Measurement occupies a peculiar and prominant place in phychology. An early proof that scientific psychology is impossible, begins with the assertion that mental events cannot be measured. In response, several generations of psychologists have argued and demonstrated the opposite.

There are two answers to the measurement problem. One consists of providing that psychological events can be measured by going out and measuring them. The second, more sophisticated approach is to prove that they are measurable; mainly, by appropriate definition of measurement. It is to the second task that Coombs bends his efforts.

An elementary property of measurements is transitivity; if A is heavier than B, and B is heavier than C, then certainly A must be heavier than C. Since the relation on numbers, "greater than," is transitive, the corresponding relation between objects must also be transitive. Generally, to obtain measurement, one must find a set of mathematical relationships and corresponding empirical relationships that have the same abstract structure.

Much of the literature on measurement, especially that stemming from philosophy and physics, concentrates attention on the mathematical system and on the system of empirical relationships needed to attain a given mathematical structure. The elementary data, such as comparisons and other judgments, are of interest only insofar as they correspond to well-defined structures. An alternative approach is to turn the problem about and study typical observations, in the hope of finding novel structures. The observations are studied, not to draw out their psychological juices, but merely to inspect their arrangement.

Judgments, studied merely in their abstract and formal arrangements, are what Coombs means by "data." There is a similarity between judging one of two brief noises louder, and judging one of two political candidates as more liberal. In both cases, judgments can be used to classify some object (noise or candidate) on some dimension. Thus, in the usual simple interpretation, the effort is to gain a geometric interpretation of the data. Most measurement, at least in physics, is basically one-dimensional; one is interested in measuring mass or emf, or some one other property. Coombs is especially interested in multi-dimensional measurement, in which the object is at once measured on several different scales or located in a multi-dimensional space. The usual solution to this problem, factor-analysis, does not satisfy him, however, because it merely assumes that measurement is already accomplished, and does not give an opportunity to show that the objects *can* be measured in the space proposed.

What relationships must obtain among a set of data to justify placing them in a certain multi-dimensional space? If we have distances between the various objects, then factor analysis makes it possible to reconstruct the space. If our data are purely qualitative, however, the conditions are extremely complex. An important but formidable part of Coombs' book is devoted to this kind of question. Here it is most noticeable that Coombs eschews the algebraic formula; the most difficult points are transmitted by a combination of words, diagrams, and tables. Those of us who have had the pleasure of hearing Coombs speak may ache for the sight of his hands shaping the argument, for in the book many of the arguments require the most determined concentration.

An important characteristic of human judgment is that it not only compares one object with another on some property, but may also reflect a relationship between object and subject. If I endorse a political candidate, this is a signal of a certain ideological proximity, not between two objects but between me and the object. This kind of relationship is of great importance in that eternal struggle to define Unity or some other summary of preference. Coombs' approach is to throw subjects and objects into the same space, having each subject contribute an "Ideal point". Then, the subject's preferences among objects are represented by the proximities of the objects to his ideal point. In this way the preferences of different subjects can all be reconciled with a single constant arrangement of objects. Different subjects differ, not in their perception of the objects, but in the location of their ideal points.

These and other ideas of Coombs' are developed in detail in *A Theory Of Data*. A multitude of special experimental arrangements and of surprising and exciting conclusions, are put forward. For example: the general idea of unfolding is related to the existence of a social preference-scale, the basic assumption of welfare economics. Elsewhere, scaling methods are shown to clarify studies of curiosity.

There are two major problems with this book. One is the difficulty of the arguments, at least to a person like the reviewer, who most easily gathers detailed arguments from equations and steps of proof. Much of Coombs' effort, over the years, has been to formulate the problem of obtaining metric or almost-metric information from non-metric data. The problem is clearly Coombs', but the solution was obtained by Roger Shepard in his Analysis of Proximities, using methods and approaches foreign to Coombs' style. Because it deals in large part with technical difficulties now well on the way to being overcome, the book, unhappily, is somewhat outdated already.

A second problem is in the abstractness of the argument. It is the nature of Coombs' approach that he treats data with respect to form, not content. For his methods, an experiment results, not in a test or refinement of theory, or in new information about the world, but in a set of data that may be given a geometrical interpretation. The book contains an unusually large amount of data, but almost no psychological facts. This is no doubt appropriate to the topic, but nonetheless separates Coombs' contribution from the more particular and substantive hypotheses put forward by many other mathematical psychologists. For example, Coombs draws many implications from his idea of "ideal stimuli," setting the chooser in among the prizes. Experiments illustrate and use the theory more than they test or refine it. A skeptic might want more evidence of validity to justify the detailed instructions of how to analyse data using these assumptions. In the depth and toughness of his arguments, Coombs resembles the experimentally-oriented mathematical psychologists, but in his choice of problems, his affinities are with what is called "Scaling."

This book is probably the most profound in the scaling literature, the most informed by general knowledge and understanding of the problems involved. It is a reasonably up-to-date account of Coombs' important work, and also provides an interrelated overview of a variety of related experimental procedures. It is not a cookbook, nor even a cookbook with "rationales," but a mature discussion of the concepts and approaches to psychological measurement. Certainly, any general theory of measurement must take Coomb's thoughts into consideration.

Indiana University **FRANK RESTLE**

Ingratiation: A Social Psychological Analysis. By EDWARD E. JONES. New York, Appleton-Century-Crofts, 1964. Pp. viii, 211. $6.00

In this monograph a series of several experiments, most of which have appeared recently in journals, are presented and discussed in detail. They all focus on a single complex interpersonal process, attraction-seeking or ingratiation, and attempt mainly to identify the behavior displayed in such strivings. Overall research procedure is guided by the assumption that "the tactical considerations involved in managing a positive impression . . . shape those communications which have implication for gaining and maintaining esteem" (p. 191). By varying the conditions under which an individual seeks to be attractive to another and by observing changes in their communication, Jones has provided us with our first clear experimental analysis of ingratiation.

Four main features of this process are considered: (1) Self-presentation; (2) conformity; (3) other-enhancement; and (4) evaluations of a potential ingratiator. The first three describe tactics to achieve attractiveness; the last, the circumstances under which these tactics succeed. It is worth considering several of the many interesting findings. Instructed to portray themselves so as to make a good impression, subjects in describing themselves to a target person tended to stress their positive qualities and minimize their shortcomings. This straightforward and unsurprising relationship was shown to be contingent on such factors as: (1) Differences in Status-Rank. Relatively high status individuals were more modest when induced to become ingratiating, while relatively low status individuals were more self-enhancing but only in predictable respects. (2) Characteristics of the Target of Ingratiation. Attempts at creating a favorable impression with a target who preferred competence in task performance led to the communication of a different set of strengths than similar attempts with a target who valued congeniality. (3) Mutual versus Asymetrical Awareness of the Ingratiator's Dependence on the Target Person. When the target was assumed to be unaware of the ingratiator's dependence on him, self-enhancement was strong and pervasive; when, however, there was mutual awareness of dependence, the ingratiator communicated strengths in some areas and weaknesses in others, as if to establish his credibility. In certain experiments a strong effect of conformity was observed. Inducements to ingratiation produced marked movement toward agreement with the opinions of the target-person. The pattern of conformity, however, varied according to the relative status of the ingratiator, with low status individuals conforming more on items relevant to the relationship than on irrelevant items and high status individuals conforming more on irrelevant that on relevant items. The evidence on other-enhancement as a tactic was inconclusive but still interesting. A strong suggestion of a boomerang effect appeared when compliments were communicated from low to high status persons. The experiments on the evaluation of an ingratiator are the least satisfying for reasons mentioned below. They do, nevertheless, demonstrate that an innocent bystander particularly likes a person involved in an ingratiating episode if the latter is under pressure to conform but avoids slavish agreement with the target-person.

Since this monograph pushes into an amorphous domain previously unexplored by systematic experimentation, it inevitably falls into conceptual and methodological tangles. Considering the problems confronted, these difficulties are relatively unimportant. For example, there seems to be a commitment to a model which is to account for the likelihood of ingratiating behavior. It is a familiar one based on the subjective probability of success of the ingratiating act in obtaining benefits from the target-person and the incentive-value of these benefits. No explicit attempt, however, is made to test this model, *i.e.* to experimentally manipulate probability of success or incentive-value. While for the most part the model is irrelevant to the experiments, it does specify classes of variables which serve as a framework for an excellent analysis of the cognitive and motivational determinants of ingratiation (Ch. 4). This chapter along with the review of past work on ingratiating tactics (Ch. 2) are stimulating and valuable. A second very simple heuristic model was presented to account for the relationship between the judged attractiveness of the ingratiator by the target-person and the extent to which the former uses ingratiating tactics under different degrees of dependence on the target. It predicts that the ingratiator, up to some extreme point, becomes increasingly attractive as his use of ingratiating tactics increases. Under extreme usage, however, attractiveness diminishes at a rate which is positively related to the ingratiator's dependence on the target. It is recognized that the experiments again are of doubtful relevance to the model. Judgments of attractiveness are made by subjects observing an ingratiating episode, not by the target person. The retionale for this is not convincing. Moreover, an extremely narrow range of values of the independent variable, usage of ingratiating tactics, was employed. Jones reports that the obtained results conform closely to theoretical expectations. This is true for only part of the data. The finding that if a relatively small amount of ingratiating behavior occurs, the ingratiator highly dependent on the target is judged more attractive than the minimally dependent ingratiator does not conform to the model which specifies that at no point is the highly dependent ingratiator to be judged more attractive. This effect is probably due to using a bystander as judge rather than the target person.

Some questions may also be raised about methodology. Thus, is the ingratiating behavior elicited by directly instructing subjects to present themselves favorably different from behavior emitted when the decision to ingratiate is voluntary? Satisfying the demands of an experimenter probably minimizes the personal commitment and conflict accompanying most voluntary decisions to ingratiate. In the Dickoff experiment (Ch. 6, pp. 169-180) the vested interest behind an evaluation was manipulated by means of instructions. In one condition the evaluator was a graduate student in clinical psychology undergoing training in accurately forming impressions of people. In the other condition the evaluator was a graduate student who is substituting for the regular assistant and who would like very much to use the subject in a subsequent experiment if the latter would agree. The distinct possibility exists that expertise was varied as well as the extent to which ulterior motives played a part in the evaluation. Whether this confounding had effects cannot be determined.

These criticisms do not significantly detract from the importance of this monograph. It is a pioneering work which presents a masterful analysis of the tactical use of ingratiation accompanied by several experiments that are frequently ingenious and provocative. The relationship between ingratiation and a variety of traditional social psychological problems such as power, social perception, conformity, and interpresonal balance are clearly delineated. While no integrated theory of ingratiation is provided, there is a solid conceptual framework linked to work by Thibaut and Kelly, Festinger, Heider, and Newcomb, among others. On the whole Jones' monograph will be a strong impetus and fruitful guide to theory and research on a topic of social and scientific importance.

University of Michigan EUGENE BURNSTEIN

Perception. By JULIAN E. HOCHBERG. Englewood Cliffs, New Jersey, Prentice-Hall, 1964. Pp. x, 118. Cloth $3.95, Paper $1.95.

This little book is the best available introduction to perception. In a mere 118 pages it covers most of the major subjects of interest and does so in language that is understandable to a beginner. The beginner will, however, have to work hard because the brevity of exposition requires him to participate actively in the development of the basic ideas.

The beginner is not the only person who could benefit from reading this volume. Those psychologists who have a secondary interest in perception will find that its rapid-fire style and conceptual parsimony will enable them to attain a well-rounded view of what it is that perception specialists are doing and why they are doing it. This is not a mere collection of perceptual phenomena which have no connections other than the fact that the eyes are basic to their occurrence. There are several unifying themes which make sense of why the important experiments in perception were performed.

As in most textbooks, Hochberg begins with a review of the techniques and basic findings of psychophysics. He places these data, however, within the framework of structuralism. In this context psychophysical research is seen as part of a program to define sensory qualities or sensations. It was the intention of structuralism to describe the elements from which the perceptions of daily life are comprised. Another part of this same program is analytical introspection which is also given a brief treatment. It is seen as related to the doctrine of specific nerve energies which correspond to the elementary sensations.

All of this is introductory to a discussion of elementary perceptual phenomena. The subjects considered here include color vision, with clear accounts of both the Young-Helmholtz theory and the opponent-colors theory, visual acuity and eye movements. The idea is to illustrate the notion that any patch of excitation of the retina corresponds to a particular sensation regardless of the scene from which that patch was extracted. This is consistent with the structuralist's constancy hypothesis which assumes that perceptions are the sums of such elementary events. Hochberg raises the question of the adequacy of this kind of thinking for an understanding of perceiving.

This leads quite naturally to the problems associated with perceiving objects. The classical view of objects as structures of sensations is clearly described and then arguments against this view are presented. The postulate of the old core-context theory that perceptions are comprised of sensations and memory-images, or associations, is raised. The classical visual cues to depth are described quite adequately together with Berkeley's attempt to explain how these cues cause depth to be observed. This serves to show how the core-context theory operates to involve kinesthetic experience in visual experience. It also serves to allow Hochberg to introduce concepts such as accommodation and convergence and the roles that these processes might serve in depth perception. The concept of prior motor behavior as a basis for learning space perception is then questioned by reference to the visual cliff-experiments. Even here, however, the student is cautioned not to accept the results of one seemingly cogent experiment. The problem is, as usual, more complicated than it might seem at first to be. The beautiful kitten carousel experiment of Held and Hein is described in an effort to show that the nature-nurture problem is not so clearly understood when stated as an either-or proposition.

Then there is a brief but adequate introduction to the perceptual constancies. Included here is a discussion of the geometrical illusions which challenges the student to think out the possible role of the so-called monocular depth cues in their production. Hochberg is trying to get the reader to think for himself by merely suggesting the old idea that the illusions are due to constancies run wild.

The doctrine of unconscious inference is also described as is the important observation that knowledge does not affect the outcome of illusions such as the Muller-Lyer illusion and the moon-illusion. The point is made that if unconscious inference were a valid principle then the constancies would be better than they are in fact. There are discussions of shape perception and motion perception as well. The facts of motion perception, shape perception, the constancies and the illusions are used to show how the addition or constancy hypothesis fails—how structuralism is essentially inapplicable to an understanding of perception.

In this same context Hochberg also discusses perceptual learning and Hebb's attempt to account for perceptual phenomena in terms of a learning process. It is here that the subtle point is made that this kind of theory is based upon some structuralist assumptions. Cell assemblies are shown to be logically equivalent to specific receptors which are responsible for elementary sensations in the old theory. The work of Hubel and Weisel is also described and its implications for cell-assembly theory considered.

This leads quite naturally into a discussion of Gestalt psychology which was, after all, a reaction against structuralism. Hochberg sees Gestalt psychology as a program to define a more appropriate unit of analysis of perception. Here the laws of organization are described as an attempt to account for form-perception. The inadequacy of this approach is also made clear and more recent efforts to define higher order variables or units of analysis are described. These include Gibson's investigations of the optic array and his discovery that gradients of texture-density have enormous implications for space perception and the constancies. The relational determination of brightness and size, the kinetic depth effect and Hochberg's own simplicity concept are all discussed in this framework. Clearly time- and space-varying properties of the stimulus add a new dimension of complexity to perceptual analysis and, at the same time, hold out the promise of providing a new basis for understanding.

The book concludes with a discussion of social perception in which such subjects as subception, perception of facial cues to emotions and intentions, and esthetic qualities are considered. There is some doubt in the reviewer's mind as to the advisability of including the material in the book but it should, nevertheless, be of some interest to the student.

The illustrations and accompanying captions are quite good. They serve a useful instructional purpose in that they make the method and results of many experiments entirely clear without intruding too much into the essential arguments of the text.

PERCEPTION is a good book for the beginner for whom it is intended. If the reviewer were teaching an advanced course, he would use it. It could serve as a frame of reference for more intensive outside reading until such a time as it is expanded, which he hopes it will be in the near future.

Sperry Rand Research Center, Sudbury, Massachusetts LLOYD KAUFMAN

The Act of Creation. By ARTHUR KOESTLER. New York, The Macmillan Company, 1964. Foreword by Sir Cyril Burt. Pp. 751. $8.95.

If, as Arthur Koestler maintains, originality flourishes on crossdisiplinary fertilization, then one might expect great new things from a work by this distinguished novelist and essayist with some training in science, very wide knowledge of modern psychology, and a widely-read study of the history of science to his credit (or *dis*credit, as historians of science have insisted in their discussion of Koestler's earlier book, *The Sleepwalkers*).

Koestler's key cognitive concept is *bisociation:* "the perceiving of a situation or idea . . . in two self-consistent but habitually incompatible frames of reference." (p. 35). Introduced in the first section of the book, dealing with humor, the concept is used in the second and third sections, dealing with scientific and artistic creativity. At first, *bisociation* seems to smack of a revision of the notion of novel or serendipitous associations to provide room for contact between whole structures of ideas rather than piecemeal associations between elements. But as *bisociation* is used, it comes to stand for all things to all geniuses. The items to be "bisociated" may be two disciplines such as physics and astronomy, as Koestler would have it in Kepler's case; or two concepts, as when Einstein is said to have "bisociated energy and matter" (p. 233); or intersensory relationships used to achieve a poetic effect. In spite of Koestler's protestations to the contrary, bisociation closely resembles oldfashioned association applied to any unusual combination of ideas.

Like all other connectionists, Koestler must find a powerful engine to facilitate the forming of *new* associations, otherwise the associative process would be too slow or too stereotyped to account for the occurrence of novelty. The Unconscious is Koestler's mighty engine, a kind of breeder reactor in which bizarre collisions occur, often during sleep or other lapses of attention, and novelties are thrown off for the alert genius to catch. In the shifting sliding scenes of this nether-world all things happen quickly and forbidden acts easily. On waking, we see what we have done, and if we do not moan "Alas!" we may cry "Eureka!"

As Koestler works out this thesis, he repeatedly uses pejorative terms to describe normal, non-revolutionary science. Scientists who merely recognize a revolutionary idea and pursue its implications are mediocre, pedantic, over-specialized, hypercritical, and guilty of establishing a new orthodoxy. In a few excellent pages Koestler does touch on the constructive relations between normal and revolutionary science (especially pp. 257-258), but the basic tone of the book lays overwhelmingly one-sided emphasis on the role of the lonely revolutionary. Thus, for Koestler the decisive phase in creative scientific thought is an irrational, asocial act. In good part, this hypersubjectivity stems from the author's undue emphasis on theoretical work at the expense of the empirical side of science. In some contexts, this emphasis might be a useful corrective, but we should not forget, as Koestler seems to do, that Galileo and Newton were not only theoreticians but fine experimenters; and Darwin coupled his skill at theoretical synthesis with many great innovations in empirical biology. By overemphasizing theoretical work at the expense of experimental, and divorcing the two, Koestler deprives science of its major source of objectivity: the blending of observation and speculation.

It is hard to tell how much special magic Koestler intends in the number "2" implied in the word, *bisociation*. Koester's own extended illustrations do not bear out the *bi-sociative* thesis. As I read his description of Gutenberg, three processes are combined in the invention of printing from movable type. In his description of Kepler, physics decisively marries astronomy, data clash decisively with theory, and two old ideas are decisively discarded. In his description of Darwin, at least three threads are interwoven. Thus, even in these greatly schematized and simplified accounts, matters turn out far too complex for bi-sociation. If the combination of only two ideas sufficed to produce a revolution, it would be plausible that the decisive event might occur unintentionally and accidentally, as Koestler maintains. If, however, revolutions in thought are seen to require the assemblage of many carefully winnowed and shaped components in a new structure, the role of purposeful and rational work would be greatly magnified.

As one reads on, the wooliness of Koestler's concept of the unconscious seems to grow. Skilled acts which have become automatic, involuntary functions such as breathing, exploratory behavior in unfamiliar situations and many other ingredients are all thrown into the same stock-pot. Anything seems to qualify as unconscious or irrational (are these the same?) so long as it is not under the explicit control of logical reasoning in the narrowest sense. In the main, Koestler's treatment represents a muddling of the pre-conscious or Jamesian fringe of awareness with the Freudian unconscious. Not only does this mixture seriously dilute the Freudian concept, it also separates explicitly rational thought from its essential pre-conscious substratum, thereby making such thought impotent and trivial.

The book contains an impressive assembly of historical anecdotes and will be a useful source for anyone wanting to illustrate the role of the unconscious in creative work. In spite of his wealth of interesting material it is not a very readable book. One might say less about style if Koestler were not a distinguished writer, for he is not much heavier, more pedantic, or more unreadable here than the scholars he attacks for these sins. The book is strewn with bewildering neologisms, overly cute epigrams, and heavy-handed verbal mystique. More seriously, Part II seems to have been written after the author wearied of the concepts used in Part I.

After the discussions of humor, scientific thought, and the arts, the book closes with a long section, Part II: "Habit and Originality," which is largely a sketch of general psychobiology as seen by Koestler. His main theme is the biological basis of creativity, as evidenced by the numerous loose analogies he can draw between various biological phenomena and the creative process as he sees it. Koestler throws out some interesting hints as to how one might construct a theory of creativity as a growth-process. His argument in the preceding sections, however, puts so much emphasis on the *single creative act* that he has not left himself room for a careful synthesis of his ideas on growth with his earlier remarks on creativity.

New School for Social Research HOWARD E. GRUBER

Subcortical Mechanisms of Behavior. By ROBERT A. MCCLEARY and ROBERT Y. MOORE. New York, Basic Books, 1965. Pp. x, 148. $2.95.

This book is the first of several which are to appear in a series called *Basic Topics in Physiological Psychology.* If this is a representative sample, the collection should provide excellent introductory reading for students taking courses in the subject. Its value to others is possibly reduced by a paucity of references, especially as the degree of simplification required in this sort of book inevitably leads to some statements that could mislead a reader with a more professional interest in the subject. Lack of individual citations also leads to overworking of the terms "neurophysiologists think," "research workers have shown," and so on.

The temptation to equate "cortical" with "cognitive," and "subcortical" with "conative and emotional" is compelling, but should be resisted; as McCleary and Moore point out several times in the introductory chapter, all parts of the brain work together. In a series directed to psychologists, I think it would have been better to adopt a behavioral parcellation of topics rather than an anatomical one. The authors have, in fact, taken justifiable liberties with their title and deal with those parts of the brain, not all of them subcortical, that seem to be primarily concerned with motivation in a broad sense. They even manage to insinuate a section on the neocortex into the introductory chapter, claiming that the preponderance of vertical over horizontal connections is evidence for subcortical involvement in cortical function. I doubt whether this point needs arguing, or, if it does, whether this is the best argument (after all, many of the vertical connections penetrate no further than the white matter before returning to the cortex). The space might more profitably have been used to discuss in a more general way the relationship between cortical and subcortical structures.

The chapter on the anatomy of the brain, which emphasises the connections of the parts to be discussed in later chapters, the reticular system, the hypothalamus, and the rhinencephalon, is very good. It includes a number of clear and very helpful diagrams, and, in spite of its apparent simplicity, it contains a great deal of useful information.

The remaining four chapters deal, at a suitably simple level, with respiration; the hypothalamic control of eating, drinking, and making merry; the role of the ascending reticular system in waking and attention; the mechanisms of punishment and reward; and a number of limbic-system functions. The problems of presenting a readable and fair account of these complicated and controversial fields in a few pages are surely insuperable. McCleary and Moore have certainly succeeded in producing a readable version, and, given that no contradictory findings are to be cited, one that conforms on the whole to the most widely received views about the systems under review. The picture of hypothalamic drive mechanisms is close to the Stellar, Teitelbaum, Epstein one, and the treatment of the reticular system is fairly close to the version according to Lindsley and Magoun. Details of both of these almost classical models have come under attack of late and students who go on to more advanced study may be in for some surprises, but at least they will have clear schemata as a basis for whatever corrections subsequently prove necessary.

A minor general criticism is that the authors perpetuate the anatomical categorization at the level of their chapter heads, which leads, for example, to the medullary respiratory mechanism popping up rather confusingly in the chapter on the hypothalamus. It also contributes to a blurring of the distinction between the reticular formation, which is an anatomical entity, and the ascending reticular system, a psychophysiological construct which only partly overlaps the former.

Although the book occasionally presents controversial findings as facts, I have found few downright errors of any consequence. In the last few pages, however, where the authors venture into the realm of memory, I know of no experiment which supports their statement that repetitious study can overcome the learning deficit of human patients with substantial bilateral lesions of the hippocampus, and it is contradicted by the fact that one such patient failed to show the slightest improvement in performance during the course of several hundred trials on a stylus maze. Only patients with less extensive lesions seem able to profit from prolonged practice.

Notwithstanding the criticisms raised above, the authors are to be congratulated on successfully filling the need for an integrated, yet basic, treatment of this interesting and active area of physiological psychology.

McGill University PETER M. MILNER

Perception and the Conditioned Reflex. By Ye. N. SOKOLOV. New York: Macmillan, 1963. Pp. x, 309. $12.00.

Sokolov's experiments, as summarized in this book, approach perception in a fashion agreeable to the most classic behaviorism, and are yet by no means out of harmony with current American theory of perception. He measures the "little r's," the subtle organismic responses to stimuli so often invoked, but so seldom measured by traditional behaviorism. His interest in these responses is not, however, in how they enrich the external input with response-produced stimuli. Instead, he is concerned with how these responses modulate the external input, tuning the organism to novel stimuli and to "signal" stimuli.

His dependent variables are vascular, respiratory and oculomotor responses, the *GSR* and *EEG*, and occasional voluntary responses such as verbal reports and hand clenching, since he works with human *Ss*. His stimuli are drawn from the classic armory of the sensory physiologist: puffs of air, electric shock, light flashes, tones, and passive limb movements. To draw these observable elements together he has an elegant set of constructs derived from Pavlov: the reflexes of orientation, adaptation, and defense. The orientation-reflex is defined as a general sensitizing response elicited by a change of stimulation, independent of the nature of that change. The adaptation-reflex is a response specific to the modality of the stimulus, which adjusts the sensitivity inversely to the intensity of the stimulus. The defense-reflex is, again, a general organismic response, elicited by strong stimuli, which limits the action of those stimuli. These three reflexes are not identified with any single response, but rather with a characteristic pattern of activity in several simultaneously registered response channels.

The problem Sokolov deals with is how these reflexes change with experience, and thus produce a kind of perceptual learning, in the sense that the input from external stimuli is modified. He mainly concentrates on the orientation-reflex. This reflex shows, first of all, a rapid extinction compared to the others. Indeed, as the threshold of the orientation-reflex rises, on repeated stimulus presentations, that of the defense-reflex tends to lower. Secondly, when the orientation-reflex has been extinguished by a regularly timed presentation of a stimulus, it can be re-elicited simply by failing to present the stimulus at the regular time. Thus the orientation-reflex may become sensitized to a stimulus-pattern, rather than to individual stimulus-elements. Thirdly, Sokolov is able to relate this orientation-effect to a concurrent change in the adaptation-reflex. The adaptation-reflex is found to undergo a kind of autoconditioning during the repetitive stimulus-cycle, so that it begins to occur in advance of the stimulus itself. The neural response to the external stimulus, when it does come, is thus partly cancelled. On the other hand, if the external stimulus does not come on schedule, the conditioned change of sensitivity will induce a change in neural response to the *unchanged* external input (p. 144). Thus the orientation-response may be elicited. (The same mechanism might provide a basis for Ivo Kohler's conditioned negative after-effects.)

The orientation-reflex is also found to be re-activated when the repeated stimulus acquires "signal" properties, *i.e.* becomes a *CS*, either because it precedes another stimulus, or because *S* has been told to respond to it in some way. It extinguishes once again when the conditioned response is well established, thus failing to support those Guthrie-type theories which suppose that response-produced stimuli of the orientation reflex are what is connected to the conditioned response (pp. 256, 291f.). If a discriminative task is then introduced, the orientation reflex is again activated. Sokolov is thus able to use the orientation-reflex as an independent index of the conditioning process.

In relating these very interesting findings, and many others, Sokolov does not present statistical evidence, but makes the reader depend on the representativeness of sample response-records. Some of his experimental procedures seen inadequate, *e.g.* presenting stimuli for verbal detection at fixed intervals, with no catch trials (p. 226); others seem ingenious but problematical, *e.g.* estimating transient thresholds from the reaction-time to a single stimulus (p. 234). The text has some annoying lapses, in the form of highly redundant passages, and unexpected transitions. On the other hand, it has a clear and very logical over-all organization, and the diagrams are excellent.

Smith College JOHN C. HAY

Stability and Change in Human Characteristics. By BENJAMIN S. BLOOM. New York, John Wiley & Sons, 1964. Pp. vii, 237. $7.00.

Over the past thirty-five years a considerable amount of longitudinal data have accumulated from several research centers throughout the United States. The high degree of interest in the research reported by these centers reflects the importance social scientists attribute to the longitudinal model as a source of primary empirical data for documenting theories and developing hypotheses for understanding human behavior. Benjamin Bloom has made a major contribution in his book, "Stability and Change in Human Characteristics," by bringing together and organizing the results from several of the major longitudinal studies. In this book he presents a model for understanding and predicting the course of development for various human characteristics. This model is based on three propositions, which simply stated are: (1) as a characteristic progresses toward its full maturity it becomes increasingly related to subsequent measures of that characteristic; (2) change-scores are unrelated to initial status in a characteristic but are due to variable environmental conditions; (3) the environment has its greatest effect during the period of most rapid growth of a characteristic, and conversely, as the developmental curve for a characteristic asymptopes it becomes more resistant to change (from environmental influences).

The author evaluates the proposed model by applying the propositions, in quantitative terms, to developmental data drawn from several of the long term longitudinal studies. Statistical relationships and curves are presented to show how closely existing developmental data fit the theoretical curves derived from the model. Longitudinal data on physical characteristics, intelligence-tests, achievement scores, and personality ratings are, each in turn, presented and evaluated within the conceptual framework outlined by the author. The facts are in excellent agreement with the developmental curves predicted by the model for physical and intellectual growth data. His analyses of the stability of scores on achievement-tests are not as concordant with the model, but here the longitudinal data are incomplete and the instruments are not as well designed (often by necessity) to provide scores for plotting growth-curves for unitary characteristics. When the author begins discussing the stability of personality characteristics, such as aggression and dependency, one begins to feel that he is arguing more from conviction and earlier established premises than from the patterning of results, which in many instances are contrary to his thesis. He deals with these inconsistent findings by attributing them to inadequacies in the collection of the data and to the superficial nature of the personality measures employed. Although these points are well-taken, his own model seems untenable for explaining these phenomena since there is little basis to assume that many personality variables follow the accumulative growth pattern that he contends exists. For instance, he purports that half the growth of "dependence in females" seen at approximately 20 years of age is obtained by age 4, based on the magnitude of the correlation for dependency scores between these two ages. This correlation in essence says nothing about growth but only indicates the relative position that is maintained by individuals in the sample (Bloom acknowledges this point but then overlooks it in presenting his analysis.) Although it would be difficult to document, many psychologists would argue that in fact there was a decrement in dependent behavior between these two age-periods.

This book represents a needed step in the direction of organizing and integrating empirical data gathered from longitudinal studies in order to derive testable theories of development, determine optimal periods for effective intervention, and enhance our ability to predict the potential of the individual. In addition, some highly desirable suggestions are made in the book for improving measurement techniques and for conceptualizing and assessing environmental variables.

Child Research Branch,
National Institutes of Health.

HOWARD A. MOSS

Language and Thought. By JOHN B. CARROLL. Englewood Cliffs, New Jersey, Prentice-Hall, 1964. Pp. v, 118. Cloth $3.95, Paper $1.50.

This is a good book. It is simultaneously comprehensive, accurate and up-to-date. At its modest cost, it is well worth purchasing even for those only minimally interested in the language processes. For those actively engaged in the field, from the psychologist to the speech pathologist and their students, it is perhaps the best basic overview of language and its relations to complex symbolic activity this reviewer has seen.

Carroll stints no aspect of linguistic behavior. He shows remarkable conscientiousness concerning detail for a volume meant to be an elementary survey text. It is, in fact, elementary only with respect to the clarity of the exposition and its limitation on space.

His choice of title alone shows commendable insight into the central issue of a book which might, more prosaically, have been entitled psycholinguistics. The relation between language and thought is both intimate and complex. A thorough comprehension of this volume will acquaint the reader with the former and clarify the latter.

Carroll belongs to that relative rarity among psychologists; he is a first generation psycholinguist. He was influential in establishing the committee on Language and Psychology of the Social Sciences Research Council, the committee initiating the psycholinguistics monograph edited by Osgood and Sebeok which gave form and substance to the then new field. It is perhaps most significant that this book should now reside, without apology, next to the more traditional titles of the 13 other volumes of the *Foundations of Modern Psychology Series* edited by Richard Lazarus.

Because of the limitations imposed by the format of the Foundations series, the book cannot stand as a text without additional reading materials. What the book does cover, however, is covered well. Beginning with the acoustic raw material of spoken language, Carroll discusses the segmentation and representation of language through phonemes to complex transformational constructions. With the tools of description thus provided, he discusses what knowledge we have about language acquisition in the child and its dissolution in disorder, ending with an appraisal of the relations of concept formation and cognition to linguistic processes. Adequately but not overly documented by experimentation, the theoretical exposition is soundly neo-behavioristic and cautious. Controversy with regard to this approach is neither avoided nor cursorily dismissed.

It is well worth having such a book from one who is so intimately associated with the development of the area.

University of Illinois MURRAY S. MIRON

Social Psychology. WILLIAM W. LAMBERT and WALLACE E. LAMBERT. Englewood Cliffs, N.J., Prentice-Hall Inc., 1964. Pp. vii, 120. Cloth $3.95; Paper $1.50.

This little book is intended as a text for the social psychology unit of an introductory course in psychology. It is one of a series of short volumes, each a self-contained book dealing with a particular aspect of psychology. The entire series is said to cover the "full scope of psychological thought, research, and application."

If the quality of the Lamberts' book is representative of the entire set, students exposed to this type of texual materials are in for a stimulating experience. Except for the first chapter which is not very informative, the authors have presented social psychological research and concepts in a manner that is informal but succinct. Their enthusiasm is apparent, and it is communicated in a way that expresses the excitement of intellectual inquiry. Unlike many textbooks, this one stresses very recent developments. A large percentage of the studies that are summarized to document the authors' assertions are no more than three or four years old, and the issues that receive major attention are usually those that are central to current research and theory.

Like all compacts, however, this model lacks features that some users may desire. Heavy emphasis is placed upon the influence that interpersonal transactions have on the individual's perceptions, attitudes, and actions. Less attention is given to influences that flow in the opposite direction, and very little concern is shown for relationships between interpersonal transactions and the properties of the collectivities in which they occur. Thus, although the book reflects the *accomplishments* of social psychology rather accurately, it tends not to familiarize the reader with vast problem areas in which successes have been more meager. Except in a final chapter dealing with personality and culture, the focus is on the individual. The student is unlikely to gain a deep appreciation of the possibilities and difficulties of approaching human behavior at levels that are more complex than the individual and less elusive than the society. He will have limited understanding of attempts to treat the group as a system, only primitive notions of the concepts of role and social organization, and rather vague insights into the structured character of person-to-person transactions.

This, however, is a very good book. Students should learn a great deal from it, and will probably form favorable, even if slightly biased, conceptions of social psychology. Instructors should find that it stimulates productive discussion and encourages critical thought. Probably this is more than is accomplished by the relevant chapters of most conventional texts.

University of Illinois IVAN D. STEINER

Psychology Applied to Industry. By MARVIN D. DUNNETTE and WAYNE K. KIRCHNER. New York, Appleton-Century-Crofts, 1965. Pp. viii, 262. $2.50.

In their preface, the authors accurately characterize this book "as a collection of brief essays about the present accomplishments of psychologists in industry." They envision it being used as a supplementary textbook in general psychology courses, or as a main text for courses addressed to non-psychology majors or possibly in courses in industrial psychology when enriched liberally by additional readings. These are rather modest objectives.

The introductory chapter sets the tone by stating that the primary aim of the field is to introduce the scientific method as the basis for decisions involving human behavior or the utilization of human resources. This is to be accomplished by psychological research, the results of which will presumably serve as data for enlightened executive decisions and actions. The book does not consider the difficulties and frustrations commonly experienced in trying to implement this traditional role of the industrial psychologist, nor does it examine possible alternative ways in which the psychologist may serve as a change-agent in industry.

The remaining eight chapters, or "essays," each cover an area toward which industrial psychologists have turned their attention; namely, personnel selection, training, counseling, equipment and systems design, motivation, organization, labor-management relations, and consumer behavior. In each instance, an attempt is made to describe briefly the main objectives and methods which characterize the topic and to describe in a few pages on or two illustrative studies. The constraints of brevity and simplicity create problems of sampling and penetration with which the authors generally copy reasonably well. On the other hand, one cannot but wonder why they elected to devote approximately twenty-five precious pages to necessarily superficial treatments of statistical and psychometric methods, plus most of a chapter and a seven-page appendix to survey research techniques, while omitting or barely mentioning such major subjects as performance ratings, sensitivity training, financial and other formal incentives, and problems of consumer-behavior, and methods other than those involving survey research.

What the authors do treat, they treat knowledgeably and in a clear and interesting style.

New York University

RAYMOND A. KATZELL

Statistics for Psychologists, By WILLIAM L. HAYS. New York, Holt, Rinehart, and Winston, 1963. Pp. xvi, 719. $10.75.

In the preface, the author states that this book is designed as a text of an intermediate level of difficulty "in a relatively nonmathematical form, but in somewhat more detail than is customary in texts designed for psychologists, and with considerably more emphasis on the theoretical rather than the applied aspects of the subject." One goal is to provide the research psychologists with enough of the language and concepts of the mathematical machinery to enable him to communicate effectively with a professional statistician.

The material is certainly there, and Hays has done an admirable job of verbalizing the mathematical implications in an attempt to articulate with the evolving sophistication of the reader. Probability is handled from a set-theoretical point of view following a fairly detailed review of elementary set-theory. There are included sections on such topics as the algebra of expectations, statistical decision-theory, Bayesian statistics, as well as the material traditional in a text at this level through two-way analysis of variance with a well-organized discussion of fixed, mixed and random models. Throughout, care is taken to introduce and define the related concepts arising from mathematical considerations, *e.g.* unbiasedness, consistency, relative efficiency, and sufficiency in the section on point estimation.

It is at this level and in sections in which material is related to a geometric framework that the enterprise seems to bog down. Intuitions at this level are largely mathematical and require an adequate background if the explanation is not to sound like some magical incantation. One gets the feeling that a student having sufficient sophistication to come to grips with much of this material would have been well-advised to take a sequence of courses in the department of mathematics or statistics.

Can a person using it more as a reference get anything from the book on his own? The answer is undoubtedly yes, providing he works hard enough at the fundamentals provided.

Duke University

C. ALAN BONEAU

Reference Groups: Exploration into Conformity and Deviation of Adolescents. By MUZAFER SHERIF and CAROLYN WOOD SHERIF. New York, Harper and Row, 1964. Pp. xiv, 370. $6.00.

This book reports the study of the ongoing interactive processes of twelve informal adolescent groups, occupying a range of socio-economic levels, by "participant-observers" who attached themselves to these groups in various ways with a minimum of interference. The project is timely, for the science of small groups is sorely in need of an adequate inductive phase, based on descriptive data obtained in naturalistic settings with a minimum of contamination by the experimenter and his biases. Unfortunately, however, the Sherifs chose not to approach their study in this way, but attempted to fit their data to a rather formidable set of preconceptions. Most of the text consists of elaborate and often redundant statements of theory, bolstered, in large part, by empirical support of an anecdotal nature. This was not the most suitable use of the descriptive-naturalistic approach, for the hypotheses were generally inadequately tested, the potential of the study for generating new insights about groups was largely lost, and one emerges with the sense that although the observations were comparatively free of observer-bias, this was offset by interpreter-bias.

The first brief chapter, titled "Grist for the Mill," is a notable exception, for here the Sherifs demonstrate their excellent skills for reporting the raw material of their study with some vivid samples of the groups in action. It thus becomes doubly regrettable that the remainder of the book does not contain more "grist" and less about the "mill."

State University of New York at Buffalo IRWIN SILVERMAN

The Unconscious in Social Relations: An Analysis of Unconscious Processes in Personality, Society, and Culture. By OTAKAR MACHOTKA. New York, Philosophical Library, 1964. Pp. xxiii, 322. $6.00.

This is an interesting survey of unconscious functions other than those discussed by psychoanalytic investigators. No monolithic theory is expounded, instead a very wide array of data is reviewed and organized to help bring coherence into what has traditionally been a neglected field. The chapter headings range from subliminal and marginal experiences; through mental set, learning and thinking; on to attitudes, values, and personality and culture process. In each section the significance of unconscious processes as revealed in experiments, as well as in more general studies and discussions, is examined.

Here we have the report of a scholarly exploration which can be highly stimulating to an open reader, and provide the point of departure for countless studies of a more conventional design. The fact that unconscious processes have not been considered an organized field of inquiry, except by the "depth" psychologists, may be an historical oversight. This book could be important; at any rate, it should not be overlooked even though some may consider it a superficial approach.

The City College of
the City University of New York EUGENE L. HARTLEY

BOOKS RECEIVED

(The books listed here have not as yet been noted in our pages. Listing here does not, however, preclude their later review.)

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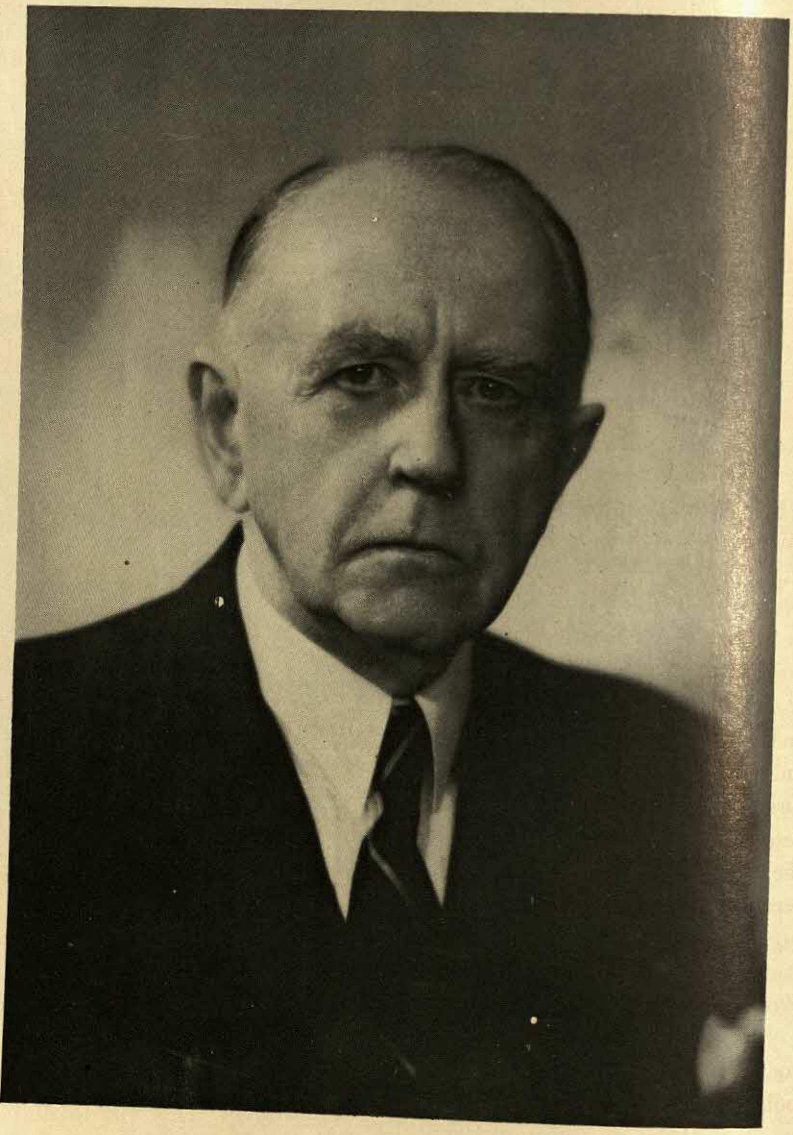

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LINGUISTIC AND NONLINGUISTIC PROCESSES IN LEARNING AND MEMORY-INTERFERENCES

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In an earlier study of retroactive interference, three levels of functional unity (*FU*) of the interpolated list were compared on (1) the rate of acquisition of the interpolated material itself, and (2) the amount of retroactive interference (*RI*) generated under each condition.[1] In this study, *high* unity was represented by a series of words forming two complete and meaningful sentences; *intermediate* unity, by these same words rearranged to form a series of apparently disconnected phrases; and *low* unity, by a random order of these words. Results of this study seemed to indicate that, while *rate of acquisition* of the interpolated list (*IL*) varied *directly* and *continuously* with functional unity (*FU*), *amount of interference* varied *inversely* and *discontinously* with level of unity. While the theoretical issues which these findings raised for both the S-R interference and the Gestalt assumptions about the processes underlying learning and forgetting were discussed in some detail, the author himself was unable, at that time, to offer any alternative theoretical solution to the problems posed.

In this earlier study, level of practice on the interpolated task was held constant—all the *Ss* receive exactly eight anticipation-trials during interpolated learning. The present study was, therefore, designed to investigate

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[1] S. B. Brent, Organizational factors in learning and remembering: Functional unity of the interpolated task as a factor in retroactive interference, this JOURNAL, 78, 1965, 403-413.

the generality of the relationship between unity and memory interference across different *levels of practice* on the interpolated task—a factor known to influence retroactive interference in other studies of this type.[2] The results of the study suggest that the distinctions between linguistic and nonlinguistic processes provide a useful approach to understanding the role of functional unity in both learning and memory interference.

METHOD AND PROCEDURE

The methods and procedures in this study were the same as those reported earlier, except for level of practice on the interpolated material. For convenience, these procedures may be summarized briefly.

A classical paradigm of retroactive interference was used. The *Ss* first learned a list—(original learning (*OL*))—to one perfect anticipation (method of serial presentation). They were then arbitrarily assigned to learn either the *high-*, the *intermediate-*, or the *low-FU* of the interpolated list, for either 2, 4, or 8 anticipatory trials. After interpolated learning, all the *Ss* relearned the original material (*OL*) to criterion.

Materials. The original list (*OL*) was composed of 18 common English words arranged in a meaningless order: *fellow it ran last expected race fifty this is to yesterday tomorrow the again he do yards in.*

For the low-*FU* condition of the interpolated list (*IL*) these same words were rearranged in yet another meaningless order: *tomorrow last yesterday yards it again he fellow to do is expected race fifty in this the ran.*

For the intermediate *FU*, these words were arranged so as to increase the likelihood that subportions of the list would be perceived as meaningful phrases, while the list as a whole would *not* readily be perceived as a complete sentence: *fifty yards this fellow yesterday in the last race ran is tomorrow he expected it again to do.*

For the high-*FU* condition, these words were arranged to form two meaningful sentences: *this fellow ran fifty yards in the last race yesterday tomorrow he is expected to do it again.*

Subjects. There were 14 *Ss* in each of the nine experimental groups. The *Ss* were students in Introductory Psychology.

Procedures. Items were presented on an MTA-100 teaching machine at the rate of 4 sec. per item. Each list began with an asterisk and ended with a blank interval. There was approximately a 20-sec. interval between original (*OL*) and interpolated (*IL*) learning, and between interpolated (*IL*) and relearning (*RL*), during which a few brief sentences of instructions were read to the *S*.[3]

RESULTS

An analysis of variance showed that experimental groups did not differ on trials to criterion during original learning ($F = 1.00$, $df = 8/117$).

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[2] See, for example, A. W. Melton and J. M. Irwin, The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses, this JOURNAL, 53, 1940, 172-203.

[3] For verbatim instruction, see Brent, *op. cit.*, 405.

(1) Functional unity and retroactive interference. Two indices of retroactive interference (*RI*) were computed: percentage of items correct on first trial of relearning, and trials to criterion during relearning. Since these two measures were highly correlated and led to essentially the same conclusions, only errors on first trial of relearning are reported here. Fig. 1 shows amount of retroactive interference as a joint function of level of unity of the *IL* material and the amount of practice on the *IL* task.

(a) An analysis of variance resulted in significant main effects for level of *practice* on the interpolated task ($F = 22.55$, $df = 2/117$, $p < 0.001$) and for level of *unity* of the interpolated material ($F = 26.50$, $df = 2/117$, $p < 0.001$), as well as a significant *interaction* between level of practice and level of unity ($F = 4.10$, $df = 4/117$, $p < 0.005$).[4]

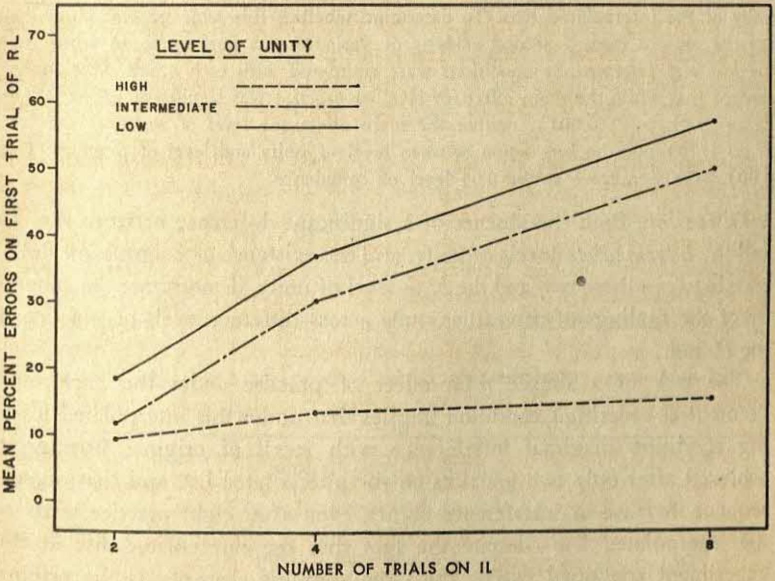

FIG. 1. RETROACTIVE INTERFERENCE
Percentage of items incorrectly recalled on first trial of relearning. Each point is the mean of 14 *S*s.

(b) Because of the significant interaction, an analysis of simple main effects was performed. *Level of practice* on the interpolated task proved to have a significant

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[4] This analysis of variance was performed twice: first on the retroaction scores themselves, and then upon an *arcsin* transformation of these scores. The *F*-ratios were in every case very close in magnitude, hence only the ratios based upon the untransformed scores are reported here.

effect upon *RI* under the *intermediate* ($F = 16.94$, $df = 2/117$, $p < 0.001$) and the *low* ($F = 17.30$, $df = 2/117$, $p < 0.001$) unity conditions, but did not have any significant effect upon amount of *RI* under the *high* unity condition ($F = 1.00$). *Level of unity* of the interpolated material proved to have no significant effect upon amount of *RI* after only *two* interpolated practices ($F = 1.05$), but did have a significant effect after *four* ($F = 6.53$, $df = 2/117$, $p < 0.005$) and after *eight* ($F = 23.17$, $df = 2/117$, $p < 0.001$) interpolated practices.

Taken collectively, these results indicate that increased practice on the *IL* results in increased forgetting of the *OL* for both the *intermediate* unity and the *low* unity of the interpolated lists, but does *not* result in any increase in forgetting under the *high* unity *IL*.

(c) One of the striking features of the results reported in the earlier study concerned the lack of any difference in *RI* between the *low* and the *intermediate* unity of the interpolated lists. To determine whether this same generalization held for the present data, a second analysis of variance was performed, in which only the *low* and *intermediate* conditions were compared with each other. This analysis showed that while the main effect for level of *practice* was significant ($F = 253.68$, $df = 2/79$, $p < 0.001$), neither the main effect for level of *unity* ($F = 2.45$, $df = 1/79$) nor the *interaction* between level of unity and level of practice ($F = 0.00$) were significant at the 0.05 level of confidence.

Discussion. Both the absence of a significant difference between the *low* and the *intermediate* levels of unity, and the existence of a significant difference between these two and the *high* level of unity, demonstrate the generality of the findings of the earlier study across different levels of practice on the *IL* task.

The lack of a simple main effect of practise under the *high* unity (sentential ordering) condition implies that under this interpolated learning condition maximal interference with recall of original learning is achieved after only two practices on the interpolated list, and that no subsequent increase in interference occurs, even after eight practice trials on the interpolated list—despite the fact that the interpolated list in this experiment contained exactly the same stimulus elements as the original list, only arranged in a different order. This finding, it seems to us, poses a serious theoretical problem for any explanation of retroactive interference which focuses primarily on the nature of the stimulus elements themselves, while ignoring the internal, structural properties of the list in which these items are embedded.

(2) Functional unity and rate of interpolated learning. In our earlier paper, the lack of difference in *RI* between the *low* and the *intermediate* unity led to the hypothesis that the *Ss* may not have 'perceived' the differences in structure which *E* had attempted to build into these two lists. In an attempt to evaluate this hypothesis, we reasoned that: if *S* did not perceive

the difference between *low* and *intermediate* unity, then there should be no difference in the rate at which these two lists were learned during the *IL* phase of the retroactive task. Analysis of interpolated learning showed, however, that the *intermediate* list was learned significantly faster than the *low* list. We therefore concluded that the *S*s did, in fact, 'perceive' a difference between these two lists *during* the *IL* phase of the task, despite the fact that this difference failed to result in differential interference during the relearning phase.

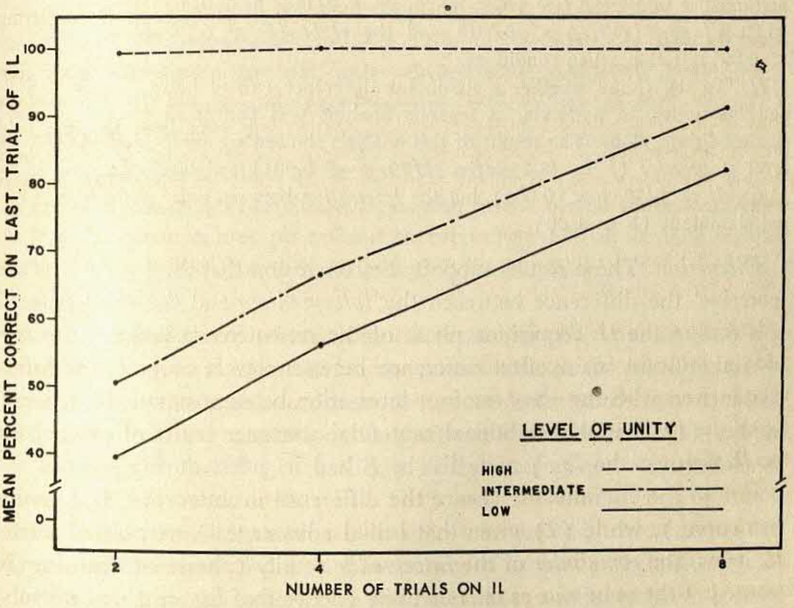

FIG. 2. INTERPOLATED LEARNING
Percentage of items correctly recalled on last trial of interpolated learning. Each point is the mean of 14 *S*s.

The lack of any difference in *RI* between the *low* and *intermediate* conditions of the present study raises exactly the same question regarding *S*s' 'perception' of the difference between these two conditions. We have attempted to answer this question by turning, once again, to the analysis of the rate of learning the interpolated list *during* the *IL* phase of the task.

(a) Fig. 2 shows the percentage of items correctly anticipated on the last trial of interpolated learning, as a function of level of practice on the interpolated task and level of unity of the interpolated materials. Analysis of variance of these data showed significant main effects for level of *practice* ($F = 48.30$, $df = 2/117$, $p <$

0.001) and for level of *unity* ($F = 111.89$, $df = 2/117$, $p < 0.001$), as well as a significant *interaction* between practice and unity ($F = 11.65$, $df = 4/117$, $p < 0.001$).[5]

(b) Because of the significance of the interaction, an analysis of simple main effects was performed. Level of *practice* had a significant effect upon level of performance under the *intermediate* ($F = 33.33$, $df = 2/117$, $p < 0.001$) and the *low* ($F = 38.25$, $df = 2/117$, $p < 0.001$) unity conditions, but not under the *high* unity condition ($F < 1.00$). Level of *unity* had a significant effect upon level of performance under the *two* ($F = 80.28$, $df = 2/117$, $p < 0.001$) the *four* ($F = 48.50$, $df = 2/117$, $p < 0.001$) and the *eight* ($F = 6.39$, $df = 2/117$, $p < 0.005$) trial practice conditions.

(c) To determine whether a significant difference existed between *intermediate* and *low* unity, in particular, a separate analysis was performed comparing these two conditions alone. The results of this analysis showed significant main effects for level of *practice* ($F = 48.25$, $df = 2/79$, $p < 0.001$) and level of *unity* ($F = 11.03$, $df = 1/79$, $p < 0.005$), but the *interaction* between unity and practice was not significant ($F = 1.00$).

Discussion. These results support the conclusion that the *S*s did, in fact, 'perceive' the difference betweeen the *intermediate* and the *low* unity *IL* lists during the *IL* acquisition phase of the retroactive task. The existence of a significant main effect difference between levels of unity, taken in conjunction with the absence of an interaction between unity and practice, implies (1) that the additional sentential structure built into the *intermediate* (over the *low*) unity list by *E* had its effect during *S*'s first exposure to the stimulus-list (hence the difference in intercept betweeen the two curves), while (2) given that initial advantage in acquisition of the *IL* items, the *remainder* of the *intermediate* unity (phrase order) list was learned at the same *rate* as the *low* unity (hence the identity of the slope in their learning curves). The *high* unity (sentential order), on the other hand, seems to have been learned in its entirety after only a single practice on the *IL* task.

GENERAL DISCUSSION

The results of these studies suggest both some basic concepts which may be useful in further structural analysis of serial learning, and, suggest certain more general hypotheses concerning the processes underlying the role of functional unity in learning and remembering.

In our earlier paper, the conceptual level of functional unity was defined as referring to "the degree to which the functions of each of the elements in a serial list contribute to and are determined by some supraordinate functions (or functional rules) governing *S*'s organization of the list as a whole."[6] In the *high* unity interpolated list of the present studies, the supra-ordinate functional rules governing the organization of the list are the semantic and syntactic rules of the language itself. The supra-ordinate function—which may, in this case, be presumed to govern *S*'s organization of the list as a whole—is that of expressing or conveying the meaning of the sentences so formed. Finally, the functions of each of the elements in this list both contributed to and were (to some extent) determined by this supra-ordinate meaning-conveying function of the list as a whole. The *intermediate* and *low* unity interpolated lists differed from the *high* unity list in that (1) only selected subsequences of items in the list conformed to the syntactic and semantic rules of the language, and (2) one was hard-put to discover any supra-ordinate function of meaning governing the organization of the list as a whole.

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[5] See Footnote 4.

Taken in the context of this analysis, the results of the present series of studies suggest certain empirical and theoretical generalizations concerning the role of functional unity in learning and remembering of lists of conventional words.

Linguistic analysis of stimulus-lists. Since the specific application of the concept of functional unity was, in the present case, based upon the subordination of the sequence of elements in the lists to the syntactic and semantic rules of the conventional language, interpretation of the stimulus-lists in linguistic terms will provide a primary basis for interpretation of the results.

Any serial list of conventional words can be divided into a number of discrete segments such that (1) the sequence of words within each segment is consistent with the semantic and syntactic rules of the language, while (2) the sequence of units does not, itself, conform to these rules. Each segment may be either linguistically complete, or linguistically incomplete. Linguistically complete units are, of course, *meaningful sentences,* while linguistically incomplete units are *fragmented sentences.* Each such sequence may, itself, be viewed as a functional unit with respect to the word-elements of which it is constituted.

From this point of view, the *high*-unity interpolated list may be seen as constituting but a single linguistic unit—one in which the functions of each of the items in the list both contributes to and is determined by a supra-ordinate function of the list as a whole, viz. that of conveying the meaning of the sentence.

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[6] Brent, *op. cit.,* 403.

Intermediate unity may be divided into 9 linguistic fragments: *fifty yards / this fellow / yesterday / in the last race / ran / is tomorrow / he expected / it again / to do.* The low unity may be divided into 11 fragments: *tomorrow / last yesterday / yards / it again / he / fellow / to do / is expected / race fifty / in this / the ran.* Finally, the original list may also be divided into 11 fragments: *fellow / it ran last / expected / race fifty / this is to / yesterday / tomorrow / the again / he do / yards in.*[7]

Functional unity and serial anticipatory learning. The data for the relationship between functional unity and rate of serial learning (Fig. 2) suggest the following set of assumptions about the underlying learning processes:

(1) A serial list of conventional words is first divided up (by *S*) into a series of functional units, such that (a) the order of items *within* each unit conforms to the syntactic and semantic rules of the language, while (b) the serial order *between* such units does *not* conform to these rules.[8] (The single, isolated word is, of course, the limiting case of such a functional unit.)

(2) The order of items within each unit is learned immediately, on the first trial of anticipated learning.

(3) Once learned, each unit, as a whole, functions as but a single 'item' of the serial list (equivalent, in this respect, to an isolated word which does not belong to any such unit).[9]

(4) During all of the remaining trials of anticipated learning, *S* is primarily concerned with the formation (*i.e.* invention or discovery) of *ad hoc,* idiosyncratic,

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[7] Certain of these partitions while not *absolutely* regular (in the linguistic sense) are close enough to being so as to justify the present method of division. Thus, for example, while the sequence *the again* is, strictly speaking, both syntactic and semantic nonsense, protocols taken from *Ss* indicate that the word *the* followed by virtually any other word except another article or a pronoun is generally perceived by *S* con constituting a linguistically *possible* functional unit. Or, again, a phrase such as *he do,* while failing to show agreement of person, is nonetheless perceived by *S* as constituting a linguistically functional unit. The reader will find that although some alternative linguistic partitionings of each of the lists is possible, the *average* number of sub-units resulting from all reasonable partitionings will not deviate markedly from those which result from the present method of partitioning. In subsequent investigations a pretest of the stimulus-materials could certainly be used to determine more accurately the most probable number of subunits characteristic of each list.

[8] Cf. G. A. Miller's concept of "chunking" as an aid to increasing the memory-span (G. A. Miller, The magical number seven, plus or minus two: some limits on our capacity for processing information, *Psychol. Rev.,* 63, 1956, 92 ff.). From the "information theory" point of view, we might say that conformity with linguistic rules serves as an "information reducing" process. The lower the level of complexity of the list, the easier it is to learn.

[9] Some recent work by J. G. Martin and his associates has demonstrated that linguistically lawful word-sequences function as a single item in paired associate learning tasks (J. G. Martin, and R. L. Jones, Size and structure of grammatical units in paired-associate learning at two age levels, *J. exp. Psychol.,* 70, 1965, 407-411; and J. G. Martin, J. R. Davidson, and M. L. Williams, Grammatical agreement and set in learning at two age levels, *ibid.,* 70, 1965, 570-574).

relationships between those 'items' (isolated words or linguistic fragments) which cannot be ordered according to conventional linguistic rules.[10]

Certain logical implications of these assumptions are immediately testable, in terms of the data already reported (Fig. 2).

Y-intercept. In a serial anticipatory task, performance on the second anticipatory trial is, in effect, a test of that learning which took place on the first trial of anticipated learning. *If,* during the first trial on anticipated learning, *S* learned only those relationships which do, in fact, conform to the rules of the language, while he does not learn any relationships which do not conform to those rules, *then,* on the second trial he should not be able to correctly anticipate the *initial* item of any subunit of the list, but, given that initial item, should be able to correctly anticipate all of the remaining items in that subunit.

This conclusion leads to a very specific mathematical prediction: viz. that the proportion of items correctly anticipated on the second trial of relearning should be expressed by the formula $P_1 = \Sigma(n_i - 1)/N$ where P_1 = proportion correct after first trial of anticipation; N = total number of items in the list; n_1 = the number of items in the *i*-th subunit of the list; and summation takes place over all such subunits. The following list compares the expected and observed values of P_1 for each level of unity of the interpolated list.

Level of unity	Expected	Observed
Low	38.8%	39.6%
Intermediate	50.0%	50.0%
High	94.4%	99.2%

The closeness of the observed to the expected values gives some support to the validity of the underlying assumptions, and suggests the potential value of a parametric study devoted specifically to exploration of number of functional subunits as a factor in serial learning.

Slope of the learning curves. The virtual identity of the slopes of the learning curves for the *low* and the *intermediate* unity conditions suggests that once the effects of division of the list into functionally autonomous subunits has been achieved, the *relationships between these subunits* are learned at about the same rate for both lists. This implies that the processes involved in learning the unstructured remainder of the list may be very similar in both cases, regardless of the nature or number of the linguistically structured subunits already formed. This observation lends credence to Assumptions 3 and 4, above. Since, on the other hand, by Assumptions 1 and 2, the *high* unity list consists of but a single functional unit which includes all 18 items, it is not surprising that such a single unit should require but a single trial for learning. Given this assumption, the difference in slope between the *high* unity and the two lower unities follows directly from the upperbound nature of the learning curve.

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[10] Protocols of the *Ss*, taken after the completion of relearning, indicated that these nonlinguistic relationships which they formed between units of the list took on one of three forms: (a) similarity in sound or appearance of the word itself (*e.g.* for the pair *yesterday-yards,* one *S* reported that he recalled them, "because both began with the letter *Y*"); (b) quasi-linguistic transformation (*e.g.* for the sequence *fifty-yards-this-fellow-yesterday-in-the-last-race-ran,* another said, "The way someone with a foreign accent would say it."); and (c) similarity of meaning (*e.g.* for the sequence *yesterday-tomorrow,* still another *S said,* "They both refer to the same thing"). These three categories are here only meant to be illustrative. They do not, by any means, exhaust the types of nonlinguistic relationships which the *Ss* may form between units.

In summary, this analysis suggests that two different types of processes may be involved in learning serial lists of conventional words. The *first* is involved in learning those word sequences which are *consonant with* the linguistic rules of the conventional language. These sequences are learned immediately, on the first trial of anticipated learning, and function as individual 'items' for purposes of subsequent learning. The *second* process involved is the learning of those sequences of items which do not conform to the conventional linguistic rules. Learning these sequences involves a gradual process in which *S* attempts to form (discover or invent) *ad hoc*, idiosyncratic rules which will aid him in determining the order in which these items should occur. It should be noted that, in addition to having to form such *ad hoc* rules of ordering, *S* must also be able to remember which of these rules applies to which particular sequence of items—a task which may be assumed to involve no small effort when the rules themselves are often highly arbitrary in nature.

Functional unity and memory interference. The data for the relationship between functional unity and memory interference, taken in the context of the foregoing analysis of the process of interpolated learning itself, suggests that the primary factor influencing amount of retroactive interference in these studies may be a *process* factor rather than either a *structure* factor or a *content* factor.

A process factor is one which is specifically associated with the *act* or *process* by which a set of material is learned. The data of interpolated learning itself suggests that two such processes were operative in the present case: a process of learning the relationships between elements within a linguistic fragment, and a process of learning the relationship betweeen the linguistic fragments themselves. The first process was shown to be virtually complete by the end of the second trial of interpolated learning; the second process was assumed to progress gradually over the remaining trials of interpolated learning. Finally, it will be recalled that the list with *high* unity was assumed to be learned by the first process alone, while the lists with *intermediate* and the *low* unity were assumed to be learned by a combination of the two processes.

Approached from this point of view, a comparison of the data from interpolated learning (Fig. 2) with that of interference (Fig. 1) suggests that the distinction between these two processes may be a partial explanation memory interference itself. Three characteristics of the interference data are particularly relevant here.

(1) After only two practices on the interpolated list, there is no significant difference between conditions in terms of the amount of interference generated: all conditions generated about 13% *RI*. At the same time, two practices on the interpolated list resulted in the greatest discrepancy between level of unity in terms of 'degree of learning' the interpolated materials. This suggests that in the present experiment, degree of interpolated learning *per se,* is *not the* most critical factor in determining amount of *RI* under this two trial condition. On the other hand, if we assume, as we did above, that the first trials of interpolated learning are, under *all* conditions, devoted solely (or at least predominantly) to the learning of the relationships *within* linguistic fragments, then we may tentatively hypothesize that *the process of learning word sequences by forming them into linguistic fragments results in a certain fixed amount of retroactive interference with original learning, regardless of the number of such fragments which constitute the list.*

(2) Under the *intermediate* and *low* conditions of unity, retroactive interference increases as a function of repeated practices on the interpolated list, while under the *high* condition of unity interference remains invariant over successive practices on the interpolated list. Since the list of *high* unity consisted solely of linguistically regular relationships, the result for this condition follows directly from and adds empirical support to the constancy of the effect of learning linguistic relationships *per se*. The lists of *intermediate* and *low* unity consisted of a mixture of linguistic and nonlinguistic fragments. It was previously assumed that the linguistic relationships themselves were learned in the initial trial(s) of interpolated learning, while the remaining trials of interpolated learning were devoted to the formation and learning of *ad hoc,* idiosyncratic, and arbitrary relationships between the linguistic fragments themselves. In terms of memory-interference, this analysis suggests the following tentative generalization: *the process of learning word sequences by forming ad hoc, arbitrary, and idiosyncratic relationships between them results in a degree of memory interference which increases at a rate approximately equal to the rate at which these relationships themselves are formed.* The close parallel in the slopes of the curves for learning and the interference for the three levels of functional unity of the interpolated list add empirical support to this hypothesis.

These two hypotheses also provide us with a means for accounting for the consistent *lack* of difference in the rate of interference between the *intermediate* and *low* unity conditions, despite the consistent difference between these conditions in terms of degree of learning on last trial of interpolated learning. For *if* learning according to linguistic rules results in a fixed amount of interference, regardless of the number of units formed, *then* the interference-curves for all levels of functional unity should have approximately the same *Y*-intercept. If, on the other hand, the progress of interference is proportionate to the rate at which the *nonlinguistic* relationships are learned, and if the progress of nonlinguistic learning is independent of the number of linguistic units which must be so related, then the slopes of the interference-curves for these two conditions must be virtually identical. Since both the identity of the slopes and the identity of the intercepts must be the same, the identity of the two curves may thus be accounted for in their entirety.

Given these assumptions concerning the differential effects of the two learning processes, the question that remains to be answered is *why* the learning of linguistically regular units should take only a single trial and result in a fixed amount of interference, while the learning of arbitrary relationships between units should take a number of trials and result in progressively increasing interference? While an answer to this question is not, at present possible, certain directions of solution seem quite attractive.

With regard to learning of the serial list itself, it seems quite apparent that the learning of those sequences which do in fact conform to the semantic and syntactic rules of the language, is primarily a recognitive task—*i.e.* one of recognizing that a particular sequence is, in fact, an instance to which some subset of these rules applies. Since, in addition, these linguistic rules are so well known to our *S*s, we may assume that to a large extent the *act* of recognition and utilization is virtually instantaneous and automatic, requiring little active, intentional processing. With regard to memory-interference, it may be assumed that because the rules themselves are part of and derivable from a supra-ordinate functionally integrated system of such rules (namely, the language) these rules and the instances to which they apply have a high degree of stability in the cognitive structure of the *S*. In addition since application of these rules results in the formation of word sequences which either completely or partially fulfill the primary function of language; namely, that of conveying the meaning of the sequence, the meaning supra-ordinate functions of a particular word-sequence provides a cognitive cohesiveness and stability to that sequence which serves to reduce the probability that the elements of that sequence will be either subject to or the source of memory interference with respect to adjacently learned materials.

On the other hand, the learning of the sequence of sentence fragments by means of *ad hoc* and arbitrary rules may be assumed to have quite contrary characteristics. First, the rules themselves, thus formed, are not systematically derivable from any supra-ordinate system of rules already familiar to *S*. Likewise there do not exist any systematic supra-ordinate rules for the application of the particular ordering rule applied to any particular situation. Both of these factors imply a highly unstable situation in which the rules which apply to one sequence of items in a list may readily be confused with the rules which apply to any other sequence of items in the same list or in an adjacently learned list. On the other hand, since the sequence of items formed according to such a rule does not itself serve any supra-ordinate functions which will confer cohesiveness upon that particular structure, the items themselves may be assumed to be readily separable from each other, hence readily subject to and readily available as a source of memory interference with adjacent memory structures.

Some logical implications of this analysis. The assumption of two processes, which forms the foundations of this analysis, as well as some of the specific hypotheses relating to learning and memory-interference, generated particular experimental hypotheses directly amenable to independent experimental evaluation. Some of these currently under investigation are the following:

(1) The initial advantage of lists of *intermediate* over the *low* unity during the initial practice on the interpolated list, should result in (a) the *intermediate* list reaching a learning asymptote sooner than the *low* lists (despite the parallel slopes in the present series of studies); and (b) the *intermediate* list reaching a lower interference asymptote than the *low* list during the recall-test.

(2) The error-curves of serial position for the mixed linguistic and nonlinguistic lists should be scalloped in form such that (a) errors are maximal in the vicinity of those items which are at the boundary between linguistic fragments, and (b) errors should be minimal between items within each linguistic fragment.

(3) Adding to the number of words *within* any linguistic fragment should have relatively little effect upon either learning or the interference curves of serial lists, if the additional items are placed in such a position as to be linguistically consistent with the already existing items in that fragment.

(4) Adding to the number of linguistic fragments should have a significant effect upon both the Y-intercept of the learning curve for that list and upon the asymptote of its interference curve.

Other hypotheses, as well, might be generated from the current set of assumptions. Those described above, however, are currently under investigation, and will be the subject of subsequent reports.

SUMMARY AND CONCLUSIONS

A paradigm of serial anticipation of retroactive interference was used to investigate rate of learning and amount of retroactive interference as a function of level of *functional unity* of the interpolated list and amount of *practice* on the interpolated task, in a 3 × 3 factorial design. Stimulus-lists were composed of conventional English words. Unity of the interpolated list was manipulated by varying the degree to which the serial ordering of items within segments of the lists was consistent with the semantic and syntactic rules of the language. While the results of these studies raise series of questions as to the role of degree of interpolated learning, *per se,* in retroactive interference, a parsimonious explanation of these findings can be achieved by assuming that learning a serial list of conventional words involves two distinct types of processes: a *linguistic process* and a *nonlinguistic process.* The linguistic process consists of applying the known rules of the language to those sequences of words whose order is consistent with these rules. The nonlinguistic process consists of discovering or inventing rules of relating sequences of words whose order does not conform with the linguistic rules. The distinction between linguistic and nonlinguistic processes manifests itself during both the interpolated learning and the recall-test of retroactive interference.

This interpretation is based upon certain specific hypotheses concerning the operation of linguistic and nonlinguistic processes in learning and memory-interference. Each of these suggested hypotheses is amenable to an independent experimental evaluation in subsequent studies of this problem.

THE EFFECT OF IMAGINED ANCHORS ON ADAPTATION-LEVEL

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The image as a concept in psychology has undergone a long and varied history. After holding a certral position in early philosophical thought, it became of prime concern in psychology at the turn of the last century. Introspection was then its chief method of study. Because this method yielded very different results in different laboratories, it became suspect. During the behavioristic period which followed, the concept of the image was itself denied.

Since the period of rabid behaviorism, interest in the concept has been revived. The experimental methods and designs used by the investigators indicate, however, that the concept is still ambiguous.

Researches concerned with studying the effect of drive-states on perception have employed, for example, the concept to describe the experimental situation in which the subject (*S*) gives his impression of what an ambiguous stimulus-object "looks like."[1] *S* in this situation does not respond to an internal representation of a stimulus which is no longer present but rather to a definite external stimulus in which characteristics are related to a known stimulus. Lazarus, Yousem, and Arenberg elaborate on this distinction in their conception of two images: one is oriented toward imagination and the other toward the stimulus.[2] For the purposes of the present paper, this distinction is mainly of historical interest although it helps clarify the image being investigated here, which is the image oriented toward imagination.

Another approach to the study of the image has been through sensory conditioning.[3] Although the response indicating recognition of the image has varied from a motor response to a verbal report, all suffer from the same difficulty. It is never known if *S* actually imaged unconditioned stimulus (*US*) or only reported imaging it. Considering the usual unreliability of verbal report, the possibility of response bias is enough to question the conclusion of a conditioned image.

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* Received for publication October 19, 1964. The first study in the paper is based on a Master's Thesis submitted to the Department of Psychology at the University of Illinois, and was supervised by Professor Charles W. Ericksen.

[1] Richard Lazarus, Herbert Yousem, and David Arenberg, Hunger and perception, *J. Pers.*, 21, 1953, 312-328; Robert Levine, Isidor Chein, and Gardner Murphy, The relation of the intensity of a need to the amount of perceptual distortion: A preliminary report, *J. Psychol.*, 13, 1934, 283-293; D. C. McClelland, and J. W. Atkinson, The projective expression of needs: II. The effect of different intensities of the hunger drive on perception, *ibid.*, 25, 1946, 205-223; R. H. Stanford, The effect of abstinence from food upon imaginal processes, *ibid.*, 2, 1937, 129-136.

[2] Lazarus, Yousem, and Arenberg, *op. cit.*, 312-328.

[3] D. G. Ellson, Hallucinations produced by sensory conditioning, *J. exp. Psychol.*, 28, 1941, 1-20; C. R. Garvey, A study of conditioned respiratory changes, *ibid.*, 25, 1939, 586-600; E. L. Kelly, An experimental attempt to produce artificial chromaesthesia by the technique of the conditioned-response, *ibid.*, 17, 1934, 315-341; O. H. Mowrer, Preparatory set: A determinant in motivation and learning, *Psychol. Rev.*, 45, 1938, 62-91.

Still another approach has been through the use of conditioning under hypnosis.[4] In addition to the criticism of the studies on sensory conditioning, the method suffers from the possibility that the verbal report of imagery cannot be taken at face value because of the tendency for a hypnotized *S* to rationalize strange behavior after hypnosis.

A potential method of demonstrating images and of comparing their effects with those of real stimuli lies in the use of the adaptation-level. Images should have an effect upon the adaptation-level comparable to that obtained by presentations of the actual stimulus. That such a technique might work has been suggested by Volkman.[5] He found that *S*s instructed to "hold an anchor-stimulus in mind" when making judgments showed a shift in their scale of judgments similar to that made by *S*s whose judgments were made with an anchor being present.

A study of images by means of anchoring effects or changes in adaptation-level has advantages over earlier methods. The effects of the image would be more subtle and indirect and would not require *S* to give a direct verbal report concerning its presence or absence. Thus, *S* would be less susceptible to suggestion or to deliberate attempts to comply with the *E*'s requests. In addition, it would afford a quantitative assessment or comparison with the effects of a real stimulus.

In this situation, the inference of the image occurs when *S*'s earlier judgments are compared with judgments to the same stimulus-values after *S* was directed to image an anchor. *S* does not directly report the experiencing of an image as in previous studies but rather the image is inferred because of a change in the scale of absolute judgment. The shift therefore is considered a function of the image, just as a shift with a presented anchor is considered a function of this anchor.

The first experiment reported below will compare the effects of an imagined anchor and an actual anchor on a scale of absolute judgment. The second experiment will ask if the shift in the scale of absolute judgment, associated with either an imagined or perceived anchor, is due to a change in the use of response-categories or in the perceived size of the constant stimulus-objects.

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[4] Clarence Leuba, Images as conditioned sensations, *J. exp. Psychol.*, 26, 1940, 345-351; H. D. Scott, Hypnosis and the conditioned reflex, *J. gen. Psychol.*, 4, 1930, 113-130.

[5] John Volkman, The anchoring of absolute scales, *Psychol. Bull.*, 33, 1936, 742-743.

EXPERIMENT I

In Experiment I, we investigated the effects of an imagined and actual anchor on absolute judgments, and compared them with the results of a control group which did not use an anchor of any type.

Procedure. The experimental task was the absolute judgment of 12 circles varying in diameter in a geometric progression from 0.6 to 7.0 in. Three groups of *Ss* were employed: an anchor-; an image-; and a control-group. The *Ss* served individually. All were given an initial practice session during which 12 judgments were made of each of the 12 circles. The judgmental categories were: "very small"; "small"; "medium"; "large"; and "very large." The circles were presented in random order with the restriction that each stimulus-size occurred once in each of the 12 presentations. Within each set of 12 stimulus-objects there were three blank spaces at which time no stimulus or light was projected. These blanks were presented randomly during each set of 12 stimuli. During the test-trials the imaged anchor or the objective anchor, depending upon the experimental group employed, was presented.

Each stimulus-object was presented for 3 sec. followed by an interval of 1 sec. during which the stimulus was changed. *S*'s judgment was made during the 3-sec. period while the stimulus appeared. After the training trials a 3-min. break was taken when the anchor-stimulus was presented to all groups for 3 min.

Instructions. During this break the control *Ss* were given the following directions:

> We will take a 3-min. rest-period. After this interval we shall continue the same task. I will leave this practice circle on during the break. This circle will not be shown during the rest of the trials. It is just a practice circle. We will continue doing the same thing we have been doing. Keep looking straight ahead.

The directions given to the anchor-group were as follows:

> We will take a 3-min. rest-period, after which we shall continue doing the what we have been doing. The circle you now see presented on the screen will also be included with the other circles to be judged. I'm showing it now so you won't be surprised when you see it among the other circles. Just judge this one as you did the others. Any questions? Keep looking straight ahead.

The image-group were instructed as follows:

> This is a 3-min. rest-period. After this interval we shall continue the same task except every few trials instead of showing you a circle I wish you to imagine the one that is now on the wall. This is called the practice circle. Notice it is a little larger than a long-play phonograph record. Do you know what I mean by a long-play phonograph record? Now I want you to picture this practice circle in your mind. In other words, imagine it. Imagine this circle now. Can you do this? [If the answer is no, *E* asks him to do the best he can.] Try and get a mental picture of it. Remember that sometimes no circle appeared on the wall? Now, every few trials when no circle appears on the wall try and picture this circle in your mind. Do this only when no circle appears. Otherwise, continue answering in the same manner as we have been doing. That is, give a judgment out loud to all the circles that appear on the wall. Also give a judgment to the one you imagine. Remember to try and imagine the circle as best you can. You want the circle you imagine to be like this circle now on the wall—the one I said was a little larger than a long-play phonograph record. Are there any questions? Remember, when a blank appears, you

imagine the practice circle here and you give an answer as you did to the other presented circles. Are you ready?

During the time when the two blanks appeared at the beginning of the test-series the *S*s in this group were told, "Now imagine the practice circle."

At the termination of the 3-min. break, the test-trials began. The procedure during the test-trials was the same as during the training-trials except that the anchor-group had the anchoring stimulus inserted in the previously blank slots and the image-group carried out the instructions to image the anchor-stimulus during these blank trials.

To provide some converging operations on the concept of the image in this experiment, two measures were employed. The first was obtained by using a group of alcoholics who had experienced visual hallucinations while undergoing delirium tremens as contrasted with another group of alcoholics who had undergone *DT*s but had no visual hallucinations. A second measure was the score on the visual tasks from the Wechsler-Bellevue memory-scale. This scale was administered to all the *S*s upon completion of the test-series of judgments, using the standard procedure of administration.

Apparatus. The stimuli were presented by means of the Bell and Howell robomatic projector and timer. The stimuli were constructed of 1/16 of an inch thick sheets of aluminum made into 2 × 2 slides with a hole drilled in the center to yield the different projected stimulus-sizes.

Subjects. The *S*s were 36 men, patients at the Kankakee State Mental Hospital, who carried the diagnosis of chronic alcoholism or sociopathic personality associated with alcoholism. All were within the age-range of 25-50 yr. Furthermore, all of them had a history of having experienced delirium tremens. They were further classified upon the basis of their visual hallucinations during those attacks. From the 36 *S*s meeting these criteria, two subgroups of 6 *S*s each—6 with visual hallucinations and 6 without—were selected for each of the 3 experimental groups: (1) anchor presented; (2) anchor-imaged; and (3) no anchor. Thus, we had a total of 6 groups of 6 *S*s each.

Results. Before comparing the results of the three experimental groups and the subgroups of visual and nonvisual hallucinators on the test-judgments an examination was made of the practice judgments to insure comparability across groups. By converting the judgmental scale into the numbers one through five and averaging each *S*'s judgments to each stimulus-object, a modified four-way analysis of variance was performed on the data (treatment groups, hallucinators-nonhallucinators, stimulus-objects, and individuals). No appreciable differences were found on any of the comparisons for the practice judgments.

The same analysis was then applied to the test-judgments. For them the main effect due to the different treatments was significant well beyond the 0.01 level ($F = 13.30$, *df* 2/30), as was also the main effect for stimuli ($F = 605.67$, *df* 11/330) and the treatment × stimuli interaction ($F = 3.37$, *df* 22/330). None of the other effects or interactions approached significance.

A comparison of the means of the different treatments indicated that, as expected, both presented and imagined-anchor groups differed significantly at the 0.05 level from the control group ($t = 2.56$, $t = 2.37$, respectively), but not from each other. The 'treatment'-groups compared here are the combination of the visual and nonvisual hallucinatory subgroups for each experimental condition. It seemed justifiable to combine them since the hallucinating-nonhallucinating factor and its interactions did not approach significance.

The nature of the significant effects is apparent in Fig. 1 where we have plotted the average judgment by stimulus for each of the three experimental groups. In this figure, the visual and nonvisual hallucinators have also been combined.

The curves for the anchor and the image groups are indistinguishable from each other although clearly different from the control group. The nature of the significant interaction that was found between treatments and stimuli is clearly revealed. The effect of an anchor, whether real or imagined, is predominantly based upon the judgments of the larger stimuli in the series. There is little or no detectable effect of the anchor upon the judgments of the three smaller stimuli in the series.

While the above results show that the effect of an imagined anchor is as great as one that is actually present, there was no indication in our data of a relationship between our two measures of *S*s' imagery ability and the amount of change in the judgmental scale due to the presentation of the imaged anchor. There was no evidence that either a history of visual hallucinations or scores on the Wechsler memory scale will predict or show relation to the effectiveness of an imagined anchor upon judgmental responses. This was revealed in the above analysis by our failure to obtain a main effect of visual-hallucinators vs. nonhallucinators or any significant interaction involving that factor. Also the scores on the visual portion of the Wechsler memory scale failed to correlate with a judgmental shift.

Discussion. The results of this experiment are quite clear in demonstrating that instructing *S* to image a stimulus is quite similar to presenting the actual stimulus. How we interpret this result, however, depends upon the interpretation given to adaptation-level effects in general. If, as it is sometimes assumed, adaptation-effects reflect a change in *S*'s perception of stimuli, then the present experimental result would be rather convincing evidence of the effect of images and would suggest this method as a fruitful one for their investigation.

There is, however, some questions as to whether adaptation-level really reflect changes in perception. Instead they may be a function of the way in which *S* uses different categories of response. In keeping with the distinction drawn by

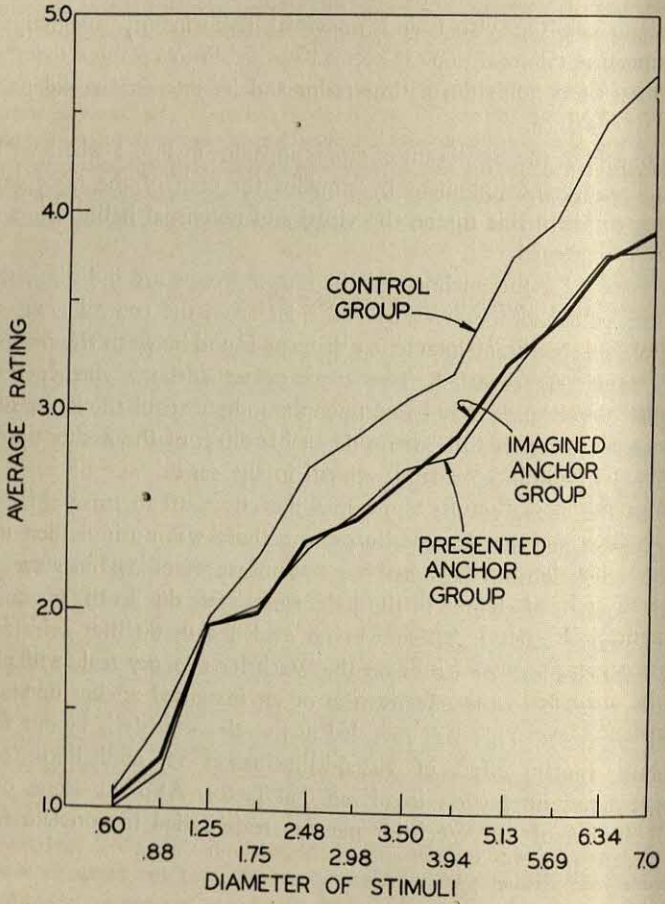

FIG. 1. AVERAGE RATING FOR EVERY STIMULUS-SIZE FOR EACH EXPERIMENTAL CONDITION

Garner, Hake, and Eriksen between perception as an intervening concept and the responses from which it is inferred,[6] we can ask in the case of the present experiment whether the circles actually were perceived as smaller by *Ss* in the anchor and the imaged anchor group or whether *Ss* knowing that a larger circle had to be included in the judgments made room for it by dropping stimuli from the other categories down into the next smaller category. If *Ss* in the present experiment were merely adjusting their response categories to take into account a larger stimulus, then the present results have nothing to tell us about the nature of images. For both the image and the anchor groups, knowledge that a larger stimulus had to be included in the judged series could result in essentially the same judgmental shifts for the regular stimuli in the series.

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[6] W. R. Garner, H. W. Hake, and C. W. Eriksen, Operationalism and the concept of perceptions, *Psychol. Rev.*, 63, 1956, 140-159.

Helson has shown awareness of this problem as to whether it is perception that has changed or merely the way *S* uses response categories.[7] He has not, however, suggested a way of answering the problem other than to suggest that these alternative interpretations can be ruled out by appropriate converging operations. A set of converging operations which would allow us to rule out a response interpretation would have to separate the changes in the response-system from the changes in the *Ss* perceptions.

EXPERIMENT II

We have attempted to separate the perceptual system from the response-system by an experiment in which one group of *Ss* is presented the anchoring stimulus in their test-judgments and required to judge it whereas a second group is presented the anchoring stimulus but is not instructed to judge it. A third control group would not be presented with an anchor and, therefore, would not be required to make allowance for it in their judgments. This experiment will allow us to determine whether or not the presentation of a nonjudged stimulus-affects the perceived size and judgment of the other stimuli in the series. If, in this experiment, it is found that the nonjudging group do not differ from the control group it would indicate that the results in our first experiment cannot be attributed to the presence of an image exerting an effect upon the adaptation-level.

Procedure. Three experimental groups were employed; (1) a judged anchor-group, (2) a presented but nonjudged anchor-group, and (3) a control group. All groups were given the training used in Experiment I. During the test-trials, the judged anchor-group was presented the anchor-stimulus during the test-judgments at the same intervals and with the same frequency as the first group but was instructed not to make a judgment. The control group did not receive the anchoring stimulus during the test-judgments.

Instructions. The directions were the same as those of the first experiment for the control and judged anchor-groups, however, the nonjudging group was given the following different directions:

> We will take a 3-min. rest-period. After this interval we shall continue the same task. The circle you now see presented on the screen will also, however, be included

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[7] Harry Helson, Adaptation-level theory, in Sigmund Koch (ed.), *Psychology: A Study of a Science,* Vol. I, 1959, 565-621.

with the other circles. This circle, however, is not to be judged. When you see the circle appear, look at it but do not judge its size as you did with the other circles. So that I know you see it, say something like 'practice' or 'see it.' In summary, judge all the circles you have seen except this one which you'll just notify me that you see it. Any questions?

Subjects. The *S*s were 21 men and women undergraduate students enrolled in the elementary psychology. They were randomly assigned to each of three groups counterbalanced for sex.

Results. The five judgmental categories were converted into a numerical scale by giving them values of one through five, respectively. For each *S* the 12 judgmental scores to each stimulus obtained in the first series of judgments were summed, as were also the 12 scores to each stimulus in the test-series of judgments.

A difference-score was obtained between the sum of training trials minus the sum of the test-trials. This difference-score was then summed over three blocks of four stimuli each in the following stimulus-order: Stimuli 1-4, 5-8, and 9-12. Thus, each *S* had three final difference-scores which were the sum of the differences between training and test-trials for three blocks of four stimuli each.

The data were analyzed in terms of difference-scores rather than analyzing the test-trials separately, as was done in the first experiment, to achieve a more precise measurement of the shift in the scale.

The first analysis was done using all the stimulus-blocks and treatments. From previous studies, it was expected that there would occur an interaction between stimuli and treatments with only the stimuli closest to the anchor being shifted. This experiment would predict that if the shift in the scale is a matter of rearranging response categories, only the judged anchor-group would be affected by the anchor. Therefore, the only cell in this nine-cell table which should be significantly different is that one which contains the largest four stimuli of the judged-anchor group.

An over-all two-way analysis of variance (stimuli × treatments) was performed. As predicted, treatments and the interaction between stimuli and treatments were significant beyond the 0.01 level ($F = 8.22$, $df = 2/19$; $F = 20.77$, $df = 4/38$, respectively). Due to the fact that the variances of the treatment-groups indicated possible heterogeneity and also since the *N*s were small, it seemed advisable to test for homogeneity of variance before progressing to the analysis of variance of each block of stimuli.[8] The Hartley test for homogeneity was used. For the largest four stimuli (9-12), which had the greatest between-groups difference in variance, there was obtained an F of 4.73 which led to an acceptance of the null hypothesis at the 0.5 level of significance.

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[8] E. F. Lindquist, *Design and Analysis of Experiments in Psychology and Education*, 1953, 76-81.

A simple two-way analysis of variance (experimental conditions × Ss) was then done on each block of difference-scores. For the largest four stimuli (9-12), the experimental effect was significant ($F = 15.88$, $df = 2/18$, $p < 0.01$) but for the other two groups of stimuli (1-4, 5-8), there were no significant differences between experimental conditions.

To test the difference between means, the mean square for within-treatments was not used because of the large differences in variance. The usual t-test for equal Ns was employed for samples with possible unequal variances.[9]

The treatment effect was significant ($t = 2.45$, $p < 0.05$) in the predicted direction as the nonjudged-anchor group did not differ from the control group but both control and nonjudged-anchor groups did differ from the judged-anchor group ($t = 3.62$, $t = 2.64$ respectively).

In considering the relation between stimulus-size and the judgment of stimulus-size, the judgments of the control and the nonjudged-anchor-groups increased in a linear fashion as the stimulus-size increased. For the judged-anchor condition this occurs until approximately the ninth stimulus, when the rate of increase is reduced for the remaining stimuli. The judged anchor therefore has its strongest effect on the judgments of stimuli closest to the anchor in size and less effect as the stimuli decrease in size. Only the largest four stimuli had to be significantly shifted from their previous judgment categories in order to fit the anchor into the scale. The smaller stimuli are not displaced by the down-shifted stimuli because the displaced stimuli, which were previously placed in the category now occupied by the anchor, seem to be absorbed by the neighboring categories. This effect has constantly accompanied research using this method and illustrates why the middle and small blocks did not exhibit significant differences.[10] The results of the first experiment also showed that the larger stimuli were affected by the anchor much more than the smaller stimuli. The dividing of the stimuli into blocks helped pinpoint the anchor effect in regard to where it has its effect on a judgment scale.

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[9] A. L. Edwards, *Experimental Design in Psychological Research,* 1960, 106-108.

[10] Allen Paducci, Direction of shift in judgments of single stimuli, *J. exp. Psychol.,* 51, 1956, 169-178; Leo Postman and G. A. Miller, Anchoring of temporal judgments, this JOURNAL, 58, 1945, 43-53.

Discussion and conclusions. The results confirmed our expectation that the effect of an anchor on an absolute judgment scale is dependent on whether or not it is judged, and the effect of an anchor occurs most markedly with those stimuli which are closest to the anchor in size. These results seem best explained in terms of *S* shifting his response categories in order to insert the anchor, and indicate that it is the response system which is affected by an anchor rather than an inner state of perception or imagery.

Studying anchoring effects on a subjective scale and then inferring a perceptual process from the results has to be done with an awareness of the possible bias which could arise in the response system. After seeing or, in fact, "imaging" an anchor, the *S* has to make one of a limited number of responses. Before the response is considered a reflection of an inner state, other converging operations have to be performed.

The second experiment is just one test of an alternate hypothesis concerning the inference of a "inner state" in this experimental design. As can be seen from the results, the alternate hypothesis tends to explain results in terms of the response system. Therefore, predictions from *A-L* theory which have been successfully applied to this method must be predicting a manipulation of responses.

SUMMARY

The first experiment reported in this paper was an attempt to study imagery by the design previously employed in adaptation level studies in which the effect of an anchor stimulus was observed on an absolute judgment scale. The main result indicated that an imagined anchor has the same effect on a subjective scale of judgment as a presented anchor.

The second experiment was done to discover if the shift was due to a change in *Ss* perception or to a change in *Ss* use of the response categories. The same design was used except, instead of using an imaged anchor, one group was presented with the anchor but did not judge it, while the other experimental group did the usual judging of the anchor. Results showed that only the judged group exhibited an anchor-effect, therefore, differing from the control group and the nonjudging group which did not differ and did not show the anchor effect. These results were interpreted in terms of an alternate hypothesis which considered the anchor having an effect on the response system rather than on perception. The point was made that predictions derived from theory and based on this procedure should be limited to the response categories and not to any "inner state."

SEPARATION OF POSITIVE AND NEGATIVE REINFORCING SYSTEMS IN THE DIENCEPHALON OF THE RAT

By D. L. MARGULES, Wyeth Laboratories

There are practical and theoretical reasons for allowing rats to terminate as well as to initiate the same brain-stimulus.[1] From the practical point of view, the technique makes it possible to assess both the positive and negative reinforcing effects of an electrode, and it provides two measures of the strength of each effect—rate of responding and duration (latency). Furthermore, the procedure allows the opportunity to see if rats will maximize the duration of positive stimulation. Preliminary work suggests that in some cases they will,[2] but further testing, especially at high levels of current, is necessary.

From the theoretical point of view, the observation that a rat will turn off the same stimulus that it turns on raises questions of interpretation. First, does the rat turn off the stimulus because it is aversive, or for some other reason (*e.g.* adaptation).[3] Secondly, does the sequential change in reinforcement-value, whether due to punishment or otherwise, occur within a single, initially rewarding, system (one-system theory) or does it result from the coincidental stimulation of a second system (two-system theory)? Roberts has favored the punishment-interpretation, considering both sequential change in a single system and overlapping stimulation of two systems as possible explanations.[4] Bower and Miller also have favored the punishment-explanation, but have not discussed whether one or two systems are involved.[5] Olds, Travis, and Schwing have taken the position that negative reinforcement—deriving either from boundary-placements between the two systems or from

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[*] Received for prior publication February 15, 1966.

[1] N. E. Miller, Objective techniques for studying motivational effects of drugs on animals, in Silvio Garattini and Vittorio Ghetti (eds.), *Psychotrophic Drugs,* 1957, 83-103.

[2] D. A. Brodie, J. L. Malis, O. M. Moreno, and J. J. Boren, Nonreversibility of the appetitive characteristics of intracranial stimulation, *Amer. J. Physiol.,* 199, 1960, 707-709; William Hodos, The nonaversive properties of long durations of rewarding brain stimulation, *Amer. Psychologist,* 18, 1963, 437.

[3] Larry Stein, An analysis of stimulus-duration preference in self-stimulation of the brain, *J. comp. physiol. Psychol.,* 55, 1962, 405-414.

[4] W. W. Roberts, Both rewarding and punishing effects from stimulation of posterior hypothalamus of cat with same electrode at same intensity, *ibid.,* 51, 1958, 400-407.

[5] G. H. Bower and N. E. Miller, Rewarding and punishing effects from stimulating the same place in the rat's brain, *ibid.,* 51, 1958, 669-674.

[6] James Olds and M. E. Olds, The mechanisms of voluntary behavior, In R. G. Heath (ed.), *The Role of Pleasure in Behavior,* 1964, 23-53.

nuclei which synapse with the two systems,[6]—motivates turning-off behavior.[7] Stein has presented several lines of evidence supporting the coincidental and progressive activation of a second punishment-system in the diencephalon and, in addition, has introduced the idea that adaptation may play a role, especially in the forebrain.[8] Valenstein and Valenstein have reported that every self-stimulating rat they tested would learn to turn off the brain-stimulus in a duration test.[9] This evidence seems to provide strong support for the one-system punishment-theory, which predicts that all positive electrodes will eventually also produce negative reinforcing effects, but the same authors go on to raise a number of critical questions about the punishment-assumption.

The objective of the present study was to determine duration-preferences at a variety of diencephalic sites. The experiment is similar to that of Valenstein and Valenstein except that the electrodes were not preselected on the basis of a self-stimulation test.

METHOD

Subjects, electrodes, and stimulus-parameters. A stainless-steel, bipolar electrode was stereotaxically implanted in each of 40 male albino rats of the Holtzman strain. The electrodes were 0.01 in. in diameter and Formvar-insulated except at the cross-section of the tips. The target-areas for implantation were the known positive, negative, and ambivalent areas of the diencephalon.[10]

The electrical brain-stimulus consisted of alternating square-wave pulse-pairs of 0.1-m.sec. duration, separated by 0.07 m.sec. and presented at 100 cycles/sec. Current was monitored on a cathode-ray oscilloscope and ranged from 50-1200 μa.

Histology. At the conclusion of the experiments, the rats were sacrificed and their brains sectioned at 35 μ. to determine electrode-tip locations. Photographs of the wet slices permitted accurate location of fiber-tracts in the brain. The area of stimulation was assumed to be immediately below the point of deepest penetration of the electrode (Fig. 1).

Apparatus and procedure. The apparatus was a 10 × 20-in. shuttlebox 12 in. high, with a lever at each end. The levers were 1 in. wide, mounted 1 in. off the floor, and projected 1 in. into the box. Pressing one lever turned the electrical brain-stimulus on, and pressing the other turned it off. If the rat did not press the 'on' lever, current was turned on automatically at 150 sec. If the rat did not press to turn the stimulus off, it was turned off automatically at 60 sec. Event-pens and cumulative timers indicated on and off times. Counters indicated the number of initiations, terminations, and automatic on- and off-sets of stimulation.

Training. The rats were placed in the boxes for one 60-min. session each day. No shaping of the lever-pressing response was used in any phase of the experiment. Each session except the first one was conducted at one fixed level of current. On this day, the current was raised from 0 to a level which produced, first, any behavioral change and, later, lever-pressing. If lever-pressing did not develop, current was raised gradually in 100-μa. steps with at least three days at each new current-level. Of the 40 *S*s, 14 learned to press one or more levers; the remaining 26 *S*s tested did not turn the current on or off at any current-level up to 1200 μa. Some of the 26 showed no behavioral effects, while others took seizures and were incapacitated by the stimulation. No further work was done with these *S*s, but typical behavior patterns for six of them are reported.[11]

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[7] James Olds, R. P. Travis, and R. C. Schwing, Topographic organization of hypothalamic self-stimulation function, *J. comp. physiol. Psychol.*, 53, 1960, 23-32.

[8] Stein, *op. cit.*, 405-414.

[9] E. S. Valenstein and Thelma Valenstein, Interaction of positive and negative reinforcing neural systems, *Science*, 145, 1964, 1456-1458.

[10] M. E. Olds and James Olds, Approach-avoidance analysis of rat diencephalon, *J. Comp. Neurol.*, 120, 1963, 259-295.

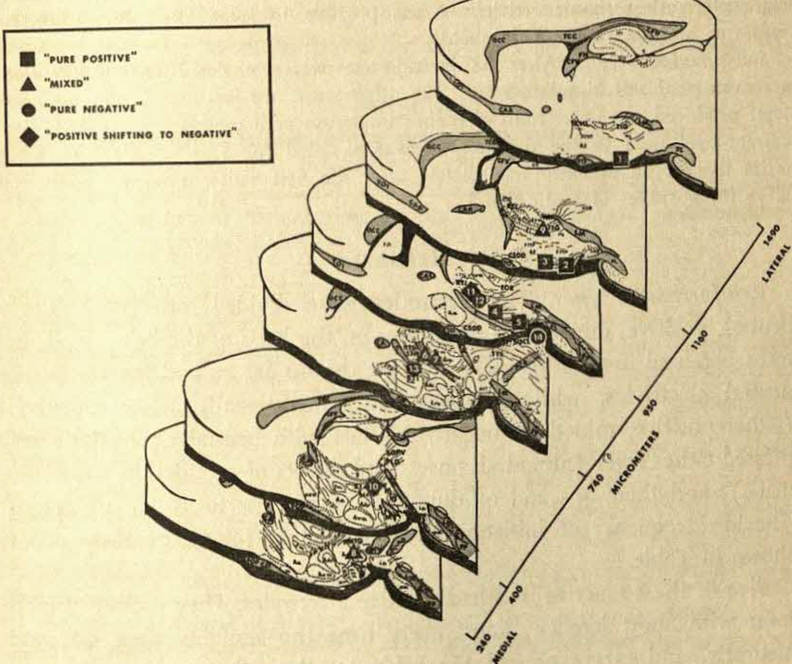

FIG. 1. SAGITTAL SECTIONS THROUGH THE RAT-BRAIN SHOWING DEEPEST PENETRATION OF ELECTRODES OF THE VARIOUS TYPES

(The squares penetrate deeper into the hypothalamus than do other symbols. *CA*, anterior commissure; *CFV*, fornix commissure; *CO*, optic chiasma; *F*, fornix; *FMP*, medial forebrain bundle; *FMT*, mamillothalamic tract; *FMTT*, mamillotegmental tract; *FOR*, reticular formation; *FPVH*, periventricular tract; *GCC*, genu corpus callosum; *HI*, hippocampus; H_1, H_2, Forel's field; *LM*, medical lemniscus; *RTI*, inferior thalamic radiations; *RTM*, intermediate thalamic radiations; *SGCL*, lateral central gray; *SGPV*, periventricular central gray; *SM*, stria medullaris; *SUM*, supramamillary decussation; *TCC*, corpus callosum trunk; *TIG*, tractus incertotegmentalis; *TO*, optic tract; *TOI*, olfactory tract; *TTS*, tectospinal tract; *ZI*, zona incerta.)

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[11] Some reasons for the high percentage of discards are: (1) because the electrodes were aimed in many cases for the bottom edge of the hypothalamus, some inevitably

Current-tests. After performances were stabilized, the *Ss* were subjected to a series of current tests; first a descending current-series with daily reductions in current; then a return to stable current-levels; and, finally, an ascending daily series. The descending series lasted approximately two weeks, and the current was reduced as low as zero. The ascending series lasted approximately three weeks, and the current was raised as high as possible without producing seizures, incapacitation, or extremely violent motor activity. It was possible in some cases to use current values as high as 12 times threshold.

Self-stimulation tests. After the duration-tests were completed, each *S* was given a conventional self-stimulation test. The offset-lever was inactivated, and the onset lever produced a 0.3-sec. train of brain-stimulation each time it was pressed. Each current-level used in the duration-test was sampled for 8 min. in an ascending series that began at zero. Immediately after the first series, a second series was given for purposes of replication.[12]

RESULTS

Reinforcement-types. The electrodes were divided into four types—neutral, positive, mixed, and negative—on the basis of the behavioral patterns obtained in training (Fig. 2). Of the 40 *Ss*, 26 had motivationally neutral electrodes, while fourteen had motivationally active electrodes (either positive, mixed, or negative). Rats with neutral electrodes rarely pressed either lever (almost all onsets and offsets of stimulation were automatic), and their on- and off-durations were close to 100% of cut-off. The low frequency of initiation and termination for six of these rats is shown in Table I.

Five of the 14 'active' rats had positive electrodes. They pressed the on-lever with short latency almost every time the stimulus went off automatically, and rarely pressed the off-lever; thus, the on-durations were maximized and the off-durations minimized (Fig. 2). The table shows the high frequency of initiation and low frequency of termination. These rats exhibited 'initiation.'

Four rats had mixed electrodes. They alternated between pressing the

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were misplaced and fell too deep and out of the brain, while others fell in ventricles, in seizure-prone tissue, and even in distant brain areas; (2) the use of very high currents and extremely long durations increased the probability that the stimulus would be incapacitating.

[12] This single day of testing for self-stimulation was considered adequate, since all rats had previous experience in pressing levers to regulate duration and had sufficient previous stimulation in the many daily duration-tests to preclude shifting of thresholds. The *Ss* did not, however, have previous experience with the rapid bursts of responding used to generate high rates in self-stimulation, and for that reason a low cut-off point of 90 responses in 8 min. was used as the threshold-value. This cut-off point occurred at the point of sharp change in slope of responding and was therefore considered to be satisfactory.

on- and off-levers, with low latency on each; thus, both the on- and off-durations were minimized (Fig. 2). The high frequency of lever-pressing is shown in the table. These rats rarely had either automatic on- or off-sets of stimulation. They exhibited what will be called 'alternation.'

Five rats had negative electrodes. They pressed the off-lever with low latency almost every time the stimulus came on automatically; thus, the off-durations were maximized and the on-durations minimized (Fig. 2). These *S*s rarely initiated the brain-stimulus, as is shown in the table. Their principal behavior will be called 'termination.'

TABLE I

FREQUENCY OF INITIATION AND TERMINATION OF BRAIN-STIMULI

Positive			Mixed		
Rat	Initiation†	Termination	Rat*	Initiation	Termination
1	56	0	6	619	618
2	55	0	7	330	330
3	47	0	8	199	198
4	55	2	9	411	412
5	54	1			

Negative			Neutral		
Rat	Initiation	Termination‡	Rat	Initiation	Termination
10	1	21	15	5	0
11	3	20	16	3	3
12	0	21	17	5	1
13	1	19	18	1	0
14	1	22	19	3	2
			20	3	2

* The current for these rats was 1,000 μa. The current for all other rats was at levels of stable effects.

† The maximal number of possible initiations with no terminations is 59.

‡ The maximal number of possible terminations with no initiations is 23.

Current-functions. In the current-tests, the five rats with positive electrodes maintained initiation at all current-levels between the threshold and maximal values. The term 'pure' will be used when the electrode produces the same pattern of behavior between these values. Fig. 3 shows the mean and range of on- and off-durations for these five rats at the various current-levels. The four rats with mixed electrodes maintained alternation over medium and high current-values, but demonstrated a shift to initiation-behavior at lower currents. Fig. 4 shows the mean and range of on- and off-durations for these rats and the current-values over which both types of behavior occurred. Three of the five rats with negative electrodes showed pure 'termination' (Fig. 5), but the other two shifted

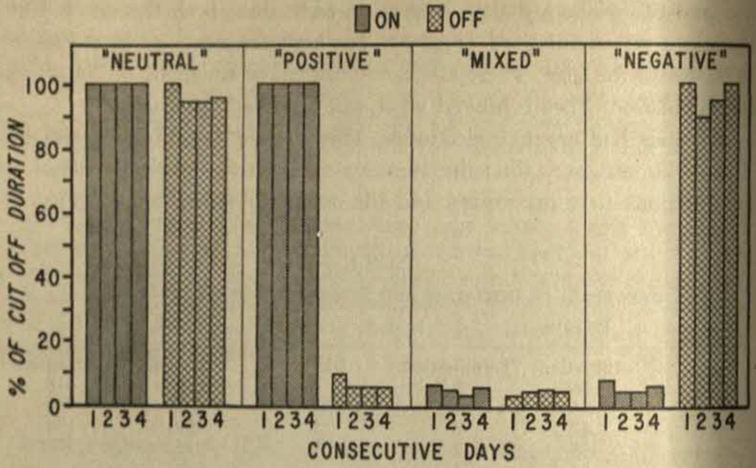

FIG. 2. MEAN DAILY ON- AND OFF-DURATIONS OF BRAIN-STIMULATION FOR FOUR RATS REPRESENTING DIFFERENT REINFORCEMENT TYPES

Rat 18, neutral electrodes; rat 3, positive; rat 6, mixed; rat 12, negative. Four consecutive days of testing are shown. Means are expressed as percentages of the maximal durations available, *i.e.* 60 sec. for on-durations and 150 sec. for off-durations.

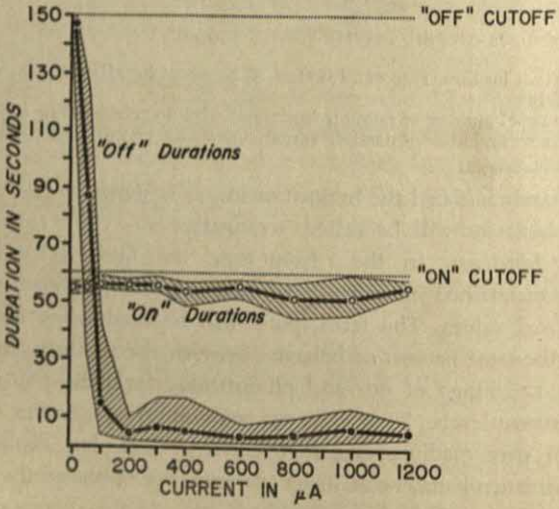

FIG. 3. MEAN ON- AND OFF-DURATIONS FOR FIVE PURE POSITIVE ELECTRODES

N = 3 at 1200 μa.

Cross-hatched areas indicate the range of means.

to 'initiation' at lower currents (Fig. 6). One of the latter also demonstrated a second shift to alternation at highest currents. This particular rat showed all three behavior-patterns: 'initiation' at low current, 'termination' at medium current, and 'alternation' at high current.

Self-stimulation thresholds. Fig. 7 shows the thresholds for self-stimulation compared to those obtained for initiation. (The term 'threshold' is used to indicate the lowest current tested which was above an arbitrarily set cut-off point in an ascending current-series; for self-stimulation, the cut-off point was 90 responses in 8 min.; for initiation, it was a mean off-time of 17 sec.). All rats with purely positive electrodes self-stimulated, and their self-stimulation thresholds ranged 200-1000 μa. higher than their initiation thresholds. Three of the four rats with mixed electrodes self-stimulated, and their self-stimulation thresholds ranged 50-200 μa. higher

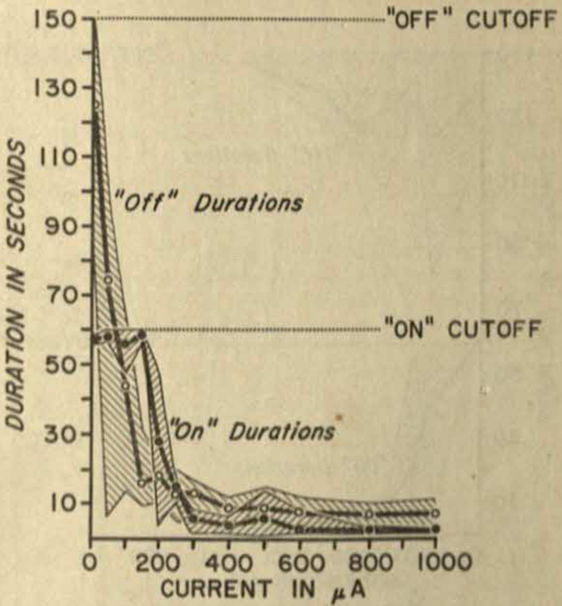

FIG. 4. MEAN ON- AND OFF-DURATIONS FOR FOUR MIXED ELECTRODES

N = 2 at 150 and 250 μa.

Cross-hatched areas indicate the range of means.

than their initiation-thresholds. The fourth rat with a mixed electrode died before it could be tested on self-stimulation. None of the three rats with purely negative electrodes self-stimulated, but the rat with the positive-shifting-to-negative electrode (no. 11) did. The other rat with this type of electrode died before it could be tested on self-stimulation.

Histology. All five purely positive electrodes were located partly within the ventral portion of the medial forebrain bundle and at less than a millimeter from the base of the brain (see the squares in Fig. 1). All mixed, negative, and shifting electrodes were located dorsally to the points found to produce purely positive effects. In addition to being distinctly deeper than all other points, purely positive points also tended to be more lateral than most other points. Mixed electrodes were found on the boundary between the thalamus and the hypothalamus; three were placed within the area of the mamillothalamic tract, and the fourth was an extremely medial placement near the posterior hypothalamic nucleus. One of the three purely negative electrodes was located mainly within the

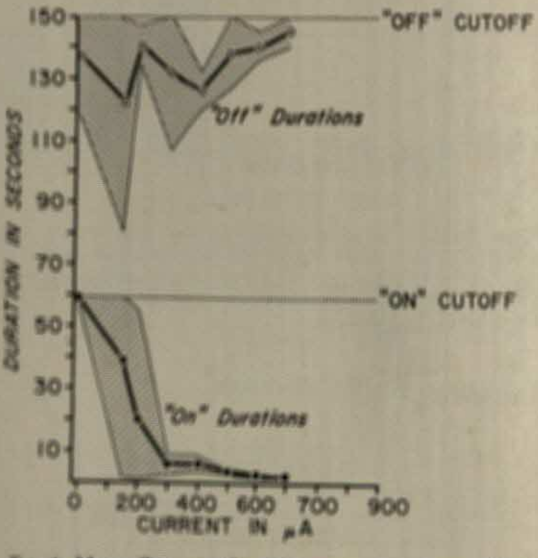

FIG. 5. MEAN ON- AND OFF-DURATIONS FOR THREE PURE NEGATIVE ELECTRODES
N = 2 at 400-650 μa.
Cross-hatched areas indicate the range of means.

thalamus; the second, mainly within the central gray; and the third, on the border of the thalamus and the hypothalamus. The two other shifting-type electrodes also were located on the thalamic-hypothalamic border, and they could not be distinguished anatomically from the mixed-electrode placements.

Discussion

Experiments on electrical stimulation of the hypothalamus have suggested that the brain-systems responsible for behavioral effects overlap. In the present work, however, five electrodes were identified which produced predominantly positive effects at all suprathreshold current-levels, even when those levels were maintained for relatively long periods. At such placements of the electrodes, the contribution of negative reinforcing systems must be very small or negligible. This finding provides strong support for the two-system theory. It shows that positive reinforcing effects can be produced separately from any negative reinforcing effects, even at high current-levels and with duration of stimulation up to 60 sec.

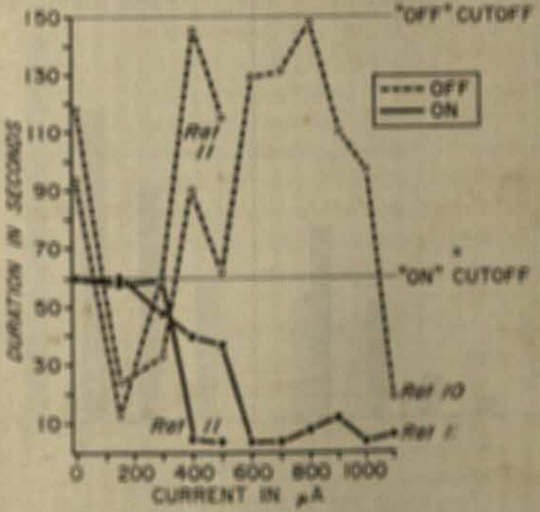

Fig. 6. Mean On- and Off-Durations for Two Electrodes which Shifted from Positive to Negative

Valenstein and Valenstein did not find any purely positive electrodes in their two-lever duration study.[13] The fact that these electrodes tend to produce self-stimulation only at high current-levels may account for their failure. If Valenstein and Valenstein preselected low-threshold self-stimulation rats, they may have excluded the more difficult to train, high-threshold, pure positives from their sample.

It may be speculated that thresholds for self-stimulation were higher with purely positive electrodes because the long durations of stimulation served to damage tissue around the electrode. Progressive tissue-destruction seems unlikely as an explanation, since initiation-latencies did not increase over testing sessions, but rather decreased. A more likely explanation for the poor self-stimulation performance of pure positives is that the 0.3-sec. fixed trains were too brief to maintain lever-pressing. Purely positive electrodes probably are located so deeply that they not only exclude most negative reinforcing systems, but also substantial parts of the positive system as well. An increase in the duration of training would

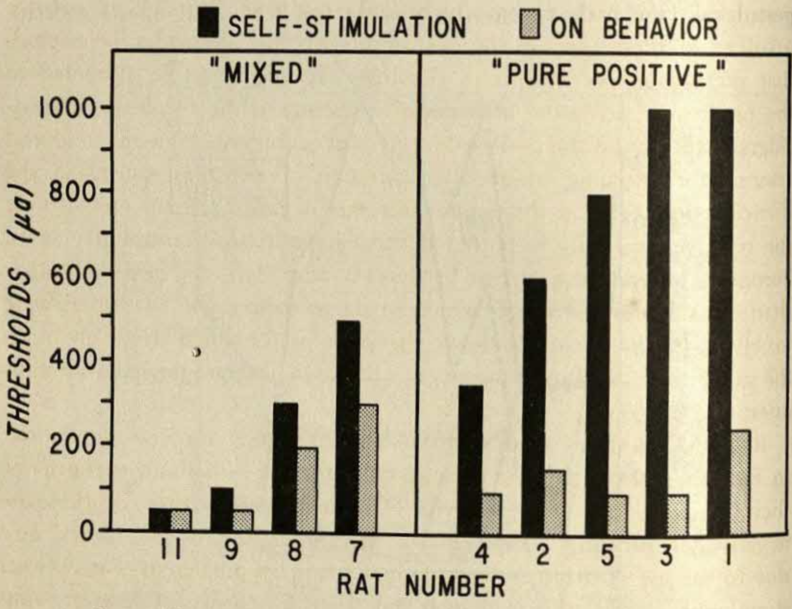

FIG. 7. THRESHOLDS FOR SELF-STIMULATION AND INITIATION

Each pair of bars gives the threshold in microamps for one rat on both tests. Criteria for the cut-off points of the thresholds are given in the text.

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[13] Valenstein and Valenstein, *op. cit.*, 1456-1458.

be expected to enhance self-stimulation, possibly by temporal summation. In an unpublished study, Stein found that an increase from 0.5-0.75 sec. was sufficient to raise the number of self-stimulation responses on a fixed-interval program significantly above chance-levels.

Negative reinforcement appears to be involved with mixed electrodes, and the amount is critical—*i.e.* just enough to produce termination without much reducing initiation. If it were less, the rat would remain an initiation-animal; if more, it would become a termination-animal. Stein has postulated two mechanisms which could result in the delicate balance involved in alternation. In the case of diencephalic electrodes, he assumes that preferences for short-train durations are largely to be attributed to the progressive activation of a negative system; in the forebrain, he considers adaptation of the positive system most important.[14] There are several reasons for assuming progressive activation of negative systems in the diencephalon. First, in this region there are negative systems present near the positive, especially in border thalamic-hypothalamic-central-gray areas. Secondly, lowering the current produces in alteration-rats, initiation rather than the subthreshold effects which would be expected if adaptation were involved. Finally, Stein has shown that barbiturates which selectively block the suppressive effects of punishment[15] markedly increase the brain-stimulus duration.[16]

Rats that show alternation have low thresholds for self-stimulation; in fact, these thresholds are close to those for the initiation-component of their duration-regulating behavior. One explanation may be that the negatively-reinforcing component of the stimulation is coincidental and due to the use of a large electrode in a small hypothalamus. For optimal stimulation of the positive system and the maintenance of lever-pressing for short durations, it may be necessary to place the electrode so high in the positive system that it stimulates a part of the negative system as well, particularly in long durations. It is also possible, however, that the negative system may contribute in some way to the positive-reinforcement effects (*e.g.* by rebound).

The three rats that showed termination suggest that purely negative electrodes produce reinforcing effects that are predominantly negative at all testable suprathreshold current-levels. These rats did not self-stimulate. Their electrodes were located in the thalamus and in the central gray of the tegmentum. Their termination-behavior occurred at low thresholds, and it was not possible to use high levels of current without completely disrupting them. At none of the tested current-values was there any indication that positively-reinforcing areas were significantly involved. Just as positively reinforcing points were found to be separate from the negative, negative ones were found that were separate from the positive.

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[14] Stein, *op. cit.*, 405-414.

[15] N. E. Miller, Motivational effects of brain stimulation and drugs, *Fed. Proc.*, 19, 1960, 846-854; Irving Geller and Joseph Seifter, The effects of meprobamate, barbiturates, *d*-amphetamine and promazine on experimentally induced conflict in the rat, *Psychopharmacologia*, 1, 1960, 482-492.

[16] Stein, *op. cit.*, 405-414.

SUMMARY

Allowing rats to initiate and terminate an electrical brain-stimulus separates their electrodes into four reinforcing types: positive, negative, mixed, and neutral. Rats with positive electrodes initiate the stimulus quickly and rarely terminate it, thus maximizing stimulation; those with mixed electrodes alternate between initiating and terminating it; those with negative electrodes terminate it quickly and rarely initiate it, thus minimizing stimulation; and those with neutral electrodes rarely initiate or terminate it. All four types of behavior are seen over wide ranges of current, although some rats change types with higher or lower currents. Self-stimulation is obtained with both positive and mixed electrodes, but, paradoxically, at much lower current-levels with the mixed.

A FURTHER COMPARISON OF KEY-PECKING WITH AN INGESTIVE TECHNIQUE FOR THE STUDY OF DISCRIMINATIVE LEARNING IN PIGEONS

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In a recent experiment on visual habit-reversal in the pigeon, Stearns and Bitterman compared the conventional key-pecking technique for the study of discriminative learning with a new ingestive technique that offers *S* a choice of grain-magazines rather than a choice of keys.[1] The comparison was prompted by the results of some early experiments with a variety of animals which suggested that discriminative learning may be particularly rapid when the discriminanda are visually differentiated food-objects which *S* is invited to ingest.[2] (The negative food-objects of those early experiments were saturated with quinine, glued to the substrate, or otherwise rendered inedible.)

In the key-situation used by Stearns and Bitterman, the pigeon pecked at one of two Plexiglas keys illuminated with lights of the colors to be discriminated. Correct choice turned off the key-lights and activated a grain-magazine whose aperture was illuminated with white light during the reinforcement-period; incorrect choice produced a time-out (*TO*) in darkness. In the magazine-situation, the pigeon attempted to take food from one of two grain-magazines whose apertures were illuminated with lights of the colors to be discriminated. Insertion of the head into the correct aperture caused the grain-tray to be presented, and the aperture remained illuminated during the reinforcement-period; incorrect choice produced a *TO* in darkness. Finding performance in magazine-situation to be markedly superior to performance in the key-situation, Stearns and

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[1] E. M. Stearns and M. E. Bitterman, A comparison of key-pecking with an ingestive technique for the study of discriminative learning in pigeons, this JOURNAL, 78, 1965, 48-56.

[2] J. R. Reighard, An experimental field study of warning coloration in coral reef fishes, *Publ. Carnegie Inst., Wash.*, 103, 1908, 257-325; David Katz and Gésa Révész, Experimental-psychologische Untersuchungen mit Hühnern, *Z. Psychol.*, 50, 1909, 93-116; W. T. Shepherd, Some mental processes of the rhesus monkey, *Psychol. Monogr.*, 12, 1910 (No. 52), 1-61.

Bitterman considered the possibility that the superiority was due, not to the use of an ingestive response, but to the fact that the magazine-animals were reinforced in light of the positive color while the key-animals were reinforced in white light. To test this hypothesis, they trained a control group of key-animals with the magazine-light always the same color as the positive key. Finding the performance of these animals to be just as good as the performance of the magazine-animals, they concluded that the ingestive indicator-response afforded no special advantage.

Object. The principal purpose of the present work was to reëxamine this conclusion. To test the hypothesis that the advantage of exposure to the positive color during reinforcement in the experiment of Stearns and Bitterman was great enough to mask a genuine (if less marked) advantage of the ingestive indicator, we compared the conventional key-technique (white magazine-light) with a modified magazine-technique, in which the color of the positive light was changed to white as soon as *S* inserted its head into the correct aperture, and remained white during the reinforcement-period. A secondary purpose of the present work was to try to determine why the colored magazine-light should be so effective. There was an indication in some supplementary results of Stearns and Bitterman that performance in the key-situation was facilitated, not only when the magazine-light was the color of the positive key, but also when it was the color of the negative key. This finding suggested that the magazine-light might serve a discriminative rather than a reinforcing function. To examine this possibility, two key-groups were trained from the outset with colored magazine-lights, the color of the positive key for one group and the color of the negative key for the other, following which certain further tests were made.

Method: Subjects. The *Ss* were 52 White Carneaux cocks maintained on a 24-hr. feeding schedule that kept them at 75% of their free-feeding weights. They were divided at random into 6 groups—4 of 10 *Ss* each and 2 of 6 *Ss* each.

Apparatus. Two different chambers were used, one for key-animals and the other for magazine-animals. Each chamber was a ventilated picnic-chest divided into two compartments by an aluminum panel, one compartment for *S* and the other for equipment. For the key-animals, the panel displayed three Lehigh Valley pigeon-keys at head-level and the aperture of a Gerbrands grain-magazine at floor-level below the center key (*CK*). For the magazine-animals, the panel displayed a single central *CK* at head-level and the apertures of two grain-magazines at floor-level below the *CK;* in the lower lip of each aperture was a recessed photocell which served to detect the insertion of *S*'s head, the necessary illumination being provided by the magazine-lamps used to flood the apertures with light, white or colored. The chests were cabled to programmers which presented the stimuli, registered responses, and determined their consequences. Records of the main events of each experimental session were made with Gerbrands six-channel event-recorders.

Procedure. After each *S* was magazine-trained (white light) and trained to peck the white *CK*, the experiment-proper was begun. The animals were trained in daily sessions of 40 trials with an intertrial interval (*ITI*) of 6 sec. in darkness. Each trial began with the illumination of the (white) *CK*. A peck at the *CK* extinguished it and presented the discriminanda (red and green side-keys for the key-animals, red and green magazine-apertures for the magazine-animals) whose spatial positions varied from trial to trial in accordance with selected Gellermann-orders. For the key-animals, a peck at the correct key turned off the side-key lights and activated the grain-magazine for 3 sec., during which time its aperture was illuminated with white light (for some animals) or with colored light (for others). For the magazine-animals, insertion of the head into the correct aperture activated the grain-magazine for 2.5 sec., during which time it was illuminated with white light. (The reinforcement-period was slightly shorter in the magazine-discrimination because the head was already in the aperture of the magazine when the grain-tray was brought up and a magazine-*S* therefore could take more than a key-*S* in a constant period of time; pilot work showed that the average magazine-*S* took as much grain in 40 2.5-sec. periods as the average key-*S* took in 40 3-sec. periods.) After each reinforcement, the 6-sec. *ITI* in darkness was initiated. An incorrect choice in each case turned off the discriminanda and initiated a 6-sec. *TO* in darkness, after which the *CK* was turned on and a peck at it produced the discriminanda once more. A limited correction procedure was used: no more than six errors (the initial error and five repetitive errors) were permitted on each trial. After the sixth error, a peck at the *CK* illuminated only the positive key or the positive magazine (guidance), and the animal was reinforced for response to it.

Two groups of 10 *S*s each—one in the key-situation and one in the magazine-situation—were trained *to criterion* on a red-green problem and then on a series of 26 criterion-reversals. The criterion was six or fewer *initial* errors in a single session with no more than three initial errors in the last 20 trials of the session. For half the *S*s in each group red was positive, and for the remaining *S*s green was positive, in the original problem (Reversal 0) and in all even-numbered reversals. Whenever any animal reached the criterion of mastery on a given day, the positive and negative colors were reversed on the following day. For the key-group, the magazine-light was white during the reinforcement-period, as it was for the magazine-group.

Two groups of 10 *S*s each—one in the key-situation and one in the magazine-situation—were trained for 97 days exactly as were the two criterion-reversal groups except that the positive and negative stimuli were reversed *each day* irrespective of performance. For both groups, the magazine-light was white during the reinforcement-period. Two groups of 6 *S*s each also were trained in a series of daily reversals, both in the key-situation; for one (positive magazine) the magazine-light on any day was the same color as the positive key on that day, while for the other (negative magazine) it was the same color as the negative key. After 35 days, the conditions of training were changed in various ways which are best described in conjunction with the results.

Results and discussion: Magazine- vs. key-discrimination. Mean initial errors to criterion for the two criterion-reversal groups are plotted in Fig. 1. The superioriy of the magazine-performance is evident. The point of maximal difficulty is lower and occurs earlier in the magazine-group than in the key-group, and the rate of improvement beyond the point of maximal difficulty is greater in the magazine-group than in the key-group. Analysis of variance yields a significant Groups-Effect ($F = 5.81$, $df = 1/18$, $p < 0.05$), a significant Reversal-Effect ($F = 4.42$, $df = 25/450$, $p < 0.01$), and a significant interaction ($F = 2.35$, $\mathrm{df} = 25/450$, $p < 0.01$).

In Fig. 2, two *within*-reversals learning curves are plotted for each group. One curve for each group shows the course of reversal late in training (pooled Reversals 23-26), and the other shows the course of reversal earlier in training (pooled Reversals 7-10), when mean number of errors to criterion for the magazine-group was about the same as it was late in training for the key-group. The curves are plotted in terms of mean number of initial errors per 10-trial block for the first day of reversal; they do not go beyond the first day, because in many cases the criterion was met in a single day. The two curves for each group are essentially parallel functions which differ in over-all level, which is precisely the result that has been obtained in previous experiments with rat and pigeon, and the early curve for the magazine-group is almost identical with the late curve for the key-group. These results suggest that the same processes are responsible for progressive improvement under the two

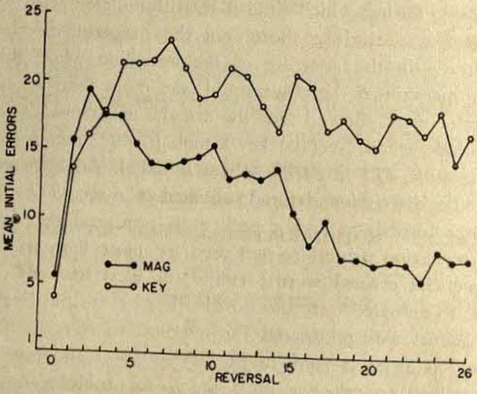

FIG. 1. CRITERION-REVERSAL PERFORMANCE IN THE KEY- AND MAGAZINE-SITUATIONS

conditions, but that they simply operate more efficiently under the magazine-condition.

In Fig. 3, mean initial errors per day are plotted for the daily-reversal groups. Again the curves show superiority of performance in the magazine-

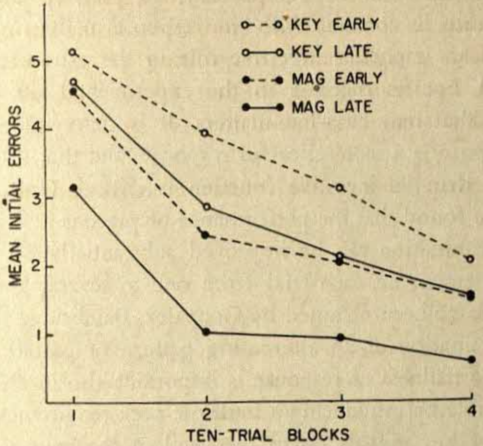

FIG. 2. WITHIN-REVERSALS LEARNING EARLY (REVERSALS 7-10) AND LATE (REVERSALS 23-26) IN THE TRAINING OF THE TWO CRITERION-REVERSAL GROUPS

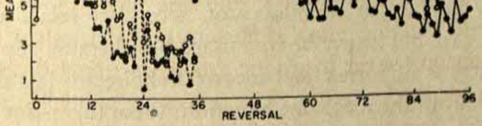

FIG. 3. PERFORMANCE OF THE FOUR DAILY-REVERSAL GROUPS

situation as compared with the key-situation. Analysis of variance based on the data of the two groups trained with the white magazine-light in 96 reversals yields a significant Groups-Effect ($F = 4.69$, $df = 1/18$, $p < 0.05$), a significant Reversals-Effect ($F = 6.46$, $df = 95/1710$, $p < 0.01$), and a significant interaction ($F = 1.40$, $df = 95/1710$, $p < 0.01$).

How is the superiority of the magazine-technique to be accounted for? One explanation may lie in the nature of the response. It is conceivable, for example, that ingestive responses are under especially precise stimulus-control, or that the nonreinforcement of an ingestive response is especially productive of frustration. Topographically, the pecking response made to the key has much in common with the response to the magazine, and it certainly plays an important ingestive role in the extra-experimental life of the animal, but its function in the experimental situation is rather different, and that may be what matters. It is conceivable also that the magazine-response is a more effortful response, and that its greater effortfulness rather than its ingestive function is critical. Graf, Bullock, and Bitterman have found that the performance of pigeons in a successive one-key color-discrimination can be improved substantially by increasing the response-requirement on each trial from one to several pecks,[3] and analogous results have been obtained by Gonzalez, Bainbridge, and Bitterman for the discrimination of an alternating pattern of partial reinforcement by rats.[4] If effortfulness of response is important also in choice-situations, it may be possible by introducing a multiple-peck requirement to bring the performance of key-animals up to the level of the magazine-animals, or even beyond.

Another possibility is that the superiority of the magazine-technique is due to the more immediate reinforcement which it provides. As shown by the pilot work on the amount of grain that key- and magazine-animals manage to eat with a constant reinforcement-time, and on the difference in reinforcement-time necessary to equate the amount of grain taken under the two conditions, the delay of primary reinforcement is about half-a-second greater in the key-situation than in the magazine-situation, where it is essentially zero. A delay of this magnitude does not ordinarily make much difficulty because an abundance of 'secondary reinforcers' are available to fill the gap, but it may be important here because the immediate afferent consequences of correct and incorrect choice are less different in the key-situation than in the magazine-situation. From this point of view, some minor changes in the sensory properties of the key-situation would be expected to improve performance.

The role of the colored magazine-light. As Fig. 3 shows, the performance of the key-groups trained with colored magazine-lights was markedly superior to that of the key-group trained with a white magazine-light, although it made no difference whether the color of the magazine-light was the same as the color of the positive key or the color of the negative key. Analysis of variance based on the data of the three key-groups for 35 reversals yields a significant Groups-Effect ($F = 9.48$, $df = 2/19$, $p < 0.01$), a significant Reversals-Effect ($F = 8.39$, $df = 34/646$, $p < 0.01$), and a significant interaction ($F = 3.72$, $df = 68/646$, $p < 0.01$). Orthogonal comparisons show a significant difference between the group trained with a white magazine-light and the pooled colored-light groups ($F = 18.12$, $df = 1/19$, $p < 0.01$), but not between the two colored-light groups ($F < 1$).

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[3] Virgil Graf, D. H. Bullock, and M. E. Bitterman, Further experiments on probability-matching in the pigeon, *J. exp. Anal. Behav.*, 7, 1964, 151-157.

[4] R. C. Gonzalez, Paul Bainbridge, and M. E. Bitterman, Discrete-trials lever-pressing in the rat as a function of pattern of reinforcement, effortfulness of response, and amount of reward, *J. comp. physiol. Psychol.*, 60, 1966, in press.

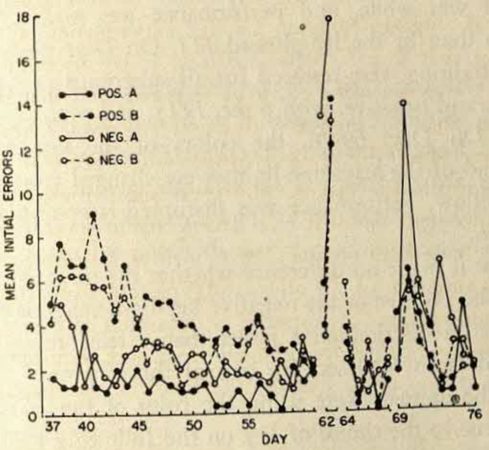

FIG. 4. PERFORMANCE IN SUBSEQUENT TESTS OF THE KEY-ANIMALS WHICH HAD BEEN TRAINED WITH MAGAZINE-LIGHT OF THE SAME COLOR AS THE POSITIVE AND THE NEGATIVE KEY

(In the first test, the duration of the *ITI* was increased for two subgroups. In the second test, the color of the magazine-light was white for all subgroups. Then the original training conditions were restored, after which, in the final test, the colors of key- and magazine-lights were changed from red and green to yellow and blue for all subgroups.)

Shown in Fig. 4 is the subsequent performance of the two key-groups trained with colored magazine-lights beginning with Reversal 36 (Day 37), when the *ITI* was increased to 30 sec. for half (the *B*-half) of each group.

(For the *A*-half of each group, the *ITI* remained 6 sec.) Because of the small *N*, the matching of subgroups could not be very close, but the first portion of Fig. 4 shows an unmistakable disruption in the performance of the *B*-subgroups, from which there was complete recovery as training continued. Analysis of variance yields only a significant *ITI*-effect ($F = 5.33$, $df = 1/8, p < 0.05$) and a significant Days-Effect ($F = 2.97, df = 24/192$, $p < 0.01$). On Days 62-63, the *ITI* was 6 sec. for all subgroups, but the magazine-light was white, and performance was markedly disrupted—much more so than by the lengthened *ITI*. On Days 64-68, the original conditions of training were restored for all subgroups (colored magazine-lights, positive and negative, with 6 sec. *ITI*), and performance improved precipitously. On Days 69-76, the colors of the key-lights (the discriminanda) and of the magazine-lights were changed from red and green to blue and yellow. Performance was disrupted temporarily, and then it improved again.

The fact that it made no difference whether the magazine-light was the same color as the positive or the negative key-light suggests that the magazine-light played a discriminative rather than a reinforcing rôle. The *ITI* was lengthened from 6-30 sec. for two of the subgroups to test the hypothesis that the animals were using the color of the magazine-light on any trial as a cue to the choice of key on the following trial. If that were the case, the colored-magazine animals might not have been learning to reverse at all. The consequent disruption of performance suggests that the color of the magazine-light did indeed have a discriminative function, although the fact that the performance of the long-*ITI* subgroups was no worse than that of the white-magazine key-group on the same experimental days (see Fig. 3) suggests that the colored-magazine animals had learned to reverse. That the improvement with continued training of the long-*ITI* subgroups must be taken to mean, not a decreasing dependence upon magazine-light color, but an increasing ability to bridge the intertrial interval, is shown by the drastic disruption in performance which occurred when the 6-sec. *ITI* was restored and the color of the magazine-light was changed to white. The extent of the disruption suggests that the animals trained with colored magazine-lights may not, after all, have learned to reverse.

The results for Days 69-76, when the discriminanda were yellow and blue, show the color-specificity which was reported also by Stearns and Bitterman.[5] Another curious effect which they reported and which is evident also in the present data is for the disruption produced by changes in color to be more marked on the *second day* of the change (the *first reversal* after the change). The effect is especially marked in the present data for the change from colored to white magazine-light. These findings, like all the others previously considered, are compatible with the conclusion that the colored magazine-light played a discriminative rather than a reinforcing rôle.

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[5] Stearns and Bitterman, *op. cit.*, 48-56.

SUMMARY

Pigeons were trained in daily and criterion discrimination-reversals, both by a key-pecking technique and by a magazine-technique (which offered a choice of differentially illuminated grain-magazines). Magazine-performance was substantially superior to key-performance. Supplementary data on the rôle of the magazine-light in the key-situation suggested that it served a discriminative rather than a reinforcing function.

INTENSITY OF THE VOICE AND THE THEORY OF ACTIVATION

By CONSTANCE HANF, University of Oregon Medical School, and
JOHN F. CORSO, State University of New York, Cortland

Changes in the intensity of an individual's voice during a conversation, and the relation of intensity to the concept of activation was the focus of this study. Two experiments, the second an expanded replication of the first, are reported.

The concepts of activation, mobilization of energy, and arousal have been defined from two positions: the energy of the overt response, and the level of energy of an individual about to make a response.[1] Investigators holding the former position have recorded magnitude or frequency of peripheral responses and sought to determine the lawful correspondence between these records and ordered magnitudes of the stimuli presented.[2] Those working within the second framework have inferred the presence of a dimension or state of activation from the magnitude of both physiological and peripheral responses to ordered magnitudes of incentive or deprivation.[3] Commonalities as well as differences in emphasis are apparent in the two positions. The present study combines both frameworks.

The theory of activation states that activity may be inferred to underlie behavior when, under conditions that vary in intensity or in magnitude of stress or deprivation, the physiological measure increases linearly while the nonphysiological, peripheral measure shows a U-shaped function, first increasing and then declining. Use of an intensively ordered stimulus judged to be motivating is necessary, but it may not be sufficient to produce the functions hypothesized. In this event, stimuli known to increase responding, for example, money, cigarettes, or shock have been added to the initial stimulating condition in order to increase the strength of motivation.

Although the concept of activation has been based primarily upon physiological evidence obtained over a range of incentives and deprivations,[4] its predictive

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* Received for publication November 13, 1963. This report is based upon a dissertation submitted by the first author in 1961 to the Graduate School of the Pennsylvania State University in partial fulfillment of the requirement for the Ph.D. degree.

[1] R. B. Malmo, Activation: A neuropsychological dimension, *Psychol. Rev.*. 66, 1959, 367-386; Measurement of drive: An unsolved problem in psychology, in M. R. Jones (ed.), *Nebraska Symposium on Motivation*, 1958, 229-264; D. B. Lindsley, Emotion, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1951, 473-516; Elizabeth Duffy, The psychological significance of the concept of "arousal" or "activation," *Psychol. Rev.*, 64, 1957, 265-275.

[2] F. A. Courts, Relations between muscular tension and performance, *Psychol. Bull.*, 39, 1942, 347-367.

[3] David Belanger, and S. M. Feldman, reported by Malmo, *op. cit.*, *Psychol. Rev.*, 66, 1959, 369, 371 f.

[4] G. J. Freeman, The relationship between performance level and bodily activity

power has been suggested for nonphysiological variables, but with somewhat less evidence.[5] This relation of a behavioral variable to *S*'s motivation as predicted by the theory of activation is the focus of the present study. Only a peripheral response was measured, and values obtained for this response were expected to assume an inverted U-shape if the conditions specified by the theory were met. In such an event, and in the absence of a physiological response measured simultaneously, the evidence would suggest the presence of an activating dimension during conversation.

Using this limited model of the theory of activation, intensity of the voice, a peripheral response was studied in relation to an inferred incentive, a condition involving prediction-of-performance during the experiment. A second variable commonly present in conversation was then introduced with the expectation that the two conditions would provide the conditions necessary to produce the inverted U-shaped curve hypothesized for the dependent variable.

EXPERIMENT I

Experiment I was designed to assess the effects of two verbally presented conditions on changes in the intensity of the voice, and to evaluate these changes in terms of the concept of activation.

Two conditions were employed: (a) predictions-of-performance by *E*, as a presumed incentive to *S* during the experiment, were presented at three relatively ordered levels of intensity: 'fail,' 'improve,' and 'succeed;' and (b) a definition of the experimental conversation was given as either a 'task' or a 'nontask.' The prediction-of-performance was assumed to be a necessary incentive, but perhaps insufficient, to produce the hypothesized inverted U-shaped function.

It was expected that the two conditions, prediction-of-performance and the definition-of-task, would produce significant differences in the dependent variable, intensity of the voice, and further, that the 'task' would produce a significantly greater difference in this measure than the 'Nontask' condition.

Method: Experimental design. To preclude familiarity with *E*'s voice an assistant, using a standard script for recruiting, obtained 54 men as the *S*s from freshmen and sophomore classes in an introductory course in psychology.[6]

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level, *J. exp. Psychol.*, 26, 1940, 602-608; R. G. Stennett, The relationship of performance level to level of arousal, *J. exp. Psychol.*, 54, 1957, 54-61; M. M. Schnore, Individual patterns of physiological activity as a function of task differences and degree of arousal, *J. exp. Psychol.*, 28, 1959, 117-118; W. W. Surwillo, Physiological factors in muscle-action potentials: EMG gradients, *J. exp. Psychol.*, 52, 1956, 263-272.

[5] Malmo, Anxiety and behavioral arousal, *Psychol. Rev.*, 64, 1957, 276-287.

[6] Constance Hanf, Intensity of vocal response as a function of vocal and verbal

A 2 × 3 factorial design was used to evaluate the hypotheses of the study. Nine *Ss* were assigned randomly to each of the six experimental conditions.

Experimental conditions. Each *S*, seated alone in an anechoic chamber, received inputs consisting of taped communications previously recorded by *E* whose taped voice served as the unseen second participant in the communication.[7] The communications were presented through a playback unit consisting of a tape recorder (Wollensak), an Eico amplifier, and a 15-in. coaxial loudspeaker (Altec). The amplifier controlled the sound-pressure level at which the taped communications reached *S*, *i.e.* at 60 ± 5 *db*, to correspond to the standard range for normal conversation. These settings were maintained throughout the experiment.

Responses of the *Ss* to the taped communications were picked up by a carbon head microphone (Western Electric), and were recorded on a level recorder (Bruel and Kjaer).

Preëxperimental communication in the laboratory was controlled. The assistant used an informal, standard script to get *S* to talk easily. *S* was then seated in the chamber, and asked questions to accustom him to hearing his own voice in the chamber. His part in the experiment was explained and he was urged to talk freely with the unseen *E*.

The chamber doors were closed and *S* remained there alone.

E's communications began the conversation and a timed silence on the tape followed each remark or question to allow *S* to respond. The pattern for the tapes consisted of comments and questions initiated by *E*, with occasional general replies intended to convey the idea that *E* was interested in and was attending to what *S* had just said, such as, "Mm-Hmm" and "That's interesting." The *Ss* had not been informed that the communications were taped.

At the conclusion of the experiment a written task was given as a pretext for *E* to send a written report immediately to each one in the "Fail" condition, indicating that his performance was better than the prediction communicated to him during the experiment. At the end of the experiment, all the *Ss* were invited to attend conferences to learn the actual purpose and design of the study.

Construction of the taped communications. The six tapes were identical in structure. Each had four parts: (a) a 'practice-period,' for introductory comments such as, "Hello, how does it seem in there all alone?" encouraged the *Ss* to respond; (b) a 'baseline-period' for measuring the intensity of *S*'s voice as he conversed with *E* about his academic and vocational plans; (c) an 'incentive-period' during which *E* supplied one of three randomly assigned levels that predicted to *S* his probable performance during the conversation; and (d) the 'experimental-period' during which the set was given that the experiment had not yet begun ('nontask,') or that it was now under way ('task.') The set was given prior to introducing successively the 10 questions or topics for conversation. These neutral topics explored the current nationwide concern about talented individuals.

One master tape was recorded, and contained all of *E*'s communications for the six experimental conditions. Copies of the various parts of this tape were made and were spliced into the appropriate position on each of the six tapes. Each of

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stimuli in interpersonal communication. Unpublished doctoral dissertation from Pennsylvania State University, 1951, 65.

[7] R. L. Berger and Eugene Ackerman, The Penn. State anechoic chamber, *Noise Control*, 2, 1956, 16-21.

these tapes, then, contained only the material specific to one of the six experimental conditions. Throughout the three experimental periods ('practice,' 'baseline,' and 'incentive') variously timed silences of 5 to 30 sec. were spliced into each tape. These followed upon *E*'s comments or questions and provided identical time for responding for each *S*. In the 'experimental-period' a timed silence of 45 sec. was spliced into the tape after each of the 10 topics or questions. The result was six tapes identical for the 'experimental-period' in verbal content, timing of communications, rate of speaking, nuance of *E*'s speech-patterns, intensity of *E*'s voice, and in the period of silence provided for *S*'s responses.

Results. The data of Experiment I consist of relative changes in intensity of *S*'s voice.

Values for this response were obtained in the following manner for both the 'baseline'- and the 'experimental-periods.' From the graphic record produced by the level-recorder, sound-pressure levels were sampled at 1-mm. intervals by reading vertically a scale graduated in mm. These were converted to *db* and averaged for each *S*. Since the experimental interest concerned the deviation from *S*'s own baseline, a corrective factor was applied to eliminate negative values. In each experiment the largest negative deviation was taken as the corrective factor and was added to every deviation in the array, thereby making all numbers positive.

Bartlett's test for homogeneity of variances indicated that differences between the six sample variances could have occurred by chance alone. Accordingly, the data were evaluated by an analysis of variance using the method of orthogonal polynomials.[8]

The results of the analysis indicate that only the condition defining the task produced a significant difference in the intensity of the voice ($p < 0.05$). Over-all means for the difference scores were: 'task,' 3.16 *db*; 'nontask,' 2.26 *db*. Group means for both 'task' and 'nontask' conditions, plotted at each level of incentive are shown in Fig. 1.

The inverted, U-shaped function assumed by the three means in the 'task' suggested the possibility that an increase in the number of experimental verbal conditions might produce significant changes in the intensity of the voice.

EXPERIMENT II

Experiment II essentially replicated Experiment I and introduced two additional variables into the conversation. The 'incentive'- and 'task'-conditions were retained as the variables of major theoretical interest. To these were added (a) intensity of *E*'s voice presented at two levels: 'high' and 'low'; and (b) verbal reinforcements presented at two levels: 'supportive' and 'critical.' The intensity of *E*'s voice could be ordered on an absolute scale of physical intensity while the two levels of the verbal reinforcement were considered to represent well-separated points on a continuum of criticism-praise.

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[8] A. L. Edwards, *Experimental Design in Psychological Research*, 1960, 229-232; E. C. Le Clerg, Mean separation by the functional analysis of variance and multiple comparisons, *Agric. Res. Serv.*, ARS-20-3, 1957, 6-13.

It was expected: (1) that the intensity of *E*'s voice and the verbal reinforcement would each produce significant changes in intensity of the voice; (2) that the 'high' level of the intensity of *E*'s voice would produce significantly greater differences in intensity of the voice than would the 'low' level of this condition; (3) that differences in intensity of the voice, the direction of which could not be specified, would occur between the two levels of verbal reinforcement, 'supportive,' and 'critical'; and (4) that the interaction of the three conditions (intensity of *E*'s voice, verbal reinforcements, and incentive) would produce significant changes in intensity of the voice.

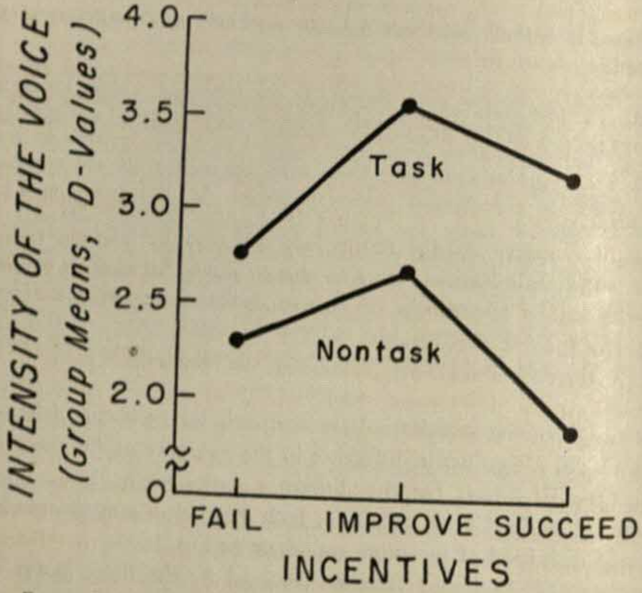

FIG. 1. CHANGES IN INTENSITY OF THE VOICE FOR "TASK" AND "NONTASK" AT EACH LEVEL OF INCENTIVE

Method: Experimental design. Following the procedures used in Experiment I, 108 men were obtained to serve as *Ss*. They were assigned randomly to the conditions of the experiment using a 2 × 3 × 2 factorial design.

Experimental conditions. These were identical to those described in Experiment

I with the exception that only the 'task' condition was used. The preëxperimental procedures were also identical to those used in Experiment I.

To obtain a specific level of intensity for E's voice, and to indicate to the Ss that microphonic adjustments were made, E randomly turned the volume-dial at the beginning of each experimental period and then brought it to one of two points previously calibrated; either to 15 *db* above or 15 *db* below the level (60 ± 5 *db*) at which it had previously been presented.

Verbal reinforcements were introduced after S had responded to Discussion Topics 2, 4, 7, and 9. An example of one supportive comment used was, "Lots of people find it hard to talk out loud and right off the cuff," while, "You seem to find it hard to talk out loud and right off the cuff," was considered to be its more critical counterpart.

Procedures identical to those used in Experiment I were employed in Experiment II. Although there were 12 experimental conditions, only 6 spliced tapes were necessary since amplifier adjustments controlled the remaining 6 experimental conditions.

Results. Bartlett's test for homogeneity of variances was applied to the 12 sample variances and the results indicated that the differences between the variances could have occurred by chance alone. An analysis of variance using the method of orthogonal polynomials was then performed to evaluate four of the five expectations.

Of the three variables manipulated, only the main effects produced by the variable intensity of E's voice. 'high,' and 'low,' reached statistical significance ($p < 0.001$). Over-all means for the corrected difference scores for the two levels of this variable were: 'high,' 15.74 *db*; 'low,' 10.99 *db*.

The first-order, interaction-effects of theoretical interest were not statistically significant, *i.e.* the interaction of the three levels of the incentive with the variable intensity of E's voice and with the verbal reinforcements. The significant interaction between the intensity of E's voice and the verbal reinforcements ($p < 0.05$) could not be evaluated within the paradigm of activation, because this interaction did not meet a requirement of the theoretical model, *i.e.* the inclusion of at least three ordered levels of an incentive or stress condition.

The significance of the second-order interaction ($p < 0.01$) and of its quadratic component ($p < 0.01$) did, however, permit further examination of the functions produced by specific combinations of the variables. Over-all means, using corrected difference-scores for the combined variables at each incentive level were: 'fail,' 69.9 *db*; 'improve,' 80.2 *db*; and 'succeed,' 83.3 *db*. When plotted, these over-all values did not result in the expected inverted U-shaped function.

Group means for each level of the significant first-order interaction involving intensity of *E*'s voice and verbal reinforcements were then plotted in relation to the three levels of incentive. Only one of the four possible combinations shown in Fig. 2 assumed an inverted U-shaped function; namely, the combination of intensity of *E*'s voice 'high,' verbal reinforcement 'supportive,' and the three levels of incentive, 'fail,' 'improve,' and 'succeed.'

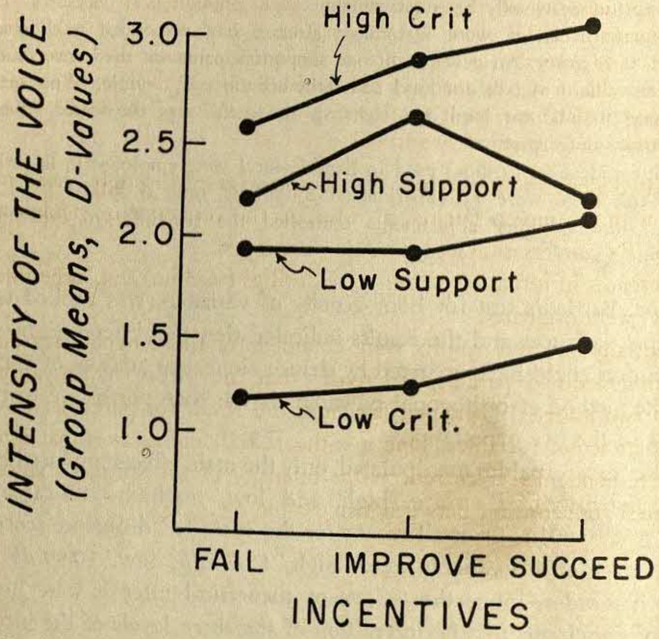

FIG. 2. CHANGES IN INTENSITY OF THE VOICE AS A FUNCTION OF THE COMBINED CONDITIONS INTENSITY OF *E*'S VOICE AND VERBAL REINFORCEMENTS AT EACH LEVEL OF INCENTIVE

DISCUSSION

In Experiment II the effects of only one combination of conditions met the criterion of an inverted U-shaped function and suggested that an activating dimension had been demonstrated. This combination of conditions included 'high' intensity of *E*'s voice, and 'supportive' verbal reinforcement, and the three presumed incentive conditions 'fail,' 'improve,' and 'succeed.' Group means plotted for specific levels of the remaining conditions contributing to the second-order interaction are presented in Fig. 2. They show similar but not inverted U-shaped functions.

Several explanations of these essentially negative findings are apparent. Restricting the independent variables to those common to spoken conversation, may have obscured effects which would otherwise be reflected in intensity of the voice—such nonverbal incentive conditions as money rewards, shock or competition might have been more successful. There may have been other actual changes in the voice that were not recorded. Furthermore, since the experimental design did not provide for simultaneous measurement of a physiological and peripheral response during the conversation, no strong theoretically-based conclusion could be reached for intensity of the voice without such physiological measures. Lastly, since Experiment I did show that labeling subsequent events as 'task' produced an effect upon intensity of the voice of the participants in the conversation, it seems reasonable to suggest that had this variable replaced prediction-of-performance as the incentive in Experiment II, the effects on intensity of the voice might have approximated the theoretical prediction.

S, seated alone in an anechoic chamber, conversed through a head microphone with an unseen person, *E*, who led the conversation. Unknown to *S*, all of *E*'s conversation and silences were taped.

Differences in intensity of the voice during 'baseline' and 'experimental-periods' were evaluated.

Essentially negative results were obtained for those hypotheses derived from the modified paradigm derived from the theory of activation, although two inverted U-shaped functions did result for particular levels of the experimental variables. One non-theoretical finding was that intensity of the respondent's voice reflects the intensity of the speaker's voice in a structured conversation between two people.

DISTAL AND PROXIMAL SIZE UNDER REDUCED AND NON-REDUCED VIEWING CONDITIONS

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Of the many aspects or properties of objects a person can perceive, the perception of size has loomed large as a field of investigation. A recent review by Epstein, Park and Casey attests to continuing interest in this area.[1] Early investigations dealt with the processes by which a person perceives the size of an object that accords with its real or distal size. There is ample evidence that a person can judge the distal size of an object accurately under favorable conditions. In addition to this ability, he can also judge the visual angle subtended by the object, the proximal size, with some degree of accuracy when instructed to do so, and under restricted viewing conditions.[2] Thus, one can perceive two attributes or properties of the size of objects, and theories on the subject have attempted to include both types of perception.

One early interpretation of perceived size was an extension of Titchener's context theory. Boring characterizes as core in the perception of size the sensory data derived from the size of the retinal image and as context all the sensory data pertaining to the distance of the object, as well as the properties of the brain that influence perception.[3] The perceived size was thought of as the integration of information from retinal size and from the context of the retinal image. Later, Boring suggested two extreme types of perception, representing in part the results of two observational attitudes. These correspond perhaps to Gibson's distinction between the visual field

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[1] William Epstein, John Park, and Albert Casey, The current status of the size-distance hypothesis, *Psychol. Bull.*, 58, 1961, 491-514.

[2] For the influence of instructions, see J. C. Baird, Retinal and assumed size cues as determinants of size and distance perception, *J. exp. Psychol.*, 66, 1963, 155-162; V. R. Carlson, Overestimation in size-constancy judgments, this JOURNAL, 73, 1960, 199-213; Size-constancy judgments and perceptual compromise, *J. exp. Psychol.*, 63, 1962, 68-73; Epstein, Attitudes of judgment and the size-distance invariance hypothesis, *J. exp. Psychol.*, 66, 1963, 78-83; A. S. Gilinsky, The effect of attitude upon the perception of size, this JOURNAL, 68, 1955, 173-192; Noël Jenkins and Ray Hyman, Attitude and distance-estimation as variables in size-matching, this JOURNAL, 72, 1959, 68-76. For the influence of restricted conditions, see A. H. Hastorf and K. S. Way, Apparent size with and without distance cues, *J. gen. Psychol.*, 47, 1952, 181-188; A. H. Holway and E. G. Boring, Determinant of apparent visual size with distance variant, this JOURNAL, 54, 1941, 21-37; William Lichten and Susan Lurie, A new technique for the study of perceived size, this JOURNAL, 63, 1950, 280-282.

[3] E. G. Boring, The perception of objects, *Amer. J. Phys.*, 14, 1946, 99-107.

and the visual world.[4] But Boring suggests that perceived size in fact falls between two extremes. Brunswik, in his probabilistic functionalism, also makes size a function of attitude. The perception of visual angle was thought to reflect an analytic attitude; whereas the perception of actual size was thought to reflect the realistic attitude.[5]

When two contrasting instructions are given, it is consistently found that different size estimations result. However, brief and ambiguous instructions, such as "make these objects look alike," lead some *S*s to set the variable stimulus close to the distal size and lead others to set it close to the proximal size.[6] Thus, it is not clear whether one type of perception is preferred or more natural for a person and whether there are different preferences in different viewing conditions.

The purpose of the present study was to investigate which type of perception is actually employed by *S*s under two different viewing conditions when no specific instructions were given. To investigate this problem, a task with learning and transfer sessions was employed which required *S* to form his own concept of what was necessary. *S*s were asked to discover the correct association between a number of standard and comparison stimuli. We called the comparison stimulus "correct" for some *S*s when it had the same measured distal size as the standard stimulus. For others we defined the comparison stimulus as 'correct' when it had the same proximal size as the standard stimulus. Furthermore, we had two viewing conditions: a non-reduced condition (normal unrestricted viewing) and a reduced condition (elimination of distance cues). The question asked was which response class is easier to learn under each of the two viewing conditions? The assumption was that more rapid learning would reflect the natural tendency of the perceiver.

METHOD

Apparatus and stimuli. The apparatus consisted of a lighted panel, used as a standard stimulus, and two sets of black rectangles, used as comparison stimuli. The lighted standard panel was located in a 4 × 5½ × 32-ft. reduction-tunnel. This was constructed by draping black cloth over two wires to cover the 32-ft. space.

The standard was a rectangular field centered at the eye-level of the seated *S*. This was provided by a light-proof box, one side of which was a uniformly illuminated milk-glass screen. It was located behind an opening in a flat-black board at the end of the tunnel. The size of the standard stimulus was varied by placing a rectangular frame in front of the milk-glass. There were five standard stimulus sizes: 9 × 0.9-cm., 22 × 2.2-cm., 27 × 2.7-cm., 33 × 3.3-cm., and 54 × 5.4-cm. The standard stimuli were placed at two distances in the tunnel.

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[4] Boring, Visual perception as invariance, *Psychol. Rev.*, 59, 1952, 141-148.

[5] Leo Postman and E. C. Tolman, Brunswik's probabilistic functionalism, in S. Koch (ed.), *Psychology: A Study of a Science*, I, 1959, 502-564.

[6] See, for example, Gilinsky, *op. cit.*, 173-192; B. E. Holiday, Die Grössenkonstanz der Sehdinge bei Variation der inneren and äusseren Wahrnehmungsbedingungen, *Arch. ges. Psychol.*, 88, 1933, 419-486; Sylvia Klimpfinger, Über den Einfluss von intentionaler Einstellung und Übung auf die Gestaltkonstanz, *Arch. ges. Psychol.*, 88, 1933, 551-598; T. M. Martin and R. W. Pickford, The effect of veiling glare on apparent size relations, *Brit. J. Psychol.*, 29, 1938, 91-103; M. R. Sheehan, A study of individual consistency in phenomenal constancy, *Arch. Psychol.*, 31, 1938 (No. 222), 1-95.

The comparison-stimulus panels were placed 10 ft. to the left of *S*. A panel consisted of 13 black rectangles pasted on an 80 × 100-cm. white background. The bases of the rectangles were at the same level, and the sizes were randomly ordered. The two panels differed in that the rectangles were in a different random order. Below each rectangle was a letter of the alphabet, starting with 'A' on the left and ending with 'M' on the right. The sizes of the comparison-stimuli on each of the panels were chosen as follows: five were the same size as the five standard stimuli; five were half the size of the standard stimuli (*e.g.* half of the 9 × 0.9-cm. rectangle was 4.5 × 0.45-cm.); and five were one-third the size of the standard stimuli. When duplicates were eliminated, there were altogether 12 comparison-stimuli. To make less obvious the gap between the 54 × 5.4-cm. and the 33 × 3.3-cm. stimulus, a 44 × 4.4-cm. stimulus was added. Thus, there were 13 comparison stimuli. Each of the stimuli, except the 44 × 4.4-cm. rectangle, was classified as 'correct' under one or more of the experimental conditions. The comparison-stimuli were presented without restriction in viewing for all the experimental conditions.

Procedures. When *S* was seated in front of the tunnel, the following instructions were given:

Instructions. The purpose of this experiment is to find out how students learn to associate two things. This is how we shall proceed: We will show you a vertical bar of light. Your task is to learn which of the bars on the panel to your left is associated with it. The same bar may occur more than once. First you must guess. I will tell you whether your choice is correct. If it is incorrect, you are to make another choice. You may look back and forth as often as necessary to make each choice. When you have made the correct choice for one bar, we shall go on to the next bar of light. Your task is completed when you have learned the correct association for all bars presented to you. Do you have any questions?

For half of the *S*s the lighted standard stimulus was first placed in the tunnel at 20 ft.; for the other half it was first placed at 30 ft. A trial began when *E* opened a guillotine door in front of *S*. Lifting the door confronted *S* with either a 1 x 1-ft. aperture or a ⅛-in. diameter aperture, depending on the experimental condition. Any given trial lasted until the correct response was given. When a correct response was given, *E* lowered the door. A block of trials consisted of the presentation of each of the five standard stimuli in random order. The criterion for learning was completion of a block of trials with no errors. When *S* met this criterion, he was sent out of the room and the distance of the standard stimulus was changed either to 30 ft. from 20 ft., or vice versa. At this time the panel with the comparison-stimuli was also changed. *S* was then brought back into the room and was told that his task was the same as before. For the sake of easier description throughout this paper, the first part of the experiment will be referred to as the learning session, and the second part will be referred to as the transfer session. The criterion of learning in the transfer session was the same as for the learning session.

White noise was present throughout the experiment in order to mask any sounds *E* might make while changing the standard stimulus.

After the transfer sessions were finished, each *S* was asked to estimate from memory the distances of the two locations used for the standard stimulus. Then he was asked to describe how he solved the problem.

Four experimental conditions were employed, as follows: In the nonreduced-distal (*NR-D*) condition, *S* viewed the standard stimulus through the large aperture with binocular regard. The cloth on one side of the tunnel was lifted up and two or three lights were visible on the tunnel floor, two when the standard was at 20 ft. and three when the standard stimulus was at 30 ft. Under this condition, the correct response was defined as the distal or objective size.

In the non-reduced-proximal (*NR-P*) condition, *S* viewed the standard stimulus under the same open conditions, but the correct response was now defined as the proximal, that is, the retinal stimulus-size. For example, when the standard stimulus was 9 × 0.9-cm. at 20 ft., the correct answer was the 4.5 × 0.45-cm., stimulus on the comparison stimulus panel (same visual angle).

In the reduced-distal (*R-D*) condition, the cloth was draped on both sides of the tunnel, and the only light in the tunnel was from the standard stimulus. *S* viewed the standard stimulus through the very small aperture with monocular regard. The correct response was defined as the distal size.

In the reduced-proximal (*R-P*) condition, the viewing condition was the same as in the *R-D* condition, and the correct response was the proximal stimulus size.

Subjects. Forty-nine Stanford undergraduates (41 men and 8 women) participated in the experiment as part of the introductory psychology course. All *S*s were naïve. The ratio of men to women was the same for the four conditions. Except for this restriction, the *S*s were randomly assigned to the experimental conditions. One *S* in the *R-D* condition was dropped from the analysis because he did not meet the criterion of learning in the learning session after one hour. After eliminating this *S*, there were 12 *S*s in each condition.

Experimenters. Two students at Stanford University served as *E*s. They were not naïve concerning the purpose of the experiment. A graduate student, man, changed the standard stimulus when he received a signal and an undergraduate, woman, read the instructions, recorded the *S*'s responses, and asked questions after the experiment.

RESULTS AND DISCUSSION

The main results were the number of errors made before meeting criterion. For each condition, the data obtained from the *S*s who started with the standard at 20 ft. were combined with those with standard at 30 ft. The same was done for the data from the transfer session. Fig. 1 represents the cumulative errors made after each two blocks of trials for the four experimental conditions in the learning and transfer sessions. Each point represents the cumulative number of errors made by 12 *S*s on 10 trials in the two blocks added to the errors made in previous blocks.

One should note that the ordinate applies to both the learning and the transfer sessions. The last point on each line represents the total number of errors for each experimental condition. The last point also represents the block of trials prior to the one on which the slowest learner in that particular condition met the criterion of no errors. One should not, however, attribute a great deal of significance to this value because it is the result of data from only a few *Ss*.

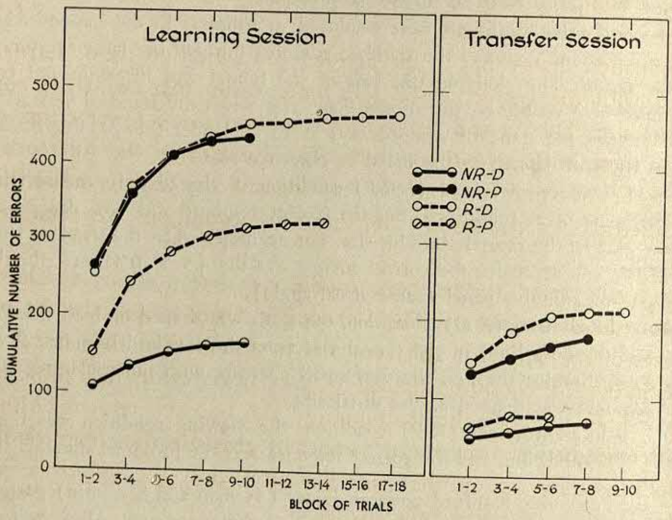

Fig. 1. Cumulative Errors as a Function of Block Trials for the Four Experimental Conditions

TABLE I

Average Number of Errors for Learning and Transfer Trials

		Non-Reduced Condition			Reduced Condition		
		NR-D	NR-P	Significance level	R-D	R-P	Significance level
Learning Trials	$\bar{X}$	13.75	36.00	$U=19$ $p<.002$	38.83	27.00	n.s.
	S^2	86.75	490.91	$F=5.658$ $p<.02$	277.97	256.91	n.s.
Transfer Trials	$\bar{X}$	5.67	14.83	$U=25$ $p<.02$	17.83	6.50	$U=11.5$ $p<.002$
	S^2	22.79	109.70	$F=4.813$ $p<.02$	40.33	20.27	n.s.

The mean number of errors per *S* and the variance in each condition are shown in Table I, with relevant statistical comparisons. The emphasis is upon the comparison of the distal and proximal conditions. The variance in the *NR-P* condition was significantly greater than the variance in the *NR-D* condition in both the learning and the transfer sessions. Because of this, a non-parametric test (the Mann-Whitney *U* Test) was used to compare the central values.

Examination shows that in the non-reduced condition the *S*s learned to respond with the correct distal size more easily than with the correct proximal size. The *S*s and the *NR-D* condition made fewer errors compared to those in the *NR-P* condition. The direction of the difference was found to be the same for both the learning and the transfer sessions, and both differences of the *NR-D* and the *NR-P* conditions were statistically significant.

The results from the reduced conditions were contrary to those of the non-reduced conditions. The *S*s found it easier to associate the proximal sizes of the standard and comparison stimuli than their distal sizes. The *S*s in the *R-P* condition tended to make fewer errors than those in the *R-D* condition in both the learning and the transfer sessions. Although the difference between the mean number of errors in these two conditions for both sessions was in the same direction, only the difference in the transfer session was statistically significant.

Our interpretation of the significant difference between the *NR-D* and the *NR-P* conditions is that it is the natural tendency of the *S*s to judge the distal size rather than the proximal size under normal unrestricted viewing. That is, a person tends to pay attention to the distal size rather than to the proximal size of an object; thus, the *S*s learned with fewer errors under the *NR-D* condition. In support of this, examination of the *S*s' errors under the *NR-P* condition showed that the errors tended to be in the direction of the distal size. Also, post-experimental interviews with the *S*s showed that most of them, under the *NR-P* condition, did not try to match the visual angle to solve the problem. Most of them estimated the distal size and then attempted to solve the problem by rote learning.

It is thought that these differential tendencies were due to past experience. In the normal life of a human being the chance of being reinforced for judging the proximal size is very rare indeed—unless one is being trained as an introspective psychologist or an artist. Thus, it is reasonable to expect that the *S*s would tend to estimate the distal size rather than the proximal size under normal viewing conditions.

At this point the limited generality of these findings should be noted. The distances employed in this study were from 10 to 30 ft., and at greater distances our conclusions may not hold. As a matter of fact, Gilinsky's study, which included distances between 100 and 4000 ft., suggests that a person tends not to estimate the distal size at greater distances.[7] Her *S*s reported that they made the

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[7] Gilinsky, *op. cit.,* 173-192.

'retinal' setting with much greater ease and confidence than the 'objective' match. Thus, the conclusion that the natural tendency of the *Ss* is to estimate the distal- rather than the proximal-size under normal viewing conditions should be limited to closer distances than that of Gilinsky's study.

As noted before, under the reduced conditions, the *Ss* in the *R-P* condition made fewer errors than those in the *R-D* condition in both the learning and the transfer sessions, although only the difference in the transfer session was statistically significant. A possible interpretation of the results is that the *Ss* tended or learned to estimate the proximal size under the reduced conditions; or, to state it differently, they can acquire or maintain a retinal attitude appropriate to the particular viewing condition. This interpretation is consistent with Rock and McDermott's notion that under reduced conditions the *Ss* respond to visual angle *per se.* They argue that the *Ss* under reduced conditions perceive size in terms of the extent to which an object "subtends much of the visual field or little of it, to loom large or small, quite apart from its objectively seen size."[8] Their main contention is that under reduced conditions perceived size is not a product of an *S*'s attempt to estimate the distal size.

A possible alternative hypothesis is that the *Ss* actually attempted to estimate the distal size and that the results are due to their assumption of equal distance from himself to the standard stimulus and to the comparison stimulus. The investigators who favor the equal distance hypothesis argue that under reduced conditions the *Ss* perceives size not in terms of proximal size, but perceives it in terms of distal size based on the assumption that the distances are equal for the two objects compared.[9] Data here from the post-experimental interviews does not support this hypothesis. Most of the *Ss* reacted with uncertainty to the question concerning the distance of the standard stimulus and often said their estimates were sheer guesses. The range of the distances by the *Ss* of the reduced conditions was 2 to 20 ft., and almost two-thirds of them estimated the distance to be less than 4 ft. for both learning and transfer sessions. This does not support the prediction of 10 ft. derived from the equal distance hypothesis. Furthermore, there were 3 *Ss* who clearly did not assume equal distance for the two stimuli compared. These *Ss* reported that the larger standard stimuli appeared closer than the smaller one and vice versa. In fairness to the hypothesis, however, it should be noted that the distance data were obtained during the post-experimental interview and were based on memory. Thus, it is possible that the perceived distances during the experimental sessions might have been different from their later estimates.

Although we believe that Rock and McDermott's assertion accounts for our results under reduced conditions better than the equal-distance hypothesis, we do not suggest that the *Ss* were 'consciously aware' of the proximal size or the visual angle. If the *Ss* were responding in terms of visual angle, they did not succeed in communicating this to *E* at the end of the experiment. In the post-experimental interview, most of the *Ss* in the *R-P* condition reported that they were trying to match the "sizes" of the standard- and comparison-stimuli, but left a clear impression that they had no idea of a distinction between the visual field and the visual world, or between proximal and distal sizes. Using verbal report as an index of their phenomenal experience, one cannot say that the phenomenal experiences of size under the reduced and non-reduced conditions were different. It is thought that, without a specialized language system such as the one used by psychologists, describing the phenomenal experience of retinal size would be difficult. Possibly the phenomenal experience of retinal size is hard to attain without the specialized language system.

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[8] Irvin Rock and William McDermott, The perception of visual angle, *Acta Psychol.* 22, 1964, 119-134.

[9] R. S. Woodworth and Harold Schlosberg, *Experimental Psychology*, 1954, 481; Epstein, Park, and Casey, *op. cit.*, 491-514; Hans Wallach and V. V. McKenna, On size-perception in the absence of cues for distance, this JOURNAL, 73, 1960, 458-460.

SUMMARY

The purpose of the experiment was to investigate whether there was a tendency to judge more readily distal or proximal size under reduced and non-reduced viewing conditions when no specific instructions were given. A task requiring *S* to form his own concept of the stimulus variable was employed. There were four experimental conditions. In the non-reduced-distal (*NR-D*) condition, *S* viewed the standard with binocular regard and with distance cues. *S* was required to associate the distal size of standard and comparison stimuli. In the non-reduced-proximal (*NR-P*) condition, *S* viewed the standard stimulus with binocular regard, and his task was to associate the proximal sizes. In the reduced-distal (*R-D*) condition, *S* viewed the standard stimulus under restricted viewing conditions with monocular regard and his task was the same as in the *NR-D* condition. In the reduced-proximal (*R-P*) condition, the viewing condition was the same as in the *R-D* condition and *S*'s task was to associate the proximal sizes.

It was found that under the non-reduced condition the *Ss* learned to associate the distal size more easily than the proximal size. This was thought to be due to the natural tendency of *Ss* to respond to the distal size rather than the proximal size under normal unrestricted viewing conditions. Under the reduced conditions, the *Ss* tended to associate more rapidly the proximal sizes of the standard and comparison stimuli. It is argued that *S* acquired or maintained retinal attitude under reduced conditions.

SOME EFFECTS OF PARTIAL REINFORCEMENT WITH SHOCK ON CLASSICALLY CONDITIONED HEART-RATE IN DOGS

By ROBERT D. FITZGERALD, University of Oregon Medical School

Experiments on classical conditioning in infrahuman *S*s often have failed to show a difference in resistance to extinction after partial as compared with consistent reinforcement.[1] Where the partial-reinforcement effect (*PRE*) has appeared in such experiments, furthermore, it usually has taken the form, not of a Treatments-effect but of a Treatments × Trials interaction.[2] It is evident that further research is needed to define the condition under which the PRE occurs in classical conditioning. The purpose of the present work was to extend the study of partial reinforcement in classical conditioning to a heart-rate response in dogs. Two experiments are reported, one with equated trials and a second with equated reinforcements.

METHOD

Subjects. The *S*s were 60 naïve mongrel dogs ranging in weight from 30-40 lb. They were purchased from, and maintained by, the Department of Animal Care of

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[1] B. D. Berger, Matthew Yarczower, and M. E. Bitterman, Effect of partial reinforcement on the extinction of a classically conditioned response in the goldfish, *J. comp. physiol. Psychol.*, 59, 1965, 399-405 (Experiments 1, 2, and 3; R. C. Gonzalez, Nicholas Longo, and M. E. Bitterman, Classical conditioning in the fish: Exploratory studies of partial reinforcement, *J. comp. physiol. Psychol.*, 54, 1961, 452-456; R. C. Gonzalez, Sandra Milstein, and M. E. Bitterman, Classical conditioning in the fish: Further studies of partial reinforcement, this JOURNAL, 75, 1962, 421-428; Nicholas Longo, Sandra Milstein, and M. E. Bitterman, Classical conditioning in the pigeon: Exploratory studies of partial reinforcement, *J. comp. physiol. Psychol.*, 55, 1962, 983-986 (Experiment 2); Earl Thomas and A. R. Wagner, Partial reinforcement of the classically conditioned eyelid reponse in the rabbit, *J. comp. physiol. Psychol.*, 58, 1964, 157-158.

[2] Berger, Yarczower, and Bitterman, *op. cit.*, 399-405 (Experiments 4 & 5); R. D. Fitzgerald, Effects of partial reinforcement with acid on the classically conditioned salivary response in dogs, *J. comp. physiol. Psychol.*, 56, 1963, 1056-1060; R. C. Gonzalez, R. M. Eskin, and M. E. Bitterman, Further experiments on partial reinforcement in the fish, this JOURNAL, 76, 1963, 366-375; E. J. Wyers, H. V. S. Harmon, and M. J. Herz, Partial reinforcement and resistance to extinction in the earthworm, *J. comp. physiol. Psychol.*, 57, 1964, 113-116; Longo, Milstein, and Bitterman, *op. cit.*, 983-986 (Experiment 1); A. R. Wagner, Shepard Siegel, Earl Thomas, and G. D. Ellison, Reinforcement history and the extinction of a conditioned salivary response. *J. comp. physiol. Psychol.*, 58, 1964, 354-358.

the University of Oregon Medical School. There were 27 *Ss* in Experiment I and 33 in Experiment II.

Apparatus. The experiments were conducted in a commercially manufactured, sound-insulated room. The *Ss* were restrained in a canvas sling attached to a wooden frame similar to the one employed by Church and Black.[3] The *CS* was a 350-cycle tone presented at 80 db. sound-pressure level through a speaker located behind *S*. The *US*, which was a 9.9 ma., 60-cycle electric shock, was delivered through plate-electrodes ½ in. in diameter that were taped to the rear limbs. The voltage-drop across a 100-Ω resistor in series with *S* was monitored with an oscilloscope on each shock-trial to insure that the amperage of the *US* remained constant both within and between trials. Stainless-steel wires sutured over the rib-cage just behind the front limbs served as *EKG*-electrodes. Recordings of heart-rate and *CS-US* events were made on a polygraph.

Procedure. To reduce struggling during conditioning, all the *Ss* were given one 2-hr. period of adaptation to the sling on each of three successive days. During the final hour of the third day, 60 *CS*-alone trials at 1-min. intervals were given in an effort to habituate the orienting heart-rate response to the tone. Acquisition and extinction were studied on the fourth day. During extinction, all trials for all the *Ss* were nonreinforced. After the last extinction-trial, every *S* was given one unimpaired presentation of the *US*. The duration of the *CS* was 13 sec. and that of the *US* 3 sec; the *CS-US* interval was 10 sec. with the *US* overlapping the final 3 sec. of the *CS*.

The *Ss* of Experiment I were assigned randomly to three groups of equal size ($N = 9$)—two experimental groups (100% and 50%) and a backward-conditioning control. All *Ss* received 10 *CS*-alone trials at 1-min. intervals followed by 12 acquisition-trials and 30 extinction-trials. In the 100% condition, all acquisition-trials were reinforced, while in the 50% condition half were reinforced and half were nonreinforced, in Gellermann order. For the backward condition, the *US* preceded the *CS* by 1 min. on each trial. The intertrial intervals (*ITI*s) during acquisition and extinction varied from 3-4 min. with a mean of 3.5 min.

In Experiment II there were a 100% group ($N = 12$), a 50% group ($N = 12$), and a backward-conditioning control group ($N = 9$). Prior to the start of acquisition, all the *Ss* received 10 *CS*-alone trials at 1-min. intervals. During acquisition, the 50% group received 12 reinforced and 12 nonreinforced trials with the same mean *ITI* (3.5 min.) as in Experiment I. The 100% group was given 12 reinforced trials with *ITI*s identical to those between reinforced trials in the 50% group—the mean (6.5 min.) was about twice as long as for the 100% group of Experiment I. The backward-conditioning control group was treated like the 50% group except that no forward pairings of the *CS* and *US* were given. On half of the trials the *US* preceded the *CS* by either 45, 60, or 75 sec., and on half of the trials no *US* was presented. In extinction, the mean *ITI* for the 50% group, for the backward group, and for one half of the 100% group was 3.5 min., while for the other half of the 100% group it was 6.5 min. The division of the 100% group was intended to check for the possible effects of changing the *ITI* from acquisition to extinction.

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[3] R. M. Church and A. H. Black, Latency of the conditioned heart rate as a function of the CS-US interval, *J. comp. physiol. Psychol.*, 51, 1958, 478-482.

RESULTS

A measure of conditioning was obtained for each *S* by subtracting the number of heart-beats during the 10 sec. preceding the onset of the *CS* (Pre-*CS*) from the number during the 10-sec. *CS-US* interval. The resulting difference-score (the *CR*) was transformed into a beats-per-minute (b.p.m.) index. The cardiac response to shock (the *UR*) was measured during a 10-sec. period beginning ½ sec. after the offset of shock.

The main results of Experiment I are plotted in Fig. 1. At left are the acquisition-data in two-trial blocks, beginning with the last two *CS*-alone trials prior to the start of conditioning. Analysis of variance yields a significant Treatments-effect ($F = 9.88$, $df = 2/24$, $p < 0.01$) indicating successful conditioning, and orthogonal comparisons show significant differences between 100% and backward groups ($t = 4.23$, $df = 16$, $p < 0.01$) and between 50% and backward groups ($t = 3.19$, $df = 16$, $p < 0.01$). For the 100% and 50% groups, there was a reliable Treatments × Trials interaction ($F = 4.48$, $df = 5/80$, $p < 0.01$), but pre-*CS* performance was not significantly different for the three groups.

Since some struggling is to be expected in conditioning situations like the present one and since struggling is known to increase heart-rate, an attempt was made to determine if the experimental groups were differentially affected. To that end, the number of trials upon which muscle-action potentials were present in the *EKG*-tracings during the *CS* was counted. The numbers for the 100%, 50%, and backward groups were 16 (no. of *S*s = 5), 12 (no of *S*s = 4), and 2 (no of *S*s = 2), respectively. When means were recalculated with these trails eliminated, the acquisition-curves shown in Fig. 1 were lowered on trial-block 3 from 49.5-39.5 b.p.m. for the 100% group, and on trial-blocks 5 and 6 combined from 29.2-20 b.p.m. for the 50% group. The other points in the curves were relatively unchanged.

At the right in Fig. 1 the extinction-performance is plotted in three-trial blocks. The curves fail to provide any evidence for the *PRE*. With the exception of trial-block 2, the 100% curve is consistently higher than the 50% curve, and it seems to decline at a slower rate. Analysis of 100% and 50% data yielded neither a significant Treatments-effect nor a significant interaction of Treatments × Trials. During extinction only six action-potential trials were scored for the 100% and 50% groups together, and their effect was negligible.

An analysis of the *UR*-data indicated that the mean response to shock in the 100% group decreased significantly from 160-138 b.p.m. during acquisition (t for related measures $= 2.66$, $df = 8$, $p < 0.05$). That this change was due to *CS-US* pairings and not simply to habituation is suggested by the fact that the *UR*s for the backward group showed essentially no change during conditioning even though they had received the same number of shocks at the same intervals as the 100% group. The *UR*s for the 50% group also failed to change appreciably during conditioning, but these *S*s, of course, received only half the number of *US*s as did those of the other two groups. On the shock-alone trial following extinction, the *UR* of the 100% group recovered to 165 b.p.m.

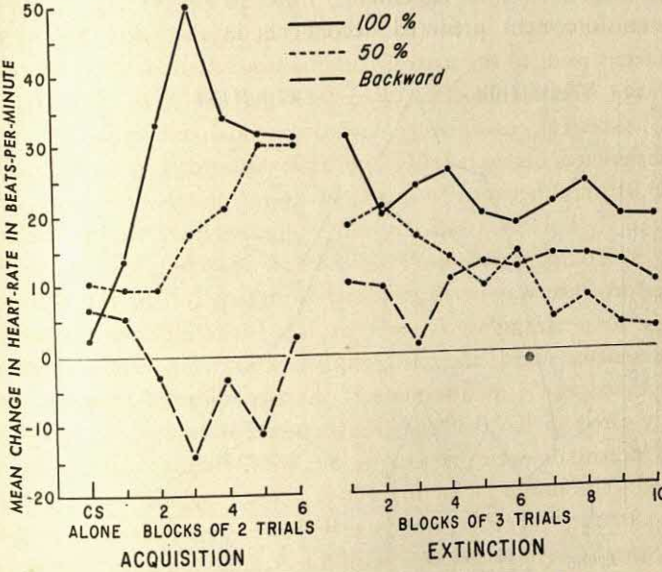

FIG. 1. RESPONSE TO THE *CS* DURING ACQUISITION AND EXTINCTION IN EXPERIMENT I.

The results of Experiment II are plotted in Fig. 2. The acquisition-curve for the 50% group was constructed by averaging over successive blocks of two reinforced trials; the values were computed by summing the (*CS*)-(Pre-*CS*) conditioning-scores over blocks of trials that began and ended with a reinforcement, and dividing by the number of trials in the block. The 50% and 100% groups are, therefore, matched at each point in their curves in terms of total number of reinforcements received. The points for the backward group are based on averages over successive blocks of two *US-CS* trials. Plotted at the far left are the mean heart-rates on the last two *CS*-alone trials prior to the start of conditioning. Inspection of Fig. 2 shows that the magnitude of the *CR* for each of the experimental groups reached a higher level than that of the backward group. The mean *CR* over all acquisition-trials was 36.8 b.p.m. for the 100% group, 44.9 b.p.m., for the 50% group, and 8.63 b.p.m. for the backward group. The difference between each experimental group and the backward group was significant beyond the 1% level, but the difference between the two experimental groups did not approach significance. Heart-rates during the Pre-*CS* interval were not significantly different for the three groups.

Nonreinforcement produced decrements in the *CRs* of the 50% *Ss* during the first half of acquisition but not during the second half of acquisition. For the first 12 acquisition-trials, 10 *Ss* showed a mean loss of 13 b.p.m. on trials following nonreinforced trails as compared with trials following reinforced trials. On the last 12 trails, performance following reinforced and nonreinforced trials was approximately the same.

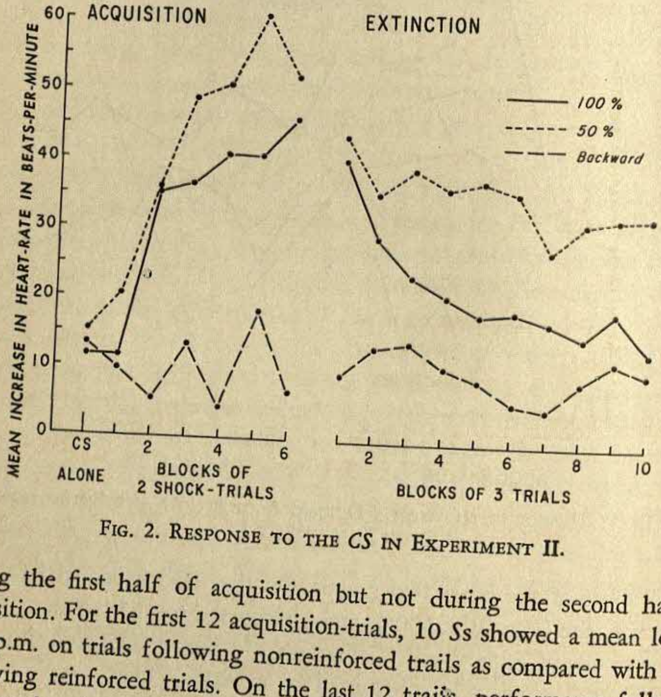

FIG. 2. RESPONSE TO THE *CS* IN EXPERIMENT II.

The percentages of trials upon which struggling occurred during acquisition for 100%, 50%, and backward groups were 13.9% (no. of *Ss* =6), 20.8% (no. of *Ss* = 7) and 0.7% (no. of *Ss* = 2), respectively. The only major effect of struggling was to inflate the mean *CR* for the 50% group on trial-blocks 3-6. With struggling trials removed at these points, the curve for the 50% group falls to the level of the 100% curve.

Extinction is plotted for blocks of three trials at the right of Fig. 2. (The data for the two subgroups in the 100% condition were combined since the intertrial interval did not have a differential effect on their performance.) Contrary to the results of Experiment I, partial reinforcement in this experiment seemed to produce more resistance to extinction than did 100% reinforcement. Although both groups began extinction at about the same level of response, by the end of extinction the 100% curve had fallen close to the level of the backward curve while the 50% curve had decreased by only 10.5 b.p.m. Analysis of variance does not, however, quite yield a significant Treatments-effect ($F = 3.14$, $df = 1/22$, $0.10 < p > 0.05$). The Treatments × Trials interaction is significant ($F = 2.09$, $df = 9/198$, $p < 0.05$), as is the Trials-effect ($F = 4.99$, $df = 9/198$, $p < 0.01$). Struggling during extinction was infrequent and did not affect the analysis.

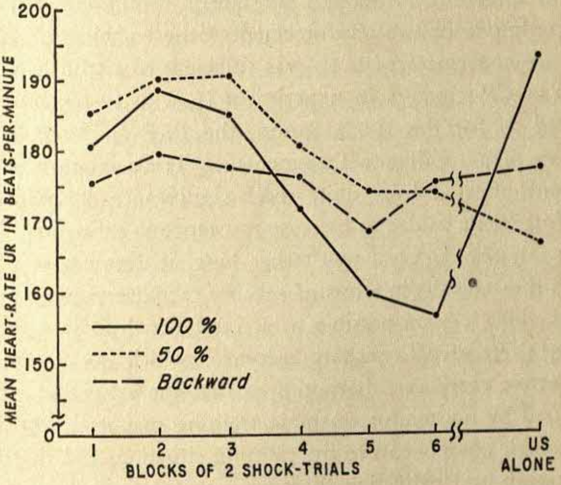

FIG. 3. RESPONSE TO THE *US* IN ACQUISITION AND ON A SHOCK-ALONE TRIAL FOLLOWING EXTINCTION IN EXPERIMENT II.

The *UR*-data for Experiment II are plotted in Fig. 3, which shows that the *UR* for both experimental groups tended to decrease in the course of acquisition. This change was highly significant for the 100% group ($F = 5.38$, $df = 5/55$, $p < 0.005$) but not for the 50% group ($F < 1$). The *UR* for the backward *Ss* changed very little ($F < 1$) even though they received the same number of shocks at the same intervals as the experimental *S*s. Furthermore, the recovery of the *UR* in the 100% group on the shock-alone trial shown on the far right in Fig. 3 was significant, as measured in relation to the *UR* on trial-block 6 of acquisition ($t = 6.84$, $df = 11$, $p < 0.001$). These results, which are consistent with those of Experiment I, support the notion that the *UR*-diminution was somehow contingent upon *CS-US* pairing.

DISCUSSION

Experiment I failed to provide any evidence for the *PRE*, but the *CR*-level in the 50% group at the end of acquisition with struggling-trials removed was quite low. Although substantial conditioning is known to occur with only one or two reinforcements,[4] the combination of six reinforced and six nonreinforced trials was sufficient to establish only a minimal amount of *CR*-strength. In Experiment II, with 12 reinforcements for 50% as well as for the 100% group, the *PRE* appeared in the now familiar form of a significant Treatments × Trials interaction; although the Treatments-effect did not quite reach significance, it seems clear that partial reinforcement tended to increase resistance to extinction.

Since the intertrial interval was rather long, it does not seem plausible to deal with these results in terms of sensory carryover;[5] nor, since the *US* was noxious, does it seem possible to account for them in terms of frustration.[6] An alternative to sensory carryover which does seem plausible here is extinctive carryover; the notion is that the extinction which tends to be produced by nonreinforcement in training may itself contribute certain characteristic components to the training situation, and that the conditions of training and extinction therefore are more similar for the 50% than for the 100% group.[7] The concept of punishment also may be useful in accounting for the present results if extinction is treated as the acquisition of response incompatible with the *CR*; on reinforced trials following unreinforced trials, the 50% group is punished by the noxious *US* for making such incompatible responses and therefore is less likely to make them either in the later stages of training or in extinction. One prediction from this view is that *US*-intensity and percentage of reinforcement will interact in determining resistance to extinction, the magnitude of the *PRE* increasing with intensity.

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[4] Church and Black, *op. cit.*, 478-482.

[5] V. F. Sheffield, Extinction as a function of partial reinforcement and distribution of practice, *J. exp. Psychol.*, 39, 1949, 511-526.

[6] Abram Amsel, The role of frustrative nonreward in noncontinuous reward situations, *Psychol. Bull.*, 55, 1958, 102-119.

[7] R. C. Gonzalez and M. E. Bitterman, Resistance to extinction in the rat as a function of percentage and distribution of reinforcement, *J. comp. physiol. Psychol.*, 58, 1964, 258-263; Gonzalez, Eskin, and Bitterman, *op. cit.*, 366-375.

A *UR*-decrement similar to that found in the 100% groups of these experiments has been reported in work with the eyeblink and with the GSR,[8] but the salivary *UR* to acid has been found to increase over reinforced trials.[9] It might be argued that the *UR*-diminution in the present case was simply an example of the operation of the 'law of initial values' according to which the magnitude of an autonomic reaction to a stimulus is an inverse function of the level of autonomic activity preceding the stimulus.[10] Thus, the development of the *CR* would make heart-rate high just before the *US* and a diminished *UR* therefore would be expected. Furthermore, the extinction of the *CR* preceding the shock-alone trial would allow the *UR* to recover. This 'law' would imply a positive correlation between *CR*-magnitude and *UR*-decrement, and, to check on this possibility, the *UR*-decrements from trial-block 1 to trial-block 6 for the 100% *S*s in Experiment II were compared with their mean *CR*s over trial-blocks 4-6. This comparison provided a rank-order correlation-coefficient of —0.33, insignificant and in the opposite direction from that predicted. Since the 50% *S*s reached a *CR*-level equal to that of the 100% *S*s, the fact that they did not show a significant *UR*-decrement also contradicts this explanation. It is not possible, however, on the basis of these data to conclude that the high heart-rate during the *CS* did *not* affect the *UR*. One procedure that should minimize such an effect would be to employ a much shorter *CS-US* interval during conditioning, perhaps on the order of 3 sec.

SUMMARY

Two classical conditioning experiments are reported in which the effects of random partial reinforcement (shock) on a heart-rate response in dogs were studied. With number of trials equated in Experiment I, no *PRE* was found. With number of reinforcements equated in Experiment II, a *PRE* appeared in the form of a Treatments × Trials interaction. In both experiments, the *UR* for the 100% group showed a significant decrement which did not appear to be simple adaptation to the shock.

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[8] H. D. Kimmel and H. S. Pennypacker, Conditioned diminution of the unconditioned response as a function of the number of reinforcements, *J. exp. Psychol.*, 64, 1962, 20-23.

[9] Fitzgerald, *op. cit.*, 1056-1060.

[10] J. I. Lacey, The evaluation of autonomic responses: Toward a general solution, *Annal. N.Y. Acad. Scien.*, 67, 1956, 126.

THE EFFECT OF VARIATION IN SPECIFIC STIMULI ON MEMORY FOR THEIR SUPERORDINATES

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One of the widely accepted notions regarding human behavior concerns the relationship between repetition—practice, exercise, frequency—and retention. Summarized by the cliche, "Practice makes perfect," the general idea was given scientific status by such pioneers as Ebbinghaus and Thorndike. Its validity has, of course, not gone unquestioned. Early challengers include, among others, Gottschaldt with his data on the recognition of hidden forms,[1] Dunlap in his demonstration involving "negative practice,"[2] and, on the basis of results from drawing lines while blindfolded, even Thorndike himself.[3] More recently, the generalization has been under attack by the protagonists in the one-trial learning controversy.[4]

Dunlap focused on the question of *what* is repeated; Gottschaldt on *where* it is repeated (the question of context); and Thorndike on *how* it is repeated. During the past forty years, numbers of investigators have explored various aspects of these not-independent questions, especially those treating the effects of changed context at the time of recall. Few, if any, investigations have sought to determine the effects on memory of varying, in successive presentations, the "what-is-repeated." Essentially a problem in memory for classes, the question in its simplest form is: Will one be better able to recall x if he has been exposed to the sequence x_a, x_a *or* x_b, x_b, rather than to the sequence x_a, x_b? Will he recall x better if presented x, twice as an aspect of ax *or* bx, than if he experienced ax and bx each once? Some of the literature on the effects of varied experience—*e.g.* that treating early environment

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[1] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren, *Psychol. Forsch.*, 8, 1926, 261-317.

[2] Knight Dunlap, A revision of the fundamental law of habit formation, *Science*, 67, 1928, 360-362.

[3] E. L. Thorndike, *Human Learning*, 1931, 8-12.

[4] Irvin Rock, The role of repetition in associative learning, this JOURNAL, 70, 1957, 186-193; B. B. Murdock and A. J. Babick, The effect of repetition on the retention of individual words, this JOURNAL, 74, 1961, 596-601; R. B. Evans and K. M. Dallenbach, Single-trial learning: A stochastic model for the recall of individual words, this JOURNAL, 78, 1965, 545-556.

and later learning[5] and the evolution of concepts[6]—suggest that varied presentations may be facilitating. The reports of detrimental effects of contextual change at the time of recall,[7] as well as inferences from some of the work on variability of stimulus compounds, might, on the other hand, lead to the opposite conclusion.[8]

In an effort to provide some answers to the questions raised above, three separate experiments were performed. The first two, 'qualitative' in the sense of asking "yes" or "no" about the effects of varied experience, differ primarily in the type of stimuli used, pictures in the first instance, words in the second. The third experiment, essentially quantitative in nature, and extending the rationale of the first two, explores the effects of different degrees of variation.

EXPERIMENT I

Method: (1) Materials. From pictures of two different specimens of 20 easily recognizable and identifiable objects (*e.g.* a red apple was arbitrarily designated specimen *A*, a yellow one, specimen *B*; a man's shoe, *A*, a woman's shoe, *B*, etc.), colored slides were prepared, the within-pair sizes of the pictured objects being approximately equal. The slides were assembled into four series, each containing two instances of every object as follows:

Series 1: Slides 1-20: all *A*-specimens
  Slides 21-40: *A* specimens repeated for one half of the slides, *B*'s presented for the other half.
Series 2: Slides 1-20: all *A*-specimens
  Slides 21-40: *B* specimens of the objects presented as *A* in slides 21-40 of Series 1 and vice versa
Series 3: Slides 1-20: all *B* speciments
  Slides 21-40: *B* specimens repeated for one half of the slides, *A* presented for the other half.
Series 4: Slides 1-20: all *B* specimens
  Slides 21-40: *A* specimens of the objects presented as *B* objects in slides 21-40 of Series 3 and vice versa.

Within each set the order of presentation of repeated (*R*) and different (*D*) specimens was balanced: *RDDRRDDR* or vice versa. No sequences or reversed sequences of objects were duplicated from first to second half of the series, and no object appeared in a highly favored or unfavored position for both presentations.

(2) Procedure. Slides were presented to the *S*s in groups by means of a Kodak Carousel 35 mm. projector with 2-sec. exposures and 2-sec. intervals between slides. The only directions given the *S*s before presenting the slides were to alert

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[5] W. R. Thompson and Theodore Schaefer, Jr., Early environmental stimulation, in D. W. Fiske and S. R. Maddi (eds.), *Functions of Varied Experience*, 1961, 92.

[6] N. L. Munn, *Introduction to Psychology*, 1962, 354-355.

[7] H. J. Reed, The influence of change of conditions upon the amount of recall, *J. exp. Psychol.*, 14, 1931, 641; Walter Weiss and Garry Margolius, The effect of context stimuli on learning and retention, *ibid.*, 48, 1954, 318-322.

[8] J. B. Fink and R. M. Patton, Decrement of a learned drinking response accompanying changes in several stimulus characteristics, *J. comp. physiol. Psychol.*, 46, 1953, 23-27; C. E. Noble, The effect of familiarization upon serial verbal learning, *J. exp. Psychol.*, 49, 1955, 333-338.

them to the fact that they were to participate in a psychological experiment during which they would see, in rapid succession, pictures of common objects on the screen before them, and to request that they not make comments, laugh, or take notes while the slides were being shown.

After the showing of the slides each *S* was given a sheet of paper on which he was asked to write the names—single words, common nouns—of all objects he could recall having seen. One and one-half minutes were allowed for recall.

(3) Subjects. Records were obtained in the classroom from four groups of *Ss*, members of undergraduate classes in psychology and education at Kansas State University, one group for each of the series described above. In all, 125 *Ss* participated; but to facilitate treatment of the data, records of the *Ss* were randomly excluded until the *N* for each group was equal to that of the smallest (26). The number of men and women per group was approximately equal.

Results. Although the objects were selected in part on the basis of ease of identification and uniformity of identifying label ("apple," "piano," etc.), some leeway was allowed in determining the acceptability of a response—*e.g.* either "purse" or "handbag" was acceptable. The number of items recalled by individual subjects ranged from eight to 16 out of the 20 possible, the mean being 11.31.

For every *S* a difference score was obtained by subtracting the number of duplicated items recalled from the number of varied items recalled—(ΣAB-ΣAA) *or* (ΣAB-ΣBB). These difference scores ranged from -5 to 8, their over-all mean being 1.27. Because of the particular design employed—each *S* was his own control for the variety-repetition portion—the usual measure of the major intervals was not available. The difference-scores were consequently subjected to a modified analysis of variance in which the mean difference for all groups was compared to zero, the expected mean-difference under the null hypothesis. In this analysis, variety was a highly significant source of variance—F (1,104) $= 35.6$, $p <$ 0.01— indicating the superiority in memory for this type of varied presentation over sheer repetition.

EXPERIMENT II

Method: (1) Materials. Instead of pictures, 20 different common nouns were used as stimulus-objects. Variety was achieved by having different modifiers or specifying words—*e.g.* "pine tree," "oak tree," "sunny weather," "rainy weather," etc. Four series were constituted in the manner described for Experiment I.

(2) Procedure. The directions to the *Ss* and the presentation of the slides paralleled those in the preceding experiment except that 3-sec. exposures were used with no between-slide interval other than the brief, uniform period involved in the machine's automatic operation. After all the slides had been shown, the *Ss* were asked only to write down all the *second* words, *i.e.* the nouns, that they could remember in 1½ min.

(3) Subjects. The *Ss* were members of undergraduate psychology and education classes at the University of California, Davis. As before, records were selected randomly from the largest groups until all four were equal in size (*i.e.* 23).

Results. In scoring, no deviations other than plurals were accepted. The number of words recalled by individual *Ss* varied from 5 to 14, the mean being 9.11. Difference scores (Σ varied-Σ duplicated words recalled) ranged from -5 to 6, with a mean of 0.54. When the variance was analyzed in the manner indicated above, variety again was a significant source, $F\ (1, 92) = 6.29, p < 0.05$.

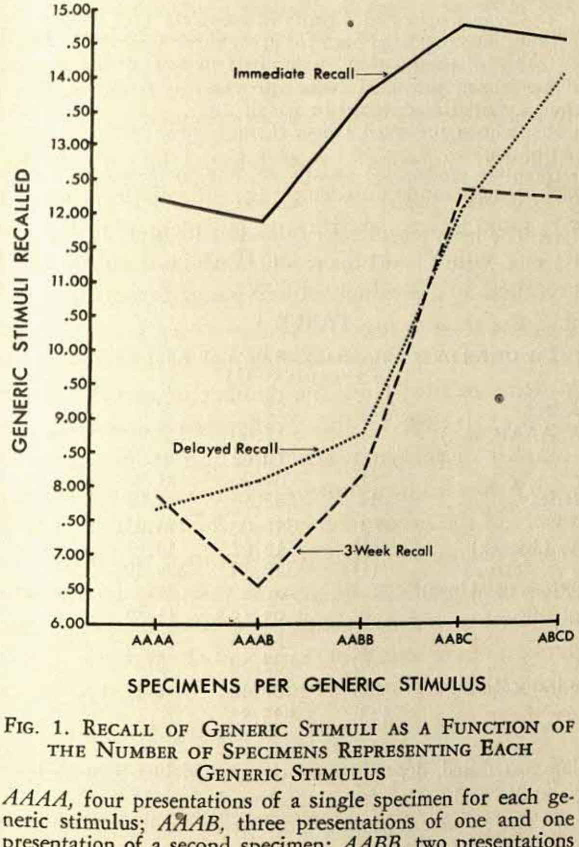

FIG. 1. RECALL OF GENERIC STIMULI AS A FUNCTION OF THE NUMBER OF SPECIMENS REPRESENTING EACH GENERIC STIMULUS

AAAA, four presentations of a single specimen for each generic stimulus; *AAAB,* three presentations of one and one presentation of a second specimen; *AABB,* two presentations of two specimens; *AABC,* two presentations of one specimen and one presentation of two other specimens; and *ABCD,* one presentation of four different specimens.

EXPERIMENT III

Because of the positive effects of variety on memory, the following experiment was performed to explore beyond the either-or question by introducing different degrees of variation.

Method: (1) Materials. The pictures used in Experiment I were supplemented by adding two more specimens (C and D) to each of the 20 categories. The slides were assembled into 5 series of 80 each as follows:

AAAA—Each *A*-specimen presented 4 times in complete, randomized sets of 20.
AAAB—Each *B*-specimen presented once for every three presentations of the *A*-specimens; order of presentation randomized in sets of 20.
AABB—Each *B*-specimen presented twice for every two presentations of the *A*-specimens. Order randomized in sets of 20.
AABC—Each *A*-specimen presented twice, *B* once, and *C* once. Order randomized in sets of 20.
ABCD—Four complete randomized sets of 20, one composed of *A*-specimens, one of *B*-specimens, etc.

(2) Procedure. The main procedural routine was the same as that in Experiment I, except that 3 min. were allowed for recall. In addition, without prior announcement, retention was tested, depending on class scheduling, two to four days after presenting the slides and again after a three-week period.

TABLE I

SUMMARY OF AN OVER-ALL ANALYSIS OF VARIANCE OF THE DATA OF EXPERIMENT III

Source	*df*	*SS*	*MS*	*F*	*P*
Series (AAAA, AAAB, etc.)	4	635.60	158.90	12.30	<.01
Linear	(1)	570.64	570.64	44.17	<.01
Non-linear	(3)	64.96	21.65	1.68	
*S*s within series	43	555.56	12.92		
Recall	2	346.06	173.03	72.07	<.01
(2–4 day) vs (3-week)	(1)	16.67	16.67	6.94	<.01
Immediate vs. delayed (both conditions combined)	(1)	329.39	329.39	137.21	<.01
Series × Recall	8	92.15	11.52	4.80	<.01
Linear	(2)	58.85	29.42	12.26	<.01
Non-linear	(6)	33.30	5.55	2.31	<.05
(*S*s within Series) × Recall	86	206.46	2.4		
Total	143	1,835.83			

(3) Subjects. The *S*s were freshmen, members of English Composition classes at Kansas State University. As before, testing was done in the classroom, separate classes being used for each of the five series outlined above. Because of the difficulty in obtaining a constant men to women ratio and in order to avoid possible variability in results due to sex, only the records of the men were used, the total *N* being 48 (9, 9, 5, 10, and 15 for the 5 groups).

Results. The *S*s' score was simply the number of generic stimuli recalled. The mean recall-scores for each of the five groups under each of the three recall conditions—immediate, two-four day delay, and three-week delay—

are shown graphically in Fig. 1. An over-all analysis of variance, as well as three one-way analyses for the three recall-conditions, was performed. The summaries of these analyses are presented in Tables I and II. These results indicate quite clearly that success in recall, both immediate and long term, is positively related to the degree of variety among the specimens representing each class of the stimulus-object.

TABLE II

SUMMARIES OF ANALYSES OF VARIANCE UNDER THREE CONDITIONS OF RECALL FOR EXPERIMENT III

	Source	*df*	*SS*	*MS*	*F*	*P*
Immediate Recall	Between Groups	4	73.03	18.26	4.77	<.01
	Linear	(1)	65.70	65.70	17.18	<.01
	Non-linear	(3)	7.33	2.44	—	
	Within Groups	43	164.45	3.82		
	Total	47	237.48			
2-4 day Recall	Between Groups	4	356.99	89.25	11.98	<.01
	Linear	(1)	310.34	310.34	41.66	<.01
	Non-linear	(3)	46.65	15.55	2.09	
	Within Groups	43	320.32	7.45		
	Total	47	677.31			
3-week Recall	Between Groups	4	297.73	74.43	11.54	<.01
	Linear	(1)	239.31	239.31	37.12	<.01
	Non-linear	(3)	58.42	19.47	3.02	<.05
	Within Groups	43	277.24	6.45		
	Total	47	574.97			

DISCUSSION

The data from each of the experiments indicate that memory for generic stimuli may be enhanced by varying, in successive presentations, the specimens representing them. *S* is, for example, more likely to recall "shoe" if he is shown one picture of a woman's ballet shoe and another of a man's oxford, than if he is shown either twice. Similarly, the likelihood of recalling the word "weather" is greater if shown the words "sunny weather" and "rainy weather" than if shown either pair two times. These results may be contrasted with the disruptive effects generally reported in experiments where variation is introduced at the time of recall rather than during training.[9] The two sets of findings are, of course, not necessarily contradictory, since the effect of any change would undoubtedly be a function both of its nature and of the time at which it is introduced. As in the case of the formally similar transfer problem, determining the types of situations—here the dimensions to be varied—which have generally positive, zero, or negative effects on retention is a further empirical problem.

One might also expect, as the results of Experiment III demonstrate, different effects from different degrees of variation among the specimens used. Increasing the degree of variation here proved to be essentially facilitating: Fig. 1 depicts the improvement in recall with diversity; the analysis indicates the significance of this relationship—F_{series} (4, 43) = 12.30, $p < 0.01$. Although Fig. 1 suggests the relationship between specimen variety and success in recall to be monotonic for the range examined, this cannot be confidently asserted since the values on the abscissa are not known to constitute an interval-scale. Nor would such a relationship be expected to hold indefinitely as degree of variety became greater. A possible depressing effect from a small degree of variation, manifest in the data of both immediate and three-week recall, is suggested by the slightly lower recall-scores with the introduction of one degree of variation in four repetitions (*AAAA* to *AAAB*). If this minimum initial drop in an otherwise increasing function is a real effect. it is not at all novel, having been observed in several other relationships in psychology.[10]

A further question concerns the manner in which the effects of varied repetition on memory operate over time. Since the same *S*s were used under each recall-condition, there is some contamination in the results from the delayed situations. Since, however, the contamination would tend to work against the effect, by minimizing temporal differences, it is, for our present purposes, of little consequence. As indicated in Fig. 1 and validated by the analysis, several temporal effects are observed in the data. First, there is the usual loss with the passage of time, immediate recall being considerably superior to the other two conditions—F (1,86) = 137.21, $p < 0.01$. Secondly, the greatest loss, as in traditional curves of forgetting, occurs early. The difference between recall under the delayed conditions, although significant, is less marked than that between the immediate and delayed—F of 6.94 vs. 137.21. Thirdly, and most important, the primary effect becomes more pronounced with time, the greatest loss over time occurring with the less varied presentations: $F_{\text{interaction}}$ (8, 86) $= 4.80, p < 0.01$.

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[9] Reed, *op. cit.*, 641; Weiss and Margolius, *op. cit.*, 318-322.

[10] *E.g.* L. P. Crespi, Quantitative variation of incentive and performance in the white rat, this JOURNAL, 55, 1942, 487-490; W. F. Dukes and William Bevan, Jr., Size estimation and monetary value: A correlation, *J. Psychol.*, 34, 1952, 43-53.

In attempting to explain these over-all findings—the positive effect of variety on memory for generic stimuli—one might equally well invoke an associationistic or a motivational hypothesis. By the former, the increased number of associations with *x* (*a*, *b*, etc.) would enhance its probability of recall because recalling any one of the several items may lead to its recall as opposed to the limited condition of the single associate. The motivational type of hypothesis would, on the other hand, make the advantage stem from heightened interest or attention as the aspects vary. The two, of course, are not mutually exclusive. The degree to which each may operate in producing this effect might be determined, or at least explored, in further empirical investigations.

SUMMARY

Three experiments involving a total of 251 *S*s, examined the relationship between the recall of generic stimuli and the number of individual specimens used to represent such stimuli. The first two compared immediate recall after two different specimens had been presented with recall after the repeated presentation of a single specimen. In the first experiment the stimuli were colored photographs of common objects; in the second, common nouns with frequently associated adjectives. In both instances, more generic stimuli were named when two different specimens represented the stimulus than when a single specimen was repeated. The third experiment was a more elaborate one in which each generic stimulus was represented by from one to four specimens. Success in recall was positively related to the number of specimens presented. Furthermore, this difference in performance was enhanced with the passage of time. Memory loss after two to four days and after three weeks was greatest for the condition involving the repetition of the single specimen and decreased as the number of specimens used was increased.

A PSYCHOPHYSICAL INVESTIGATION OF TRIANGULAR FORM

By DONALD W. STILSON, University of Colorado School of Medicine

Until recently psychophysical methods have rarely been applied systematically to the investigation of the visual perception of form.[1] One reason for this is that "form quality" is sometimes held to be an emergent property and not a simple function of the physical parts of stimuli.[2] Hebb, however, has suggested that, although emergent form-properties may exist for single figures, perceived differences between shapes may nonetheless depend upon differences between their parts.[3] Since psychological scales are usually based on apparent similarities and differences among stimuli, it appears that psychophysical methods can be applied to form-perception without denying the existence or importance of emergent properties for individual stimuli. As a number of recent studies have been shown, relatively simple functions of the physical parts of shapes permit the prediction of some of their psychological characteristics. In previous psychophysical studies of form, however, the relationship between physical characteristics and subjective similarity (usually with respect to one designated dimension) has nearly always been examined with the stimuli in a fixed orientation (rotational position) throughout the experimental procedure.[4] In the present study, Attneave and Arnoult's descripion of form as a "somewhat vague set of properties which are invariant under transformations of color and brightness, size, place and orientation" was adopted.[5] Then, from a few physical characteristics, the predictability of perceived dissimilarity

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[1] J. J. Gibson, What is form?, *Psychol. Rev.*, 58, 1951, 403-412; R. B. Sleight and Grace Duvoisin, An annotated bibliography of form perception, J.H.U. Report No. 166-1-153, Office of Naval Research, Washington, D.C., 1952, v; K. M. Michels and Leonard Zusne, Metrics of visual form, *Psychol. Bull.*, 63, 1965, 74-86.

[2] F. H. Allport, *Theories of Perception and the Concept of Structure*, 1955, 92; E. G. Boring, *A History of Experimental Psychology*, 1950, 442-443.

[3] D. O. Hebb, *The Organization of Behavior*, 1949, 17-18, 26-28, 30.

[4] Fred Attneave, Dimension of similarity, this JOURNAL, 63, 1950, 516-556; Physical determinants of the judged complexity of shapes, *J. exp. Psychol.*, 53, 1957, 221-227; J. E. Hochberg and Virginia Brooks, The psychophysics of form: Reversible perspective drawings of spatial objects, this JOURNAL, 73, 1960, 337-354.

[5] Fred Attneave and M. D. Arnoult, The quantitative study of shape and pattern perception, *Psychol. Bull.*, 53, 1956, 452-471.

of triangles was estimated independent of color and brightness, size, and orientation. Due to the attempt to eliminate or to control these variables, perhaps the study can be described as an investigation of 'pure' form. Although no effort has been made to eliminate the effects of place on perceived dissimilarity, it seems unlikely that this variable would have much effect, provided direct fixation on the stimulus is permitted.

METHOD AND PROCEDURE

Stimuli. The stimuli consist of two systematic samples of triangles, none with an interior angle larger than 126.67° or smaller than 14.67°. The 13 triangles, of each sample are shown in Fig. 1 and 2. Each was formed of black construction paper with an area of 1 sq. in. mounted on a white octagonal card 6½ in. in diameter. The orientation of each triangle on the card was determined by a random rotation. In an attempt to provide common anchoring points, Triangles 12 and 13 were included in both samples.

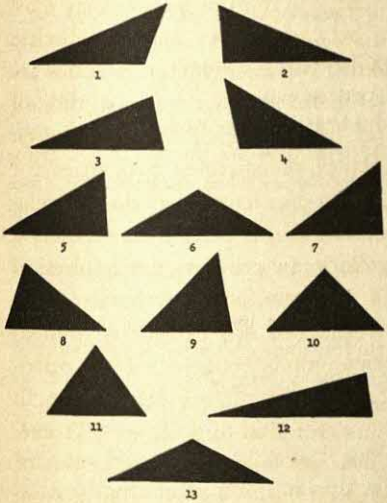

FIG. 1. THE TRIANGLES OF SAMPLE 1

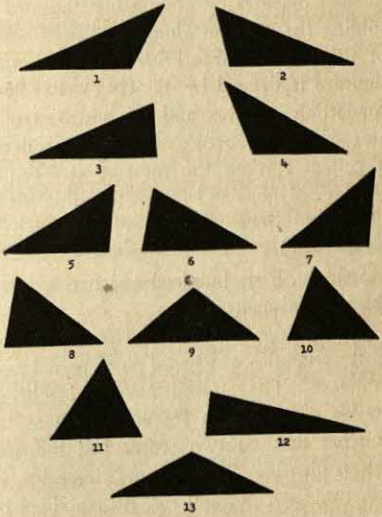

FIG. 2. THE TRIANGLES OF SAMPLE 2

Physical measures of the stimuli. All physical measures were so defined as to be independent of area and applicable to polygons of any number of sides. The first is the ratio of perimeter squared to area, P^2/A. Attneave and Arnoult describe this variable as a size-independent measure of dispersion.[6]

The second physical measure is Variation of Sides. It is found by evaluating

$$\frac{2}{N(N-1)\sqrt{A}} \sum_{i=1}^{N} \sum_{j=1}^{N} |S_i - S_j|,$$

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[6] *Ibid.,* 468.

where A is the area, N is the number of vertices, and S_i and S_j are the lengths of sides i and j, respectively. This measure is similar to the third, Variation of Angles, which is defined by

$$\frac{2}{N(N-1)} \sum_{i=1}^{N} \sum_{j=1}^{N} |a_i - a_j|,$$

where a_i and a_j are the interior angles at vertices i and j, respectively. This is similar to a measure described by Attneave and said to reflect something akin to "good continuation."[7] Both this and the immediately preceding measure are zero for regular polygons and increase with irregularity. The factor $2/(N(N-1))$ was omitted in the present study, since $N = 3$ for all stimuli.

The fourth measure, Sum of Overlaps, is obtained for polygons with an even number of sides by finding first the area of contact of the 'halves' that result from folding the polygon along the line connecting a pair of opposite vertices.[8] The sum of these areas for all $N/2$ pairs of opposite vertices can be called T, and the measure is defined by $4T/(NA)$. For N odd, the polygon is folded along the line connecting a vertex and the midpoint of the side opposite to it, and the resulting area of contact between "halves" is determined. If the sum of all N such areas is called T, then the measure is $2T/(NA)$. This measure always equals 1 for regular polygons and is less than this for irregular shapes. Thus, it appears to reflect symmetry. Since all figures were triangles, the factor $2/N$ was omitted in the present study.

Subjects. Two hundred and twenty students in introductory psychology served in these experiments.

Paired associate words. The method of assessing the psychological dissimilarity of the triangles was based on a paired-associate learning task similar to that used by Attneave.[9] The stimuli associated with the triangles were three-letter words so chosen as to have a frequency of 5 to 10 per million words.[10] They were also so selected that no two words had the same first letter or both second and third letters identical. The 15 words were *bin, cod, hap, jut, ken, log, mug, nip, pew, sup, tar, vat, wan, git,* and *rot.* As explained below, the last two were never paired with any triangle.

Experimental procedure. The triangles of Sample 1 were presented to 118 *Ss* and those of Sample 2 to 102 *Ss*. Fifteen sessions were required with 11 to 19 *Ss* serving per session.

Since it was possible that words would be confused as well as triangles, a different word was associated with a given triangle for each session, except that that assignment of words to triangles was the same for the two smallest groups.

The triangles were projected on a 6 × 8-ft. beaded glass screen located 22.5 ft. from an opaque projector (American Optical Company Delineascope, Model V). The ratio of linear distances on the stimulus-cards to those on the screen was 1 to 14. Each stimulus was projected in a black circular frame 4 in. in diameter. Since form is considered to be independent of orientation, a given stimulus appeared in 1 of 8 different randomly chosen positions at each presentation.

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[7] Attneave, *op. cit.*, 1957, 223.

[8] Two sides (or a side and a vertex) are said to be opposite one another if they are separated by the same number of vertices when counting in the clockwise and counter-clockwise directions.

[9] Attneave, *op. cit.*, 1950, 544-545.

[10] E. L. Thorndike and Irving Lorge, *The Teacher's Work Book of 30,000 Words*, 1944.

Instructions. Before beginning paired-associate training, the following instructions were read to the *S*s.

> I am going to test your ability to remember. To do this, I will project a number of triangles on the screen one-at-a-time. Each time you see the same triangle, it will be in a different position. No two triangles are the same. Every time you see a triangle, I will call out and spell a short word. You will see each triangle five times, and each time you see it I will call out the same word. Now, here is what I wish you to do: Try to learn the word that goes with each triangle. After you have seen each triangle five times, I will show them to you once more and ask you to tell me the word that goes with each one. Do you understand?

Presentation order of the triangles was random on the first trial, the second trial was an 'inside-out' ordering the first, the third trial an 'inside-out' ordering of the second, etc. Each stimulus was exposed for 10 sec., and while it was on the screen, *E* called out the paired-associate word, spelled the word, and called it out again. Approximately 5 sec. elapsed between successive presentations.

Following the completion of the first trial, *E* announced: "You have now seen each triangle once and heard each word once." Upon completion of the fifth trial, *E* distributed paper on which the 15 words appeared along with space for writing the word recalled by *S* for each triangle. The following instructions were then read:

> Now I will show you the triangles once more, but this time I will not tell you the word associated with each one. You are to write down the correct word for each triangle on the piece of paper. If you have trouble remembering the correct word, look over the list and guess. Your first guess has a good chance of being correct. Be sure that you do not leave any of the spaces blank and that you use each word no more than once. Are you all ready?

The stimuli were then presented in an inside-out ordering of the Trial 5. The ordinal number of each triangle was called out instead of the word associated with each. Otherwise, the procedure was identical to that of the training trials.

RESULTS

*S*s who responded on the test-trial with either of the words that were never paired with a triangle during training (*git* or *rot*) were considered less cooperative, less motivated, or less able than the remaining *S*s.[11] They were dropped from further consideration. This reduced the first group of *S*s from 118 to 76 and the second group from 102 to 72.

If p(i,j) is the proportion of *S*s who gave the paired-associate word attached to Stimulus i during training in response to Stimulus j on the test trial, then $S(i, j) = [p(i, j)\ p(j, i)/[p\ i, i)\ p(j, i)]]^{1/2}$ has been suggested by Shepard as a measure of generalization between Stimuli i and j.[12] Shepard, furthermore, has adduced evidence indicating that stimulus-generalization is a negative exponential function of psychological distance. If this is correct, then $-\log_e S(i, j)$ is a measure of the psychological distance between Stimuli i and j. This measure of psychological dissimilarity was calculated for each pair of triangles within each of the two samples.

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[11] Those who did respond with either of these words had a significantly lower mean number of correct responses than the other *S*s (3.84 compared to 4.92 in the first group, and 4.07 vs. 5.08 in the second).

In addition, the absolute value of the difference between each pair of triangles within each sample was found for each of the four physical measures, and the multiple linear regression of these differences on psychological dissimilarity was examined for each sample separately. The linear regression equation of Sample 1 predicts 53.4% of the variance in psychological dissimilarity, and in Sample 2 the corresponding percentage is 42.5%. The corrrelation of each of the physical measures with dissimilarity and the unstandardized regression coefficients for predicting dissimilarity as a linear function of these measures are shown in Table 1. Using P^2/A alone, 48.3% of the variance of dissimilarity can be predicted in Sample 1, and the corresponding figure is 38.7% for Sample 2. Similarly, Variations of Sides alone permits the prediction of 44.5% of the variance of perceived dissimilarity of Sample 1 and 37.5% for Sample 2. The remaining two physical measures contribute relatively little to predictability.

TABLE I

SUMMARY OF THE LINEAR REGRESSION OF DIFFERENCES IN THE PHYSICAL MEASURES ON PSYCHOLOGICAL DISSIMILARITY

	Sample	P^2/A	Variation of sides	Variation of angles	Sum of overlap	Constant
Correlation with psychological dissimilarity	1	.695	.667	.239	.318	—
	2	.622	.612	.263	.279	—
Unstandardized regression coefficients	1	1.150	.677	−.006	.179	1.253
	2	.869	.560	.003	.159	1.561

DISCUSSION

Although it is unlikely that confusion among words affected the psychological dissimilarity much, interaction between word and triangle may have influenced the likelihood of confusion in some instances.[13] For example, the likelihood of confusions was probably reduced for those *Ss* for whom "jut" was paired with Triangle 12. Rotation of assignments of paired-associates from one experimental session to another presumably, however, eliminated large, systematic effects of this kind.

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[12] R. N. Shepard, Stimulus and response generalization: A stochastic model relating generalization to distance in psychological space, *Psychometrika*, 22, 1957, 325-345; Stimulus and response generalization: Deduction of the generalization gradient from a trace model, *Psychol. Rev.*, 65, 1958, 242-246.

[13] D. W. Stilson, A psychophysical investigation of triangular shape, Unpublished Doctoral dissertation, University of Illinois, 1956, 54-58.

It is of special interest that differences in P^2/A predict perceived dissimilarity of triangles as well as any other single measure, since perimeter has often been found to be related to illumination thresholds for form recognition.[14] Though Casperson found little relationship between recognition thresholds and perimeter for isosceles triangles, a stronger relationship probably would have appeared if scalene triangles had been included so as to increase the variability of perimeter.[15] Thus, it appears that compactness-dispersion as measured by area-independent functions of perimeter is related to supra-threshold similarity as well as to the recognitive threshold. Since the correlation of Variation of Sides and P^2/A is 0.797 in Sample 1 and 0.807 in Sample 2, it appears that differences on these variables reflect similar characteristics. Although Variation of Angles appears to be quite similar to Variation of Sides, the correlations between their differences were only 0.495 and 0.435 for Samples 1 and 2 respectively. Scatterplots, however, revealed that the low correlation of Sum of Overlaps and Variation of Angles with dissimilarity was partly attributable to the great subjective separation of Triangle 12 from the others. When Triangle 12 was deleted, the correlations of dissimilarity with Sum of Overlaps and with Variation of Angles increased. On the other hand, the restriction of range resulting from the deletion of Triangle 12 decreased the correlation of dissimilarity with P^2/A and Variation of Sides to about 0.4 to 0.6. Since P^2/A and Variation of Sides more accurately reflected the extreme subjective position of Triangle 12, they appear more sensitive than the other two measures to differences that are relevant to subjective dissimilarity.

Since variation of Sides and P^2/A both depend on the lengths of sides, these results appear to necessitate a revision of Michels and Zusne's statement that measures on sides have proved useless or at least less effective than measures of angles as predictors of psychological properties of planar forms.[16] It is important, however, to keep in mind that in the studies they reviewed, orientation of the stimuli was held constant, whereas in the present investigation, orientation was varied randomly. As Attneave has shown, the relative position of triangles affects subjective judgments of their similarity, and if the orientation of the figures had remained constant in the present research, the pattern of relationships between physical measures and perceived dissimilarity might have been quite different.[17] For example, if each triangle always appeared with a particular side horizontal, then Variation of Angles might have a stronger relationship to psychological similarity.

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[14] For example, see E. S. Lamar, Selig Hecht, C. D. Hendley, and Simon Shlaer, Size, shape, and contract in detection of targets by daylight vision: II. Frequency of seeing and the quantum theory of cone vision. *J. opt. Soc. Amer.*, 38, 1948, 741-755; J. E. Hochberg, Henry Gleitman, and F. D. MacBride, Visual threshold as a function of simplicity of form, *Amer. Psychologist.*, 3, 1948, 341-342 (Abstract); M. E. Bitterman, John Krauskopf, and J. E. Hochberg, Threshold for visual form: A diffusion model, this JOURNAL, 67, 1954, 205-219; John Krauskopf, R. A. Duryea, and M. E. Bitterman, Threshold for visual form: Further experiments, this JOURNAL, 67, 1954, 427-440.

[15] E. C. Casperson, The visual discrimination of geometric form, *J. exp. Psychol.*, 40, 1950, 668-681.

Limiting the stimuli to triangles restricts the range of forms and makes it impossible to use some physical measures that have demonstrated utility for predicting subjective characteristics of two-dimensional shapes.[18] This restriction has probably limited the degree of predictability. Further reduction in predictability may have resulted from individual differences in perceived dissimilarity and physical variations among the stimuli not reflected in the particular physical measures chosen.[19]

SUMMARY

On the basis of confusions occurring on a test-trial in a paired-associate learning task, perceived dissimilarity of pairs of triangles was determined. The procedure was such that measured dissimilarity of the triangles was independent of their relative orientation. Four physical measures were so defined that they are independent of area and applicable to polygons with any number of sides. The extent to which a linear function of the absolute differences between triangles on these four measures permitted the prediction of psychological dissimilarity was determined. For one sample of triangles, 53.4% of the variance in psychological dissimilarity was predictable and 42.5% was predictable in a second sample. The ratio of perimeter squared to area by itself predicted only 4% to 5% less variance in perceived dissimilarity than a linear function of all four physical measures.

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[16] Michels and Zusne, *op. cit.*, 79.

[17] Attneave, *op. cit.*, 1950, 542.

[18] For example, "number of turns" (Attneave, *op. cit.*, 1957, 224).

[19] For example, difference in symbolic meaning are mentioned by Gibson, *op. cit.*, 1951, 410.

PERCEPTION OF ORIENTATION: ADAPTATION TO LATERAL BODY-TILT

By JOSEPH H. MCFARLAND, Antioch College, and
FRANK CLARKSON, Astor Home for Children

It is well known that proprioceptive and exteroceptive stimulation affect perception of the visual vertical.[1] It is also well documented, but not so well known, that certain proprioceptive and exteroceptive stimuli affect not only an *O*'s perception of the visual vertical but also the orientation of his longitudinal axis. For example, a change in vestibular stimulation by tilting *O*'s body laterally or rotating it around the longitudinal axis leads to displacements of both the apparent vertical position and the apparent longitudinal axis from their respective positions.[2] Similarly, a change in visual stimulation, by the lateral tilt of a 'frame,' leads to similar displacements in these perceptions.[3]

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* Received for publication July 19, 1965. This study, undertaken at Clark University, was supported by USPHS Research Grant MH-00348 from the National Institute of Mental Health.

[1] Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 218; J. J. Gibson and O. H. Mowrer, Determinants of the perceived vertical and horizontal, *Psychol. Rev.*, 45, 1938, 300-323; H. A. Witkin and S. E. Asch, Studies in space orientation: IV. Further experiments on perception of the upright with displaced visual fields, *J. exp. Psychol.*, 38, 1948, 762-782; C. W. Mann, N. H. B. Berry, and H. J. Dauterive, The perception of the vertical. I. Visual and non-labyrinthine cues, *J. exp. Psychol.*, 39, 1949, 538-547; Gibson, The relation between visual and postural determinants of the phenomenal vertical, *Psychol. Rev.*, 59, 1952, 370-375; Heinz Werner and Seymour Wapner, Sensory-tonic field theory of perception: Basic concepts and experiments, *Rev. Psicol.*, 50, 1956, 315-337; K. A. Chandler, The effect of monaural and binaural tones of different intensities on the visual perception of verticality, this JOURNAL, 74, 1961, 260-265; Brant Clark and Ashton Graybiel, Perception of the postural vertical in normals and subjects with labyrinthine defects, *J. exp. Psychol.*, 65, 1963, 490-494.

[2] H. E. Burtt, The perception of slight changes of equilibrium, with especial reference to problems of aviation, *J. appl. Psychol.*, 2, 1918, 101-115; P. M. van Wulfften Palthe, Function of the deeper sensibility and of the vestibular organs in flying, *Acta Otolaryng.*, 4, 1922, 415-449; L. H. Bauer, Dangers of high speed and acrobatic flying, *Aviation*, 16, 1924, 174-176; F. Leiri, Über die Bedeutung des Vestibularapparats bei der aviation, *Z. Hals-Nasen-, und Ohrenh-lk.*, 17, 1927, 381-391; A. Gemelli, L'orientazione lontana vel volo in aeroplane, *Riv. Psicol.*, 29, 1933, 297-325; Witkin, Perception of body position and of the position of the visual field, *Psychol. Monogr.*, 63, 1949, (No. 302), 1-46; Perception of the upright when the direction of the force acting on the body is changed, *J. exp. Psychol.*, 40, 1950, 93-106; J. H. McFarland, Seymour Wapner, and Heinz Werner, Relation between perceived location of objects and perceived location of one's own body, *Percept. mot. Skills*, 15, 1962, 331-341.

[3] Witkin, *op. cit.*, 1949, 1-46; Mc Farland, Visual and proprioceptive changes during visual exposure to a tilted line, *Percept. mot. Skills*, 15, 1962, 322.

The latter findings suggest that exteroceptive and proprioceptive stimulation influences a referent system which underlies perception of the orientation of both visual objects and the longitudinal axis. While the neurophysiology of such a referent system is speculative, it is clear from studies of infra-human species that convergence of many afferent sources do take place within the cortex, the reticular formation, and the cerebellum.[4] Although the functional significance of such convergence is not obvious from these studies, at least the minimal fact of convergence has been shown between exteroceptive and proprioceptive signals in infra-human species, thereby providing some neurophysiological plausibility to the system hypothesized.

Recently, adaptative effects following visual exposure to a tilted line have been interpreted as due to a change in this hypothetical system; there are adaptative effects as measured by the *O*'s visual apparent vertical, the Gibson effect, and there are also adaptative effects as measured by *O*'s apparent longitudinal body-axis.[5] The purpose of the present study is to show that adaptative effects will occur with exposure to a tilted posture; there will be adaptative effects as measured by *O*'s visual apparent vertical and his apparent longitudinal axis.

Method. There are three methods of studying the adaptative effects: *(1)* an indirect method, by changing typical constant proprioceptive or visual stimulation and thereby changing typical relations between proprioceptive and visual stimulation; and *(2)* and *(3)*, directly, by changing typical relations between respectively constant or changing proprioceptive and visual stimulation.[6]

The first method was employed, viz. *O* laterally tilted in a dark room by a constant amount. The effect of this condition on the perceived orientation of proprioceptive stimuli defining the *O*'s longitudinal body-axis was determined by measuring apparent body-position (*ABP*), and the effect of this condition on the perceived orientation of visual stimuli was inferred from measures of apparent vertical position (*AVP*).[7]

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[4] For brief reviews, see C. K. Levy, J. D. Loeser, and W. P. Koella, The cerebellar acoustic response and its interaction with optic responses, *Electroenceph. clin. Neurophysiol.*, 13, 1961, 235-242; H. H. Kornhuber and J. S. Da Fonseca, Optovestibular integration in the cat's cortex: A study of sensory convergence on cortical neurons, in Morris Bender (ed.), *The Oculomotor System*, 1964, 239-277.

[5] Mc Farland, *op. cit.*, 1962, 322.

[6] Examples of these three methods are: (1) introduction of constant atypical proprioceptive stimulation only, *e.g.*, by laterally tilting *O* a constant amount in a dark room, or by introducing constant atypical visual stimulation only, which is not possible; (2) introduction of constant atypical relations between constant proprioceptive and visual stimulation, *e.g.* by laterally tilting *O* a constant amount in a lit room with a visual scene composed of vertical contours, or by keeping *O* erect in a lit room with a visual scene composed of contours tilted a constant amount; and (3) introducing a constant atypical relation between changing proprioceptive and visual stimulation, *e.g.* by having *O* wear a brace which tilts him a constant amount while walking about a typical scene, which is not possible, or by having *O* wear a prism which tilts the visual field a constant amount while he is walking about a typical visual scene.

Procedure. To obtain the measures, *E* instructed *O* so to rotate a luminous line that it appeared parallel with his longitudinal body-axis, or so to place it that it appeared vertical. The position of the line was read from a protractor to the nearest 0.5°. For each measure, there were two trials, starting positions for the line of 30° left and right of, respectively, physical longitudinal body-axis and physical vertical. Both measures were obtained by means of the visual index before, during, and after 52.5′ of lateral body-tilt. During body-tilt, measures were obtained immediately after tilt and every 7.5 min. thereafter. Thus, for each *O* there were 2 trails for each of 2 measures on 10 occasions (Fig. 1).

These measures were obtained with lateral body-tilts of 20° left, 30° left and right, and 40° left. Three independent groups, each composed of eight men and eight women, were used for each degree of tilt. For all groups, *AVP* and *ABP* were measured on different days; and the order of measuring was counter-balanced in a 2 × 2 Latin square. The group receiving 30° left and right tilt had a counter-balanced order of presenting left and right directions of tilt. Within groups there were equal numbers of each sex who received a left-right and right-left sequence of positions; this sequence remained constant for each *O* throughout the experiments.

On each test-day, *O*, wearing light-tight goggles, was led into the experimental room and seated in an unpadded chair which could be tilted in appropriate lateral directions and maintained at the appropriate degree of tilt. The chair had foot- and arm-rests; and *O*'s head could be clamped parallel with his body. He was instructed to remain stationary in the chair at all times and was observed continuously by *E*. At no time was he permitted to view anything within the experimental room except the test-stimulus during a trial.

Results. Before tilt, *ABP* and *AVP* both approximated the direction of gravity ($P < 0.05$) (Fig. 1 A). Immediately after tilt, *ABP* and *AVP* deviated from respective physical positions in a direction dependent on magnitude and direction of body-tilt ($P < 0.001$) in agreement with the findings of previous studies.[8] With prolonged body-tilt, *ABP* approached physical body-position for all magnitudes and directions of tilt ($P < 0.001$). The direction of change in *AVP*, however, varied as a function of magnitude of the tilt ($P < 0.001$). After 20° and 30° tilt, *AVP* moved in the direction of body-tilt; after 40° tilt, *AVP* moved opposite the direction of body-tilt.[9]

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[7] Numerous studies of perceived body orientation use a measure of the postural vertical, i.e. *O* adjusts himself from a tilted to an upright position. Such a measure is equivalent to *O*'s adjusting a visual line from a tilted to a vertical position to measure the visual vertical. Employing such a measure provides a means of showing the effect of starting position or after-effect of body-tilt on the perception of the postural vertical. Such a measure is not suitable for the present study which requires a measure of perceived body-orientation while *O* is tilted.

[8] Mc Farland, Wapner, and Werner, *op. cit.*, 1962, 331-341; Martin Bauermeister, Effect of body tilt on apparent verticality, apparent body position and their relation, *J. exp. Psychol.*, 67, 1964, 142-147.

The effect of prolonged body-tilt on *ABP* and *AVP* can best be seen by a consideration of the angular difference between *ABP* and *AVP* before, during, and after body-tilt (Fig. 1B). Before tilt, when the body was in a typically erect position, there was only 0.5° difference. Initially after tilt, there was a maximal deviation which varied with magnitude of body-tilt ($P < 0.001$); as body-tilt continued, the difference diminished at a rate dependent on magnitude of body-tilt (the *F* for the interaction between

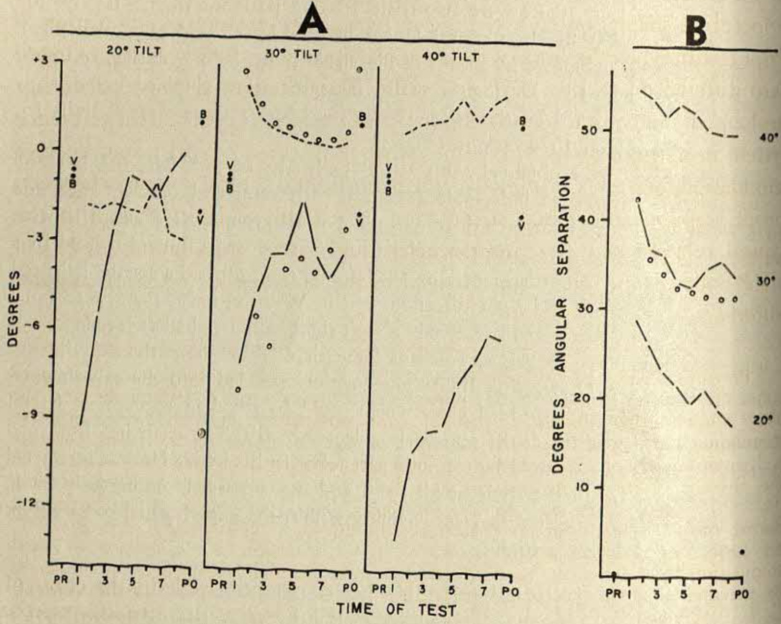

FIG. 1A. PLOT OF APPARENT VERTICAL POSITION AND APPARENT BODY POSITION
AVP, dashed line; *ABP*, solid line; *PR*, pre-tilt; *PO*, post-tilt; 30° right, unfilled circles. A minus (—) number indicates the deviation from the physical vertical or body position in the direction of body-tilt; a plus (+) number, the deviation opposite the direction of body-tilt.

FIG. 1B. REPLOT IN TERMS OF ANGULAR SEPARATION
Unfilled circles at 30° represent the results when tilt was to the right.

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[9] The results for 30° tilt have been reported previously: J. H. Mc Farland, Frank E. Clarkson, and Seymour Wapner, Effect of prolonged body tilt on the relationship between perceived position of one's body and of other objects, *Amer. Psychologist*, 16, 1961, 421.

magnitude of body-tilt and measures is $P < 0.001$). When the body was erect again, there was a deviation of 3° for all magnitudes of body-tilt, which differed from the 0.5° deviation before tilt ($P < 0.001$).

Discussion. These effects of prolonged and constant lateral body-tilt, in the absence of a visual scene, were taken as a form of visual and proprioceptive adaptation to a change in typical proprioceptive stimulation.[10] Prior to tilt, when the body was in a typical position, there was approximate zero difference between *ABP* and *AVP*. The stimulation provided during prolonged and constant lateral body-tilt affected the hypothesized reference system in a manner which redefines what constituted typical proprioceptive stimulation, and thus, indirectly, what constituted typical relations between proprioceptive and visual stimulation. With this indirect change in the typical relation between proprioceptive and visual stimulation, *ABP* and *AVP* both change, and they changed in the direction of reducing angular difference.

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[10] Previous studies using Methods 1, 2, and 3 of studying adaptation have used either measures of visual stimuli *or* measures of proprioceptive stimuli defining the body. Studies using Method 1, that is, a constant change in typical proprioceptive stimulation only, have employed measures of the visual horizontal during constant body-rotation (6° or 12° deviation of resultant force from gravitational vertical) in the absence of visual stimulation and have shown no adaptation-effects during 240 min.: Brant Clark and Ashton Graybiel, Visual perception of the horizontal during prolonged exposure to radial acceleration on a centrifuge, *J. exp. Psychol.*, 63, 1962, 294-301. Constant body-tilts of 60 sec. duration in the absence of visual stimulation have, however, been reported to result in adaptation-effects as measured by perception of the postural vertical, that is, *O* adjusts himself to the vertical: G. E. Passey and F. E. Guedry, The perception of the vertical: II. Adaptation effects in four planes, *J. exp. Psychol.*, 39, 1949, 700-707; see also R. G. Pearson and G. T. Hauty, Adaptive processes determining proprioceptive perception of verticality, *ibid.*, 57, 1959, 367-371. In regard to Method 2, a constant change in the typical relation between constant visual and proprioceptive stimulation, studies using this method and measures of the visual vertical show adaptation-effects after erect *Os* are exposed to tilted visual stimuli: Gibson, Adaptation, after-effect, and contrast in the perception of tilted lines: II. Simultaneous contrast and the areal restriction of the after-effect, *J. exp. Psychol.*, 20, 1937, 553-569; R. B. Morant and H. K. Beller, Adaptation to prismatically rotated visual fields, *Science*, 148, 1965, 530-531. One study employing this procedure and measures of both *O*'s perceived body position and visual vertical, as indexed by a visual marker, has also reported adaptation effects for both measures (Mc Farland, *op. cit.*, 1962, 322). Method 3 has been used in numerous studies which show adaptation-effects as measured by perception of visual stimuli; reviews can be found in K. U. Smith and W. M. Smith, *Perception and Motion*, 1962, 59-104; Sonoko Ohwaki, An investigation of figural adaptation: A study within the framework of sensory-tonic field-theory, this Journal, 74, 1961, 3-16; J. G. Taylor, *The Behavioral Basis of Perception*, 1962, 163-188, 298-337; Harutune Mikaelian and Richard Held, Two types of adaptation to an optically-rotated visual field, this Journal, 77, 1964, 257-263; C. S. Harris, Perceptual adaptation to inverted reversed, and displaced vision, *Psychol. Rev.*, 72, 1965, 419-444. For a review of the studies measuring proprioceptive adaptation while using Method 3, see Harris, *op. cit.*, 1965, 419-444.

Further support for this interpretation was provided by the fact that after prolonged tilt, when the body was returned to the previously typical, erect position, there was now, relative to before tilt, a significant difference between perceived orientation of the longitudinal body-axis and the visual vertical. This was to be expected if, indeed, the typical proprioceptive stimulation was now provided by the tilted posture as positioning the body to a physically vertical posture should be equivalent to the effect of initial tilt, viz. lead to an angular difference between *ABP* and *AVP*.

It must be emphasized that the perceptual adaptation reported here is not fundamentally different from that reported in numerous recent studies.[11] Although the conditions used in the present experiment differ, at first glance, from those used in these recent studies, which are, respectively, comparable to Methods 2 and 3, outlined above, *i.e.* they directly change typical relations between constant and changing proprioceptive and visual stimulations; the present study employed Method 1, which indirectly changes typical relations between proprioceptive and visual stimulations. Thus, the only difference in conditions related to whether relations between proprioceptive and visual stimuli are directly or indirectly changed.

It could be objected, however, that both Method 1 and 2 differ fundamentally from Method 3 in that the latter introduces reafferent stimulation, whereas the former methods do not.[12] To meet this objection, it can be argued that all three procedures introduce reafferent stimulation during the adaptative period. Method 1 introduces reafferent stimulation that is consequent to the tilted posture maintained by *O* with the aid of the tilted chair in which he is sitting. In Method 2, the reafferent stimulation is consequent to the self-produced erect posture maintained by *O*, or to the tilted posture maintained by *O* with the aid of the tilted chair in which he is sitting. With Method 3, the reafferent stimulation is consequent to the self-produced walking about in the environment.

To summarize: As long as there is an intact organism, it may be argued that there is always reafference consequent to either a static posture which is self-maintained or consequent to a change in posture which is self-produced. Such reafference constitutes a part of what is here grossly referred to as proprioceptive stimulation.

Viewing these recent experiments in terms of the hypothesized referent system may help resolve some of the dispute as to whether exposure to atypical stimulation leads to visual or proprioceptive adaptation, that is, change in apparent position of visual stimuli or change in felt position of body or body-parts.[13] For example, the wearing of prisms while walking about exposes *O* to an atypical but constant relation between changing visual and proprioceptive stimulation. Exposure to such atypical stimulation leads to changes in the reference-system defining typical relations between visual stimulation and proprioceptive stimulation defining the body. Thus, both visual and proprioceptive adaptation are expected to occur with this exposure. The fact that either visual or proprioceptive changes are reported in a particular study, and not both, may be a simple function of the decision to obtain only one type of measure.[14]

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[11] Studies such as those conducted by Gibson, by Morant and Collaborators, and by Held and collaborators.

[12] Held, Movement-produced stimulation is important in prism-induced aftereffects: A reply to Hochberg, *Percept. mot. Skills*, 16, 1963, 764.

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[13] Harris, Adaptation to displaced vision: visual, motor, or proprioceptive change?, *Science,* 140, 1963, 812-813; C. R. Hamilton, Intermanual transfer of prism adaptation, this JOURNAL, 77, 1964, 457-462; Harris, *op. cit.,* 1965, 419-444.

[14] Illustrative here are the recent findings of J. C. Hay and H. L. Pick, Jr., Visual and proprioceptive adaptation to Optical displacement of the visual stimulus, *J. exp. Psychol.,* 71, 1966, 150-158.

SPEED OF READING WHEN THE SPAN OF LETTERS IS RESTRICTED

By EDWIN B. NEWMAN, Harvard University

When a person reads a page of print, he is presented with a very complex visual display. His eyes scan this display in an orderly fashion, and as the page is scanned, the information presented on the page is converted from its extended, simultaneous form into sequential information. This sequence governs the flow of ideas or actions. To the extent that we understand this process of conversion, we shall also understand many of the formal properties of perception and cognition. Such percepts are one step in the chain from complex stimulus to complex response.

It is well known from studies of eye-movements that the process of conversion proceeds in a saltatory manner. The actual processing lies between two extremes. On the one hand, the total page of print is complex far beyond the span of apprehension. With no single point of regard would it be possible to take in all of the detail in a single page, no matter how long the eye remained fixated. On the other hand, the eye is neither fast enough in its movements, nor is there enough efficiency in the processing of information, so that a person could gain understanding by scanning every discriminable point of a page in any possible order. The eye cannot function as the inverse of the point of my pencil as I write. Rather than either extreme, the eye performs a series of fixations, usually several to a line of print, and a block of information is taken in before the eye moves on to the next area in succession.

There is no assurance, of course, that some more central mechanism may not be scanning the field within each fixation and in some cases, such as typing, the seriatim character of the output makes it quite clear that an internal scanner must be at work. There are many more typing strokes than fixations by the eye. What we know from studies of eye-movement is simply that there is a largest size of the block that can be effectively handled.

Our ordinary eye-movements are so highly automatic that they do not

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offer us a very useful tool for experimental control. They are not particularly docile and they are technically difficult to record. There is real need for a somewhat different approach. This paper reports an experiment in which the usual situation is reversed, that is, in which the span and rate of presentation are experimentally controlled while the eye presumably remains relatively fixed. In this way the capacity of *S* to take in and process information can be tested under limiting conditions that will not occur in the course of natural reading.

PROCEDURE

The procedure employed was relatively simple in concept. Motion picture films were prepared that presented a set of from 1 to 10 letters on a screen. In each successive presentation the letters were moved one space to the left, the first letter was dropped off and a new letter was added at the end, that is, to the right. The appearance to the *S* was then that of a continuous band of text moving from right to left behind a window that permitted him to see just *n* letters. His task was to read aloud the passage being shown to him.

In the experiment reported here, 13 conditions were tested. Six spans were used; namely, 1, 2, 3, 4, 6, and 8 letters. The rate of presentation was also varied by having a given pattern occupy a single frame on the film, or 2, 3, or 5 successive frames. The films were projected at a constant speed of 10 frames per sec. Thus, the rates at which the text moved were 10 new letters per sec., 5 new letters, 3.3 new letters, and 2 new letters per sec. Not all combinations of the six spans and the four rates were employed, because for many of them *S* was either reading perfectly or not at all. This decision was reinforced by the difficulty of making films that had to be photographed one frame at a time. The tedium was considerable.

The texts presented to the *S*s consisted of passages chosen from John Locke's *Treatise of Civil Government*. Each passage was roughly 100 words in length. *S* read aloud to a magnetic tape recorder and he was later scored on the percentage of the words that he spoke correctly. The long spans that were easy to read were shown first and the task became progressively more difficult. Each *S* went through the experiment once, requiring about 40 min.

Before describing the results, I wish to make one point clear about this procedure. At any given rate of exposure, say 10 new letters per sec., there is the same total amount of information presented whether the span is one letter or eight. It takes the same length of time to run through a passage of 100 words; *S* speaks at the same rate into the recorder. To be sure, a given letter is exposed to view for a longer time with the greater span, but in so doing, it moves from one place in the eye to another. So far as what falls on one given area of the retina is concerned, a span of one is just like a span of eight. There is a sense, of course, in which more information is presented in the case of the longer spans. This is true in that a given letter is presented over and over again as it marches across the screen. But in the more technical meaning of the term information, no new information is added. The repeated presentations of each letter are entirely redundant in that they tell *S* nothing that he did not get before (if he saw it, of course!).

TABLE I

PERCENTAGE OF WORDS READ CORRECTLY

Texts roughly 100 words in length were read by 15 *S*s at each rate and span for which a value is given. Variability is homogeneous if percentages are converted to *Z*-scores. Standard error of these values is 0.1 sigma units, equal to about 4% in middle of scale.

Span of letters in each frame	Rate of adding new letters per sec.			
	2	3.3	5	10
8				96.51
6				95.24
4			98.91	88.43
3			97.27	78.84
2		92.01	94.14	31.36
1	87.85	71.18	47.10	9.53

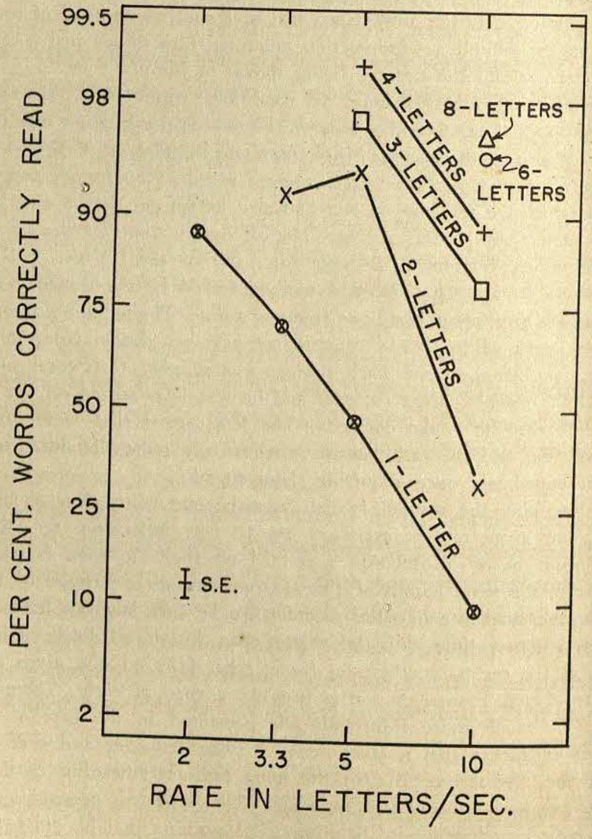

FIG. 1. PERCENTAGE OF WORDS READ CORRECTLY

RESULTS

The results of the experiment are given in Table I. Each value represents the mean percentage of the words spoken correctly by 15 *S*s. The same results are shown graphically in Fig. 1, in which the percentage has been plotted on a normal-probability scale and the rate of adding new letters on a logarithmic scale.

To estimate the variability in these data, it was necessary to transform the percentages into *Z*-scores because of the restriction at either end of the scale. When this is done, the variance is fairly homogeneous. The standard error of estimation is about 4% in the middle of the scale and proportionately less at either extreme.

The numerical results make clear what was strikingly obvious to the *S*s; namely, that the lack of context radically reduces the speed at which the material can be read. Taking the last column of the table as an example, text that could be read easily and with 96% accuracy so long as a string of eight letters was present became almost totally unintelligible as a series of single letters. To be sure, *S* has to pay rather close attention to the rapidly moving string of letters, but it was, nevertheless, clear. In contrast, the single letters were seen as a jumble, a twinkling jumble, out of which every now and then there came a clear letter. It was hard to believe that this jumbled series of letters was actually a constituent of the pattern seen clearly and in movement when the context was present.

TABLE II

NUMBER OF LETTERS CORRECTLY REPORTED PER SECOND

Each value is the percentage in Table I multiplied by the rate at which new letters appeared.

Span of letters in each frame	Rate of adding new letters per sec.			
	2	3.3	5	10
8				9.6
6				9.5
4			4.9	8.8
3			4.8	7.8
2		3.1	4.7	3.1
1	1.8	2.4	2.4	.95

The data of Table I may be cast in slightly different form in order to emphasize how limiting is the effect of the short spans. Suppose that we calculate the absolute number of letters correctly reported by multiplying each rate by the percentages reported. The results are shown in Table II. The last column again shows the ten-fold drop in rate as the context is reduced. But the table shows one other thing, that the rate of reading increases as the rate of presentation increases, and in some cases it reaches a maximum and then falls off. This is true both for single letters and for two letters. I feel sure that the same maximum would appear with longer spans of letters, but these were not tested because of the technical difficulty of producing a fast-moving display.

Discussion

There is an intuitive sense in which the display of the longer span of letters is richer in terms of possible cues than is the display with the short span. In strict informational terms, as was said, the net rate at which information is supplied is, for a given rate of presentation, always the same. The longer span is simply more highly redundant. On the other hand, this redundancy, that is, the continued presentation of the same letter in successive locations across the screen, gives the *S* freedom to utilize this information in some manner better suited to his capacities. This is the basic premise of the experiment.

It might be assumed, for example, that it takes about 0.3 sec. to process any one item of information. Thus, as will be seen in Table I, single letters presented at a rate of 3 per sec. lead to words that are 71% correct. When the span is 3 letters and the rate is 10 per sec., we might assume that the first exposure is seen, two and three are blanked out by some subjective process, then four is seen, five and six are blanked out, and so on. The rate at which an entirely new group of letters was presented would be 10/3, or 3.3 per sec. Indeed it proves to be true that three-letter 'chunks' can be seen with 79% accuracy at this rate. The over-all rate of reading with a 3-letter span is then three times as great as with the single-letter span, because the 'chunk' is three times as large, while the time per 'chunk' is constant. I will call this the 'shutter' hypothesis. There is no direct evidence to support it, but it is consistent with the numerical results.

The phenomenal appearance of the display raises other problems that make this somewhat simplified view less attractive. The description given above suggested that when the span was large, *S* saw a more or less continuous band of letters (or words) moving from right to left behind an opening. This appearance was not constant. It would break down from time to time. The direction of movement would occasionally reverse, or single words would appear stationary, often followed by a confused impression before the normal movement would resume. Much of the time the impression of a steady movement was, however, quite strong.[1]

The appearance of movement that has been described is characteristic of all of the longer spans. It is absent, most naturally, for single letters, because there is nothing to condition the display to move in any direction. Rather, there is an internal movement in the single letters that was quite absent with the longer span. One letter is transformed into another with a varied movement of its parts;

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[1] I should remark that it is possible to destroy this movement by greater spacing of the letters and by a choice of unfavorable size and brightness, but these facts limit but do not destroy the argument.

in the former case there is a translation of an unchanging letter. The transition between the two types of movement was, with the particular conditions of this experiment, between a span of two and a span of three letters. There was almost never a translatory movement of two letters; there was more often translation of three than not.

The problem posed is simply this: if there is an appearance of translatory movement, there must be an underlying identity between the geometric figure in one square and the next. Failing this, there can be no sense of direction in the movement. Now it is evident that this identity must be established locally and with sufficient saliency to control the movement. It is also evident that this local action takes place within a time that is several times shorter than the time necessary to 'recognize' the letter, as is evidenced by the almost complete inability of *S* to report anything about the fast display of single letters. In short, there is here direct evidence of a process reflecting the form of the letter well below the level of reportable recognition and presumably below the level at which apparent movement is determined. This is not, of course, a new problem—it is the identity problem described by Ternus[2]—but in these experiments it appears in a peculiarly persuasive form.

As an alternative to the 'shutter' explanation, we might use a model that had something like the following stages:

(1) An elementary establishment of form, perhaps at a quite primitive level.

(2) The determination of identity (or similarity) sufficient to condition the apparent movement of the display.

(3) The perceptual grouping of elements of form into strings of letters, possibly into words, that move as a unit across the field. This process may well be coincident with *(2)*, although it need not be.

(4) A transformation that provides *S* with a movement-free (and, more generally, stimulus-free) pattern of letters. The optimal length is uncertain, but it is certainly at least 4 or 5 letters in length (as inferred from the present paper) and is probably less than 10 or 12 (on the basis of other studies of eye-movements).

(5) The recognition of this string, or whatever other process may lead to a behavioral consequence that is recorded.

Admittedly, such a scheme is loose and speculative, but it may help to sharpen the results of this experiment.

SUMMARY

An *S* was asked to read a display that consisted of a meaningful string of letters displaced one letter at a time to the left. In a given presentation, the display had a constant span of letters, from eight down to a single letter and a constant rate at which new letters were added from 2 to 10 per sec. *S* read aloud all that he could see.

The results showed a 10 to 1 difference in the amount correctly reported when there was context present (a string of 8 letters) than when 1

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[2] Josef Ternus, Experimentelle Untersuchungen über phänomenale Identität, *Psychol. Forsch.*, 7, 1926, 81-136.

letter was present alone. Moreover, the longer spans were seen as letters of fixed form moving from right to left, while single letters underwent a plastic transformation.

The results set a lower boundary on the size of the perceptual 'chunk' which is handled more or less as a unit in the process of recognition and further response.

VISUAL SIZE-CONSTANCY AS A FUNCTION OF DISTANCE FOR TEMPORARILY AND PERMANENTLY MONOCULAR OBSERVERS

By H. W. LEIBOWITZ and R. A. DATO, The Pennsylvania State University

In their paper on the relation between apparent size and distance, Holway and Boring demonstrated that the tendency toward size-constancy is reduced in monocular observation.[1] They attributed the reduction to the fact that fewer cues are available with monocular than with binocular vision. In a later paper, Taylor and Boring reported that two permanently monocular *O*s showed a similar approximation to the law of the visual angle.[2] The extent of the reduction in the tendency toward size-constancy resulting from temporary monocularity as compared with the permanent loss of one eye is of critical importance to an understanding of the mechanisms of size-constancy. If the magnitude of the loss is the same for temporary as for permanent monocularity, binocular cues must be essential for size-constancy, but if the permanent loss of one eye does not result in an irreversible reduction in constancy, some sort of compensation for the binocular cues must be assumed.

The present study was designed to replicate and extend the work of Taylor and Boring. In addition to the critical importance of determining the extent of the loss of constancy resulting from permanent and temporary monocularity, there were two methodological reasons for reëxamining this problem: (1) Because size-matches, especially for distant objects, show significant inter-*O* variability,[3] the use of only two permanently monocular *O*s by Taylor and Boring necessarily limits the generality of their results. (2) Since the publication of the paper by Holway and Boring, a number of investigators have reëmphasized the observation of Holaday that the attitude of *O* as induced by different instructions can

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[1] A. H. Holway and E. G. Boring, Determinants of apparent visual size with distance variant, this JOURNAL, 54, 1941, 21-37.

[2] D. W. Taylor and E. G. Boring, Apparent visual size as a function of distance for monocular observers, this JOURNAL, 55, 1942, 102-105.

[3] Holway and Boring, *op. cit.*, 30; W. M. Smith, A methodological study of size-distance perception, *J. Psychol.*, 35, 1953, 143-153.

significantly influence the magnitude of the effect of size-constancy.[4] Although the instructions used by Holway and Boring and by Taylor and Boring were similar, differences in the sophistication of the *O*s might have resulted in their adopting different attitudes.[5] Holway and Boring's *O*s were Professor Boring, three graduate students, and the wife of one of the graduate students, while the two permanently monocular *O*s used by Taylor and Boring were undergaduates. The *O*s of Holway and Boring knew about size-constancy and the purpose of the study, while the undergraduates of Taylor and Boring were naïve regarding the purpose of the experiment. It would seem desirable to study temporarily and permanently monocular *O*s drawn from more comparable populations.

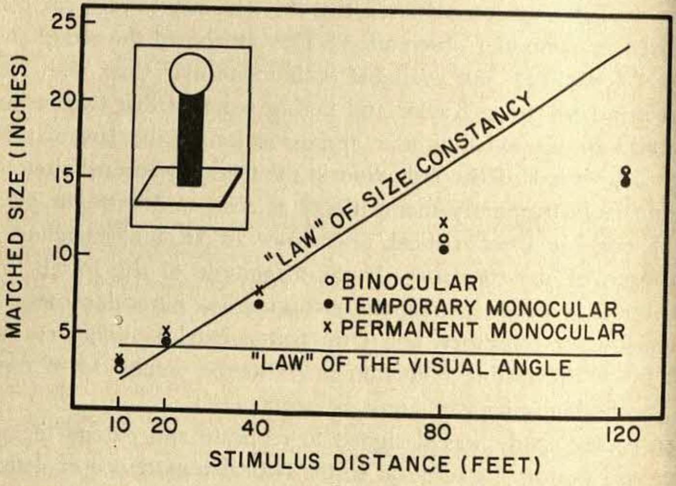

FIG. 1. MEAN MATCHED SIZE AS A FUNCTION OF DISTANCE FOR 20 NORMAL *O*S OBSERVING MONOCULARLY AND BINOCULARLY, AND FOR 15 PERMANENTLY MONOCULAR *O*S

(The diameters of the circular test-objects were proportional to the distances at which they are viewed. The insert shows the structure of the test-objects.)

The present experimental arrangement was modeled, insofar as possible, after the procedure used in the earlier studies, although a larger population of permanently monocular *O*s was used. The permanently monocular *O*'s as well as the normal *O*'s all were undergraduates at the Pennsylvania State University.

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[4] B. E. Holaday, Die Grössenkonstanz der Sehdinge bei Variation der inneren und äusseren Wahrnehmungsbedingungen, *Arch. ges. Psychol.*, 88, 1933, 419-486; A. S. Gilinsky, The effect of attitude upon the perception of size, this JOURNAL, 68, 1955, 173-192; V. R. Carlson, Overestimation in size-constancy judgments, this JOURNAL, 73, 1960, 199-213; Size-constancy judgments and perceptual compromise, *J. exp. Psychol.*, 63, 1962, 68-73.

[5] K. Kume, On the relation between perceived size and viewing distance with some consideration of factors determining size constancy, I and II, *Jap. J. Psychol.*, 22, 1952, 235-244; 23, 1952, 32-43.

Method. The stimuli were five circular wooden disks (see insert, Fig. 1) designed to subtend the same visual angle (57.2′ of arc) at the viewing distances of 10, 20, 40, 80, and 120 ft. These disks were made of ¼-in. plywood and painted flat white, with a reflectance of 87% and a luminance of 0.91 ft.-L. The diameters of the disks were 2, 4, 8, 16, and 24 in. The disks were supported by wooden bases made of plywood painted flat black, with a reflectance of 3%. The centers of all the disks were 4 ft. from the floor. Their bases consisted of two parts (see insert, Fig. 1). The lower part was cut from 1.5 in. wood stock and of such a size that the side facing *O* was twice the diameter of the disk; the other side, parallel to *O*'s line of vision, was equal to the diameter of the disk. The vertical supporting part was cut from 0.75-in. stock to a width equal to one half the diameter of the disk. The vertical portion was anchored in the center of the base; the disk was positioned in front of, and centered on, this vertical support.

O was seated at the end of a corridor which was 139 ft. long and 8 ft. wide. Various objects, *i.e.* chairs, coat-racks, and people, were present in the hallway during the experiment. Thus, the experimental situation provided a great many visual cues. A chin-rest was provided to support and stabilize *O*'s head. At *O*'s left side, there were switch-controls for a motor-driven adjustable projector which produced a variable spot of light on a screen located in a dark room 82° to the right of *O* and at a distance of 22 ft. The center of the spot of light (luminance 0.63 ft.-L.) was the same distance above the floor as the disks, (4 ft.). *O*'s task was to manipulate the projector control until the size of the spot of light appeared to be equal to that of the disk present in the corridor. *O* was allowed to turn freely from the standard-stimulus to the comparison stimulus. No time-limit was imposed for the matching procedure.

Instructions. The instructions were as follows:

> Five circular objects will be placed in the hallway one at a time. You are to adjust the circle of light on the screen so that it *appears* to be the same size as the object in the hallway. Do not take into consideration the distance or size you may think it to be, but just the way it appears to your eye.

The five stimuli were presented according to a 5 × 5 Latin-square design. The permanently monocular *O*s were tested twice. The control *O*s were tested once with monocular vision and once with binocular vision, half of them binocularly first, and the other half monocularly first. The size of the comparison-spot was randomly offset between trials.

Observers. The 20 control *O*s, volunteers from elementary psychology classes, were 11 men and 9 women with a mean age of 19.0 yr. They were unaware of the purpose of the experiment and unfamiliar with the phenomenon of size-constancy. The 15 permanently monocular *O*s all had 20/20 vision in the 'good' eye, with or without correction. These *O*s, 9 men, and 6 women with a mean age of 20.6 yr, were volunteers from the University student-population. The duration of the visual loss varied. One *O* was blinded as recently as 1.5 yr. before the experiment, while others were congenitally blind in one eye. The extent of the visual loss in the 'blinded' eye ranged from complete absence of vision to, for one, 20/200 vision (with divergent strabismus).

Results. Fig. 1 presents mean matched size as a function of distance for the control group observing monocularly and binocularly and for the permanently monocular group. At each distance, the differences among the groups appear to be quite small, and a two-way analysis of variance shows that they are not significant ($F = 1.4$, $df = 2/22$, $p > 0.05$); neither is the interaction of Groups with Distance ($F < 1$). Only the effect of Distance is significant ($F = 102.2$, $df = 4/88$, $p < 0.01$). For the permanently monocular *O*s, there was no appreciable correlation between size-of-match and duration of loss of vision ($r = 0.24$, $p > 0.05$).

Discussion. The present data indicate that the functions relating matched size to distance are approximately linear, the curves tending to lie closer to a theoretical prediction based on the law of size-constancy than to one based on the law of the retinal image. The most striking result of the present study is the absence of a significant difference between either temporary or permanent monocular observation and normal binocular observation. These data are at variance with those of Holway and Boring[6] and of Chalmers[7] which show a reduction for temporary monocularity, as well as with the data of Taylor and Boring[8] which show a similar reduction for two permanently monocular *O*s.

A plausible interpretation of this apparent discrepancy is suggested by the results of Holaday who systematically impoverished the stimuli available in a size-matching study over a range of 28 experimental conditions, among which were included a number of binocular-monocular comparisons.[9] His data indicate that under 'rich' cue-conditions (normal room illumination, presence of intervening objects, etc.), neither temporary nor permanent monocularity had any appreciable effect on size-matching, but that as the stimulus-situation was impoverished, both temporary and permanent monocularity produced a lesser tendency toward size-constancy than did binocular observation. In the Holway-Boring and Taylor-Boring studies, the corridor lights were extinguished and a minimum of cues was available; a similar procedure was used by Chalmers. The present experiment, by contrast, was carried out in a university corridor in which no attempt was made to eliminate the cues which normally would be available. It would appear, then, that for adult *Os* the effect of temporary or permanent monocularity depends upon the complexity of the cues available. With adequate cues, as Holaday has shown, binocular cues are not critical for normal *O*s. As the stimulus-situation is impoverished, the loss of size-constancy is greater for monocular than for binocular observation, as reported by Holway and Boring and by Chalmers.

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[6] Holway and Boring, *op. cit.*, 21-37.

[7] E. L. Chalmers, Jr., Monocular and binocular cues in the perception of size and distance, this JOURNAL, 65, 1952, 415-423.

[8] Taylor and Boring, interpreting their data, state "It would seem, therefore, that monocularity of many minutes' or a couple of hours' duration represents a perceptual reduction comparable, at least in this judgment, to permanent monocularity of several years standing." (Taylor and Boring, *op. cit.*, 104). If, however, one quantitatively compares the functions for either of Taylor and Boring's permanently monocular *O*s with the average obtained from Holway and Boring's temporarily monocular *O*s, it would appear that permanent monocularity results in a greater deficit than temporary monocularity. This interpretation has been expressed by Graham: "The two subjects long experienced in making monocular discriminations, gave data that more closely approximate the law of the visual angle than do the data of temporarily monocular subjects". (See C. H. Graham, Visual perception, in S. S. Stevens (ed), *Handbook of Experimental Psychology,* 1951, 874.)

With respect to permanent monocularity, the present data are consistent with those obtained by Akishige on one permanently monocular *O* who showed the same tendency toward size-constancy as a binocular *O* under normal conditions, but whose matches were lower than those made by a normal *O* observing binocularly and monocularly when cues were eliminated.[10] They also agree with the data obtained from Holaday's one monocular *O* who showed size-constancy equal to or better than that shown by normal *Os* under rich cue-conditions, but markedly less than that of normal *O*s as the cues were progressively eliminated. These observations with monocular *O*s also are consistent with the popular belief that permanent monocularity does not interfere with judgments of distant objects which ordinarily are involved in such activities as athletics, the driving of automobiles, and the flying of planes.

The results of the present study, obtained from a relatively large population of monocular *O*s, are of interest in relation to the mechanisms which mediate size-constancy. In this context, they are consistent with the hypothesis that size-constancy for distant objects is a result of perceptual learning based on experience with monocular cues.[11] It may well be the case that size-constancy can be mediated by a number of cues, some of which are redundant for adults under rich cue-conditions. Under this assumption, the loss of size-constancy under impoverished conditions would be accounted for in terms of the elimination of cues which can not be compensated for by those remaining. This point of view is consistent with the data presently available.

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[9] Holaday, *op. cit.*, 436-437, 454-456.

[10] Yoshiharu Akishige, Studies on constancy problem in Japan, *Psychologia*, 1, 1958, 143-157.

[11] H. P. Zeigler and Herschel Leibowitz, Apparent visual size as a function of distance for children and adults, this JOURNAL, 70, 1957, 106-109; Herschel Leibowitz and M. A. Heisel, L'évolution de l'illusion de Ponzo en fonction de l'âge, *Arch. Psychol., Geneve,* 36, 1958, 328-331; Noël Jenkin and Sally M. Feallock,

SUMMARY

An experiment on size-constancy as a function of distance is reported in which the matches made by a relatively large number of permanently monocular *Os* were compared with those made by normal *Os* viewing monocularly and binocularly. At each distance, the matches made under the three conditions were indistinguishable statistically. The results are consistent with the assumption that size-constancy for distant objects is mediated by learned monocular cues as well as by binocular cues. The discrepancy between these results and those of previous experiments is attributed to the difference in number of monocular cues available to *O*.

Developmental and intellectual processes and size-distance judgment, this JOURNAL, 73, 1960, 268-273; Leibowitz, Apparent visual size as a function of distance for mentally deficient subjects, this JOURNAL, 74, 1961, 98-100.

SOME CRITICAL FACTORS IN PRISM-ADAPTATION

By HARRY B. COHEN, Stanford University

There is general agreement as to the kind of systematic errors in reaching which are induced by wearing prisms displacing the visual field. There is disagreement, however, as to the relative importance of some of the factors which may, alone or in combination with others, determine such displacements.

A visuo-motor mechanism has been postulated with strong emphasis on the importance of reafferent stimulation from the musculature.[1] An even stronger emphasis on proprioceptive and motor adjustments, with a corresponding neglect of possible visual factors, was made by Harris.[2] As yet, the visual components, though tacitly assumed in the current explanatory schema, have not been specifically studied. This is surprising, since Kohler[3] reported an early experiment in which prisms were worn which displaced half of the visual field. When the prisms were removed, after-effects were confined to the exposed portion of the visual field. Since this effect varied as *S* moved his eyes, as well as his head, it was assumed to be not primarily visual in nature. The possibility remains that there may be some specific visual component which may be uncovered if investigated under appropriate conditions. The experiments described below explore the relative importance of visual mechanisms in prism-adaptation. Experiment I will study adaptation to induced displacement in a limb seen through the prism, but not moved by *S*, hence, providing little reafferent information. Experiments II and II will examine the effects of exposure of a limited retinal area to the prism. A *purely* proprioceptive-motor hypothesis would be excluded if it were found that under certain conditions of exposure the after-effects were confined to a specific portion of the visual field.

General procedure. The *Ss* in all three experiments were drawn from a group of 20 McGill University undergraduates, ranging in age from 18 to 23 yr. There were 10 men and 10 women. In each experiment, the *Ss* were equated for sex and handedness.

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[1] Richard Held, Exposure-history as a factor in maintaining stability of perception and coordination, *J. nerv. ment. Dis.*, 132, 1961, 26-32.

[2] C. S. Harris, Adaptation to displaced vision; visual, motor or proprioceptive change, *Science*, 140, 1963, 812-813.

[3] Ivo Kohler, Ueber Aufbau und Wandlungen der Wahrnemungswelt: insbesondere über "bedingte Empfindungen," *S. B. Ost. Akad. Wiss.*, 227, 1951, 1-118.

The apparatus for producing visual displacement consisted of a pair of small, close-fitting goggles having a 15 × 21 × 15 mm. right isosceles prism mounted in one eyepiece; the other eyepiece was occluded. The goggles allowed the *Ss* a monocular visual field at about 60°, which could be diminished when necessary. Objects in the field appeared displaced by approximately 22° toward the left of their true position if the prism's base orientation was toward the right, and the same distance to the right if the prism's orientation was base-left. After-effects were, therefore, in the same direction as the base orientation of the prism.

The displacement after-effect *(DAE)* which resulted from exposure to the prism was measured by a device similar to that described by Held and Gottlieb.[4] Briefly, when looking into the apparatus, the *Ss* could see the reflection of a target in a mirror so placed that the target appeared to lie on the horizontal surface of the table before them. There was an opening in the device through which the *Ss* could mark the apparent position of the targets. The mirror, however, concealed the *Ss* hands and the marks they made from view. They could neither observe nor correct their errors of localization. Head-movements during the marking were minimized by the positioning of close-fitting eyepieces through which the *Ss* looked during testing.

The common procedure for all three experiments was to allow the *Ss* to mark the apparent position of the target points until the scatter of their marks reached a consistent value, usually of the order of 1° of visual arc. This required from 3-5 min. of practice. They were then instructed to make a total of 10 marks at each of the target-points. To reduce any dependence on cues provided by sequential movements, the *Ss* were told to withdraw their hands after making each mark. When the pre-exposure marking was completed, the *Ss* were instructed to close their eyes, lift their heads from the marking apparatus and goggles were placed on them. The *Ss* then opened their eyes and for an exposure-period of 1 min. reached for a target which the *E* placed in view before them. The hand used in reaching will be referred to as the active, or exposed hand. The *Ss* could see the active hand and the target displaced from their actual positions during the exposure-period. When the exposure was over, the *Ss* looked into the marking apparatus and closed their eyes. The goggles were quickly removed, the *Ss* opened their eyes, and repeated the marking procedure. The *Ss* made 10 marks on each target during the first 30 sec. and 10 more during the next 30 sec. Different types of marks were made during each marking that they could be easily distinguished. The difference in position between the pre-exposure and post-exposure markings served as a measure of the size of the *DAE*.

EXPERIMENT I

Two conditions were employed in this experiment. The active hand was in view during the standard 1-min exposure. In the first condition, the other hand was clearly visible in the periphery, but was at rest throughout the exposure-period. In the second condition, the *Ss* held their unused hands behind their backs with no part of the arm visible during the exposure-period.

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[4] Richard Held and Norman Gottlieb, A technique for studying disarranged hand-eye coordination, *Percept. mot. Skills,* 8, 1958, 83-86.

Procedure. Before exposure, the *Ss* marked the apparent position of the target which in this case was made up of four dots set at the corners of a square, 15° along the sides. The *Ss* marked first with the hand to be exposed later, and then with the hand which was to be passive or concealed. After an exposure period of 1 min. to the active hand, the prism was removed and the *Ss* marked the targets with their passive hands for 1 min. The *Ss* were then asked to mark the target with the active hand to ensure that an after-effect had been produced. Twenty *Ss* were tested.

Results. All 20 *Ss* showed a significant *DAE* when marking with the hand which had been passive but visible during the exposure-period. The mean displacement was 5.2°. In contrast, no *S* showed significant inter-manual transfer when the passive hand was not visible during the exposure of the active hand. The difference between the two conditions was significant beyond the 0.001 level using a two-tailed t-test for correlated means ($t = 25.0$).

EXPERIMENT II

Experiment II was performed in order to study the transfer of *DAE* between the central and peripheral regions of retina of each eye.

Four women and four men served as *Ss*. Before they looked through the prism, they marked the apparent position of a series of three target-points arranged at 20° intervals across the visual field. The *Ss* were instructed to fixate a point in the center of the field during both the pre-exposure and post-exposure markings, so that when marking the lateral target-points the peripheral part of the retina was being used.

Procedure. There were two exposure-conditions. In the first, vision through the prism was limited to a central field 10° in diameter. The *Ss* were instructed to fixate an object placed in front of them, while reaching for it for 1 min. In the second condition, the goggles were masked to allow a 15° horizontal and a 10° vertical field at the edge of either the nasal or temporal periphery of the goggles' 60° field. There was, in addition, a small pinhole aperture in the center of the mask that allowed the *Ss* to fixate a point straight ahead. Thus, assuming no gross eye-movements, the two conditions achieved differential stimulation of the central and peripheral retinal areas. During lateral exposure, the *Ss* were asked to bring their hands slowly into the periphery of the visual field; when it was visible, they commenced reaching for an object in that area while fixating straight ahead through the pinhole for one minute. After the exposure was completed, they again marked the target display with the exposed hand.

Results. The results of this study showed that if the *Ss* saw their hands moving in the central 10° of the visual field, equally large after-effects were obtained at points 20° to the left, at the center, and 20° to the right of the fixation-point. In conditions in which the visually displaced hand was exposed only in the periphery of the retina, the *DAE*'s obtained for the first 30-sec. interval were significantly greater on the exposed side of the field than on the other parts ($p < 0.01$, $t = 4.2$, two-tailed test). After 2.5 min., a *DAE* was measurable only in the exposed periphery. These data appear in Table I.

EXPERIMENT III

The results of Experiment II suggest that the larger *DAE*s obtained from targets which fell within the exposed peripheral areas were a function of the retinal locus stimulated. Proprioceptive and motor learning which resulted from repeatedly reaching for the visually displaced object in the same part of the field relative to *S*'s body could also account for the enhanced after effects from targets on the exposed side. If the increased *DAE*s were the result of proprioceptive and motor adjustments, one would expect them to occur while testing without fixation, since the increased *DAE*s would be present at the same side of the *S*s' body as the exposed visual field, no matter where he was looking during the marking. If, on the other hand, the retinal area on which the target fell was crucial in producing the enlarged peripheral *DAE* found in Experiment II, then one would not expect any great difference between *DAE*s from targets which correspond to exposed and non-exposed retinal areas, so long as the *S* looked directly at the target which he was marking and did not fixate the center of the field. Therefore, in Experiment III, *S*s were allowed to mark targets without fixation after exposure.

TABLE I

MEAN DAEs FROM 8 *S*s FOR 3 TARGET AREAS DURING FIRST 30 SEC. AFTER EXPOSURE OF A 10° CENTRAL RETINAL AREA OR A 10°×15° LATERAL AREA

Field exposed	Field tested		
	right	central	left
right	7.9°	4.7°	3.2°
central	9.2°	10.2°	8.9°
left	3.4°	4.5°	9.1°

Procedure. Eight *S*s marked the apparent positions of targets 20° to the left, at the center, and 20° to the right of the mid-point of the 60° field of the testing apparatus. They then watched their hands reaching for a target in the periphery in a manner similar to the peripheral exposure conditions described above in Experiment II. The *Ss*, however, did not fixate the center of the target display during the post-exposure marking. They were instructed to look directly at the portion of the target display which they were marking.

Results. The mean *DAE*s obtained without fixation were 5.5°, 4.4°, and 3.8° for the exposed, central, and opposite target areas during the first 30 sec. after exposure. There were no significant differences between *DAE*s for the three target areas. The corresponding mean *DAE*s obtained with fixation were 8.5°, 4.6°, and 3.3° for the same target-areas. When *DAE*s from the exposed peripheral target areas were compared, those from Experiment II with fixation were significantly larger than those from Experiment III without fixation. *DAE*s at the exposed peripheral target for these two conditions differed at better than the 0.01 level ($t = 5.2$). Again, two tailed t-tests for correlated means were used in all comparisons.

DISCUSSION

The results show that a *DAE* can be induced in the passive hand only when it is visible while the exposed, active hand is seen moving (Experiment I). Also, it has been found that a *DAE* can be confined to only one portion of the visual field (Experiments II and III). It is not unreasonable, therefore, to conclude that an important visual (as well as visuo-motor) component is involved in adaptation in these experimental conditions.

The demonstration of visual factors in prism-adaptation requires an extension of current theories. Thus, the model proposed by Held and elaborated by Hein and Held cannot account satisfactorily for the results of Experiment I, where one of the *Ss'* hands was passive and yet significant *DAE*s were obtained.[5] Similarly, the hypothesis offered by Harris cannot easily account for the visual field-effects obtained in Experiments II and III, since their explanations are solely concerned with changes in the significance of proprioceptive information from the *Ss'* limbs.[6]

The model proposed here is based on the idea that adaptation to a displaced visual field consists of a temporary reorganization of neural connections among cells which normally subserve visuomotor functions. This reorganization is initiated by a matching process between outputs of neurons which convey visual information and those which convey proprioceptive information as to limb and body position. These connections between visual and proprioceptive systems might be formed by a diffuse projection of retinal fibers which combine with projections from proprioceptors in the limbs. Under normal perceptual conditions, these combined projections would provide the motor system with the information needed to execute an appropriate response, *i.e.* reaching for a seen object. When vision is misaligned by the introduction of displacing prisms, new response patterns must be found in order to execute visually guided movement. This computation would be facilitated by the fact that the relations between proprioception and movement are normally fixed, providing a source of constant reference between motion and the perception of that motion. The relations between motion and its perception via other sensory modalities may, however, be experimentally rearranged by a fixed or varying amount.

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[5] Held, *op. cit.*, 26-32; Allen Hein and Richard Held, A neural model for labile sensorimotor coordinations, in E. E. Bernard and M. R. Kare (eds.), *Biological Prototypes and Synthetic Systems*, I, 1962, 71-74.

[6] Harris, *op. cit.*, 812-813.

The model would then operate in the following manner: When the *S* reaches for a visually displaced target, visual and proprioceptive information regarding the location of the target and the position of the *S*'s hand are fed into a comparator which provides brain areas controlling motor responses with the information needed to perform movement to the target. Since target and limb are seen through the prism, movement computed from stored information based on past experience is not successful. On subsequent attempts (fed back), internal *reafferent* input from proprioceptors is matched with *reafferent* visual input at the comparator. Here a new computation is made which may compensate for the size and direction of error. This correction is passed on to the response control center. Success or failure of the attempted reach is signaled to the comparator by tactual as well as visual input. When the *S*'s attempt is successful and the target is reached, antecedent movements and computations are reinforced. When the prism is removed, this compensation remains (as DAE) until the system is reset to the pre-exposure coordination by normal visual, tactual and proprioceptive inputs.

The critical factor in the production of intermanual transfer of DAE was the presence of the passive hand in the visual field while the active hand was seen moving. In this condition, there was an opportunity for both visual and proprioceptive representations of the active and passive hands to reach the comparator. Corrections made for the active hand were passed on to the contralateral hand in somewhat attenuated form. When the passive hand was not in the field, there was no opportunity for a combined input to reach the comparator, and hence no displacement after effect was found in that hand. Thus, it would seem that the essential conditions for prism adaptation must include the input of rearranged sensory (visual) signals, as well as combined visual and proprioceptive *reafferent* information resulting from motion of the *S* made under the influence of the rearrangement.

SUMMARY

Transfers of displacement after-effects (*DAE*) between hands as well as between center and periphery of each eye were investigated following brief exposure to a monocular prism. Twenty *S*s were tested for intermanual, and eight for intraocular transfers. No intermanual transfer was found as long as the non-exposed, inactive hand was out of sight; however, transfer was present if the passive hand was visible during exposure of the active hand. Complete transfer of the *DAE* was found from exposed central areas to non-exposed peripheral areas of each eye. When, however, peripheral areas were exposed, *DAE*s were largely confined to the exposed periphery. These results cast doubt on any hypotheses which attribute the production of *DAE*s solely to proprioceptive and motor adjustments.

EFFECT OF LIGHT-PERCENTAGE, STIMULUS-LUMINANCE, EXPOSURE-DURATION AND THEIR INTERACTIONS UPON THE *CFF* THRESHOLD

By Thomas J. Moore, University of Massachusetts

The phenomenon of critical flicker fusion (*CFF*) has been extensively investigated and has achieved some popularity as a clinical test for such diverse conditions as fatigue, perseveration, and anxiety-reactions.[1] Although numerous parameters have been investigated relatively little is known concerning the interactions of these parameters.[2] The importance of such interactions has been emphasized in recent years by a series of studies which seem to indicate that what appear to be contradictory findings concerning the relationship between the percentage of time the light pulse is on in one stimulus-cycle (P_L) and *CFF* threshold may be reconciled by the existence of an interaction between the luminance of the stimulus during the on-period in any one cycle (I) and P_L.[3]

A third parameter which may interact with other variables is that of exposure-duration (*ED*), *i.e.* the amount of time that the intermittent stimulus is presented. Granit and Hammond have shown that increases in *ED* up to 1.0 sec. resulted in an increase in *CFF*-threshold.[4] Durations beyond 1.0 sec. were not investigated.

The present study investigated the effects of three parameters (I, *ED*, and P_L) and their interactions. Four groups, with 8 *O*s each, served in this study. Each group received either an intermediate or a high value of luminance (I) and an exposure-duration (*ED*) at each of three P_L-values.

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* Received for publication June 7, 1965. The author is presently at the Bionics and Biodynamics Branch, 6570th Aerospace Medical Research Laboratories, Wright-Patterson AFB, Ohio.

[1] Ernest Simonson and Josef Brozek, Effects of illumination level on visual performance and fatigue, *J. opt. Soc. Amer.*, 38, 1948, 384-393; S. Biesheuval, The measurement of the threshold for flicker and its value as a perseveration test, *Brit. J. Psychol.*, 29, 1938, 27-38; H. E. Krugman, Flicker fusion frequency as a function of anxiety reaction: An exploratory study, *Psychosom. Med.*, 9, 1947, 269-272.

[2] Carney Landis, Determinants of *CFF* threshold, *Physiol. Rev.*, 34, 1954, 259-286.

[3] S. H. Bartley, Some factors influencing critical flicker frequency, *J. Psychol.*, 46, 1958, 107-115; S. H. Bartley and T. M. Nelson, Equivalence of various pulse-to-cycle fractions in producing critical flicker frequency, *J. opt. Soc. Amer.*, 50, 1960, 241-244; V. V. Lloyd and Carney Landis, Role of the light-dark ration as a determinant of the flicker-fusion threshold, *ibid.*, 50, 1960, 332-336.

[4] R. A. Granit and E. L. Hammond, Comparative studies on the peripheral and central retina. V. The sensation-time curve and the time course of the fusion frequency of intermittent stimulation, *Amer. J. Physiol.*, 98, 1931, 654-663.

Method: Apparatus. The intermittent photic stimulus was produced by an electronic source (Grason-Stadler Flicker Fusion Apparatus, model E622), which drove a cold cathode glow modulator tube (Sylvania R1131C).

The point-source of light, which was located 24 in. from *O*'s eyes, was centered on the fovea and subtended a visual angle of 17′. The surround subtended a visual angle of 1.2° and had an *I* level of 0.88 mL. These conditions were constant for all *O*s. The *I*-levels of the test-patch and surround were determined by taking the mean of a series of determinations made with a Macbeth Illuminometer at *O*'s eye-level.

The study was run in a darkened room in which illumination was provided by three 25-w. red bulbs, thus initiating dark-adaptation as soon as *O* entered. *O* was seated on a cushioned, adjustable chair within a plywood cubicle, which had been painted a flat black. The test-light and surround were built into the wall of the cubicle opposite *O*. Attached to the chair were an adjustable chin-rest and head-support, which aided in inhibiting movement.

Procedure. After *O* had been brought into the experimental room he was seated and the necessary adjustments to the chin-rest and head-support were made. *O* was then instructed to fixate at the center of the green surround at all times and was given five stimulus-presentations at various frequencies to acquaint him with the task. The test-patch, surround, and room-illumination were then extinguished to allow partial dark-adaptation. At the end of 5 min. the surround was turned on and *O* was told the experiment was about to begin.

Three ascending and three descending binocular determinations were then obtained by means of a method of limits at each of three P_Ls. The values of *I* and *ED* employed for any one *O* were determined by the group to which he had been assigned. The stimulus was first presented at a frequency where it was certain it would be seen as 'flickering.' The frequency was then increased by steps of 10-c.p.s. until it was reported to appear 'steady.' *E* started at the lower limit of the previous 10-c.p.s. interval and increased the frequency by discrete 1-c.p.s. steps, until *O*'s report changed from 'flicker' to at least four consecutive 'steadies.' The frequency at which the first of the four consecutive 'steadies' was elicited was taken as the threshold for that ascending series. The reverse procedure was then employed to determine the threshold for a descending series. The mean of the six determinations was considered *O*'s threshold for the particular P_L used, under the experimental conditions specified for his group.

Observers. The *O*s consisted of 32 men drawn from introductory classes in psychology. All were between 17 and 20 yr. of age and had no previous experience in flicker-studies. They were first screened for visual acuity by means of a Snellen eye-chart, and met a criterion of 20/30 corrected vision. Those who wore glasses while taking the eye-test also wore them during the study.

Design. The *O*s were randomly assigned to one of four groups. Each group received one of two values of two variables: *I* (7.72 and 32.32 *mL*), and *ED* (0.5 and 1.5 sec). Thresholds were obtained at three P_L-value (20, 50, and 90%) for each *O*. Order of presentation of the P_L was randomized within each group. Values of *I* were uncompensated.

Results. The mean *CFF*-values for the various combinations of P_L, *I*, and *ED* employed appear in Table I. The data collected were subjected to an analysis of variance, with I and ED as between Os and P_L as a within Os. The data contributing to the two significant main effects revealed that *CFF*-threshold varied directly with I ($p < 0.025$), and inversely with P_L ($p < 0.001$), with a slight drop in threshold between $P_L = 20\%$ and $P_L = 50\%$ (1.25 c.p.s.) and a large drop between $P_L = 50\%$ and $P_L = 90\%$ (13.26 c.p.s.).

Of greater interest for purposes of the present study are the interactions.

TABLE I

MEAN CFF VALUES IN CPS FOR COMBINATIONS OF P_L, I AND ED

P_L	I (mL)	ED 0.5 sec.	ED 1.5 sec.
20%	7.72	33.67	36.94
	32.32	40.81	36.27
50%	7.72	32.88	34.71
	32.32	39.85	35.25
90%	7.72	19.07	21.15
	32.32	26.29	23.85

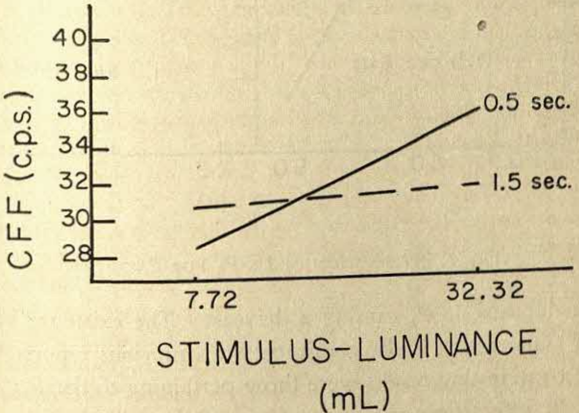

FIG. 1. INTERACTION OF I x ED

The significant double interactions are $I \times P_L$ ($p < 0.01$), and $I \times ED$ ($P < 0.05$). The $I \times P_L$ interaction was significant primarily because of a proportionately greater drop in threshold at the 90% P_L for the 7.72-mL stimulus. More interesting are the functions for the $I \times ED$ interactions shown in Fig. 1. The function for 0.5 sec. displays the expected relation that as I increases, *CFF* threshold increases. The function for 1.5 sec. indicates, however, that there was no significant change in threshold between 7.72 and 32.32 mL. Inspection of the functions for the $I \times ED \times P_L$ interaction ($P < 0.025$), shows that this lack of significant difference between the *CFF* thresholds for 7.72 and 32.32 mL at 1.5 sec. *ED* exists at each of the P_Ls employed in the present study (Fig. 2).

Discussion. The main effects are similar to those which have been previously reported, with an increase in the level of *I* causing a rise in threshold and an increase in P_L causing a decrease.[5] The existence of a significant $I \times P_L$ interaction is also congruent with previous reports.[6]

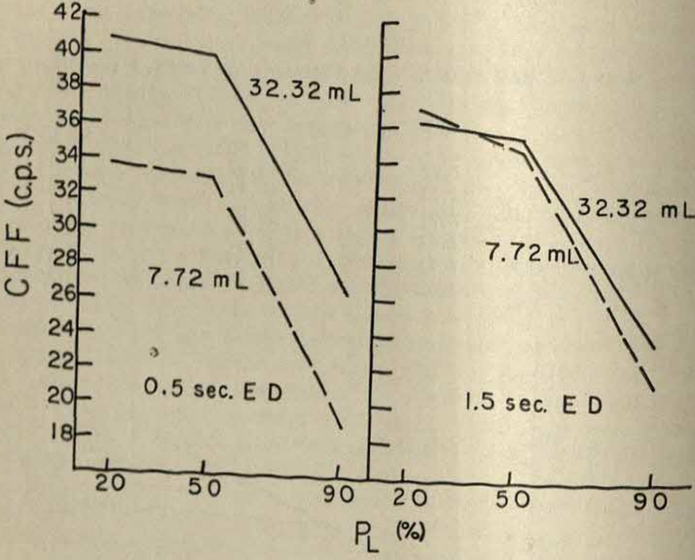

FIG. 2. INTERACTION OF $I \times P_L$ FOR EACH *ED*

The most interesting results were those pertaining to the $I \times ED$ and $I \times ED \times P_L$ interactions. From the form of these interactions it appears that the levels of *I* employed in the present study had no differential effect upon *CFF* threshold when an *ED* of 1.5 sec. was employed. With an *ED* of 0.5 sec. the usual effect of the higher *I* yielding the higher threshold was obtained. It is possible that for the shorter *ED* the contrast between the light and dark periods of each cycle was the primary basis for the judgment of flicker or fusion. Because of this the cycle with the greater contrast, *i.e.* the higher level of *I*, would be easier to perceive as flickering and would therefore result in a higher *CFF* threshold. The longer presentation-time may have enabled *O* to adopt an alternate, more stable, judgment criterion, thus yielding approximately the same threshold for each of the two levels of *I*.

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[5] Ross McFarland, A. B. Warren, and Charles Karis, Alternations in critical flicker frequency as a function of age and light-dark ratio, *J. exp. Psychol.*, 56, 1958, 529-538.

[6] Bartley and Nelson, *op. cit.*, 241-244; Lloyd and Landis, *op. cit.*, 332-336.

LOCAL ADAPTATION TO INTERMITTENT LIGHT AS A FUNCTION OF FREQUENCY AND ECCENTRICITY

By Norman Ginsburg, State University of New York, Oneonta, N.Y.

Critical flicker frequency (*CFF*) can be influenced by a variety of variables; physical (intensity, area), physiological (drugs, ablation), and psychological (anxiety). Granit and Von Ammon demonstrated that *CFF* could also be lowered by exposing the eye to intermittent light prior to threshold determination.[1] They labeled this effect 'local adaptation' (*LA*). Little is known about the variables that influence it, or about its neurophysiological correlates. The present experiment was intended to determine the effect on local adaptation of two variables: frequency of the adapting stimulus and retinal position.

METHOD

Two methods have been used to measure local adaptation; either frequency is held constant and time measured, or time is held constant and frequency measured. In the first case, the stimulus is made to flash at a frequency a few cycles below the *CFF* and the time required for fusion to occur is measured.[2] In the second, the *CFF* is measured before and after exposure to an adapting stimulus of constant duration.[3] The latter technique was used in the present study since the effect of frequency was of primary concern.

Observers. The *Os*, 15 in number (7 men and 8 women), were composed of 2 psychologists and 13 patients at the New York State Psychiatric Institute. The psychologists were experienced in observing intermittent lights; the patients, who ranged in age from 17 to 33 yr., median 24 yr., were not experienced. At the time of their dischange, 1 of the patients was diagnosed as a psychoneurotoc; 12 as schizophrenics.

Apparatus. The intermittent light was provided by a small spiral fluorescent tube manufactured by Aristo Grid Lamp Products, Forest Hills, N.Y. A square wave was produced with equal light-dark time, variable in frequency from 3 to 70 ~/sec. The apparatus was callibrated and checked during the experiment with an electronic

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* Received for publication June 14, 1965. These data were collected at the New York State Psychiatric Institute. The writer is indebted to the late Carney Landis for advice and encouragement.

[1] R. A. Granit and Wilma von Ammon, Comparative studies on the peripheral and central retina: III. Some aspects of local adaptation, *Amer. J. Physiol.*, 95, 1930, 229-241.

[2] L. A. Riddell, Local adaptation to flicker and its relation to light adaptation, *J. Physiol.*, 84, 1935, 111-120.

[3] P. A. Snell, An introduction to the experimental study of visual fatigue, *J. Soc. Mot. Pict. Engrs.*, 20, 1933, 367-390.

counter (the Berkeley-Events-per-Unit-Time Meter, Model 5556B made by Beckman Instruments, Inc., Richmond, California). The frequency of the stimulus was recorded continually on a Varian recorder. The light source was placed 5 cm. behind a sheet of Plexiglas. A 3-cm. disk of light at a distance of 1 m. from *O* provided a target of 1° 43′ with a luminance of 15 m£.; the surround was a gray cardboard of 0.25 m£. The room was illuminated by an overhead, 100-w bulb; the luminance of the walls measured 1 m£. During peripheral stimulation, a white bead of ⅛-in. diameter was fixated binocularly, the fixation-point being moved vertically from the target for different eccentricities.

The rate of intermittence of the stimulus was changed automatically with an acceleration of 4 ~/sec./sec. This rapid rate was used since *LA* drops approximately 80% after a 20-sec. delay.[4] During *CFF* measurement, the direction of change in frequency was controlled by *O* who, on command of *E*, pressed a push-button (causing frequency to decrease) and released the button (causing frequency to increase to a maximum of 60 ~/sec.) when the target first appeared to flicker.

Procedure. Each trial consisted of a *CFF* determination by the descending method of limits, then exposure to the adapting stimulus, then another *CFF* determination. The amount of *LA* was determined by subtracting the second value from the first. Each *O* was tested at 8 retinal positions: 0, 3, 5, 7, 9, 11, 13, and 15°. The frequency of the adapting stimulus was always set relative to the *CFF* that had just been obtained. The following settings were used: —30, —20, —10, 0 (at the *CFF*), 10, and 20 ~/sec. The duration of the adapting stimulus was 10 sec. A control condition also was used in which the shutter, which measured the same luminance as the surround, remained closed for 10 sec. prior to the second *CFF* determination. Each adapting stimulus was used at all retinal positions, so that each *O* contributed 48 *LA* plus 8 control values.

After an initial day of practice, there were seven daily experimental sessions, each consisting of eight trials, one at each retinal position. In all, eight orders of presentation of retinal position were used for the *O*s, but for any given *O*, the same order was used throughout the experiment.

Results. The mean values of *CFF* before adaptation were (from 0° to 15°): 42.6, 43.2, 43.5, 44.2, 44.7, 44.4, 43.5, and 41.9 ~/sec.

In Fig. 1, *LA* is shown as a function of the frequency of the adapting stimulus, and in Fig. 2 as a function of eccentricity. Since the curves for practiced and naïve *O*s were very similar in both figures, the data from both groups were combined for purposes of analysis.

Analysis of variance indicates that the effects of both frequency and eccentricity are significant at the 0.1% level. ($F = 94.56$ and 10.71, $df = 6$ and 7 respectively; $df = 84$ and 98 for the error terms.) For further analysis of the data, the Duncan range-test was used. With four exceptions, each frequency yields significantly more *LA* than those to the right of it in Fig. 1 ($p < 0.01$). The exceptions are: —30 vs —20, 20 vs C, 10 vs C, and 10 vs 20. Similar analysis of the eccentricity data shows that 0° to 3° do not differ significantly, but both yield significantly less *LA* than all points from 7° to 15°. None of the points from 7° to 15° differ significantly from each other.

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[4] Adolph Streng, Jr. and Norman Ginsburg, Time decay of local adaptation to intermittent light, *Amer. Psychologist*, 19, 1964, 719.

Discussion. Granit and Von Ammon reported that with central fixation there was little or no *LA*.[5] The present results show that, although *LA* is greater in the periphery, there is a considerable amount in the fovea. The difference in result may be due to a difference in method; Granit and Von Ammon do not give a description of the procedure by which they measured *LA*.

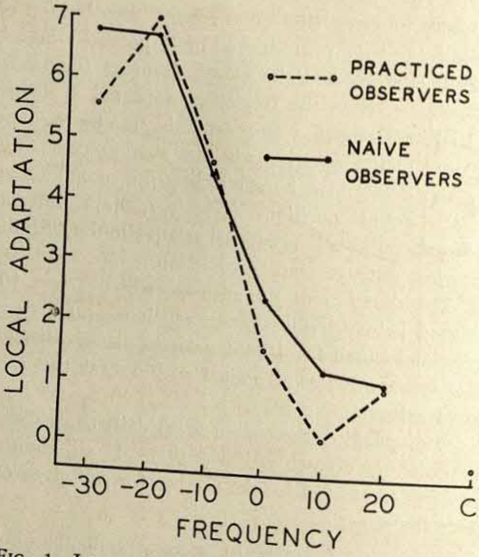

FIG. 1. LOCAL ADAPTATION (CYCLES PER SEC.) AS A FUNCTION OF FREQUENCY OF THE ADAPTING STIMULUS (CYCLES PER SEC.) Frequency is given relative to the *CFF*.

The present results differ also from those reported by Alpern and Sugiyama.[6] These investigators, confining the stimuli to the fovea, found no systematic differences in *LA* with variation in the adapting light from 2 to 20 ~/sec. below *CFF*. In the present experiment, by contrast, the foveal *LA* values for 0, 10 and 20 ~/sec. below *CFF* were 1.2, 2.9 and

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[5] Granit and Von Ammon, *op. cit.*, 229-241.

[6] Mathew Alpern and Sadao Sugiyama, Photic driving of the critical flicker frequency, *J. Opt. Soc. Amer.*, 51, 1961, 1379-1385.

4.1 respectively. The duration of the adapting stimuli in the experiment of Alpern and Sugiyama was 3 min.; in the present experiment, it was 10 sec. It may be that the shorter period of adaptation is more sensitive to differences in frequency.

From the effect of frequency on *LA* one could predict a difference in *CFF* between ascending and descending trials in the method of limits. We would expect ascending as compared with descending values to be depressed since lower frequencies produce more *LA* and the test-stimulus itself would act as an adapting stimulus. This predicted difference has been shown to occur.[7]

Granit and Von Ammon have found that *LA* increases as a function of time to at least 6 sec.[8] Alpern and Sugiyama have reported an increase to 10 sec.[9] In the ascending method of limits, the lower the starting point the longer *S* is exposed to the flickering target, which could function as an adapting stimulus. We would predict lower *CFF*s for lower starting points, a result which has been obtained by Ryan, Bitterman, and Cottrell.[10]

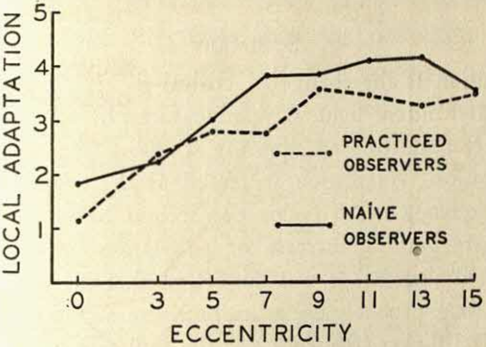

FIG. 2. LOCAL ADAPTATION (CYCLES PER SEC.) AS A FUNCTION OF ECCENTRICITY

The finding of differences in *LA* due to differences in frequency of the adapting stimulus has implications for possible neural mechanisms underlying *LA*. The results of two experiments in which impulses from single on-off fibers of the retina and visual cortex of the cat during intermittent light stimulation have been recorded show that, at low rates of intermittence each pulse of light elicits multiple impulses; as flash-frequency increases, the number of nerve impulses for each light-pulse decreases until each flash evokes a single impulse; and that for higher frequencies, firing becomes sporadic or disappears altogether.[11] *CFF* is defined by these workers as the highest rate at which impulses in the single cell can keep in step with the pulses of light.

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[7] Ginsburg, Ascending-sescending differences in *CFF* as a function of eccentricity, *Amer. Psychologist,* 18, 1963, 456.

[8] Granit and von Ammon, *op. cit.,* 229-241.

[9] Alpern and Sugiyama, *op. cit.,* 1379-1385.

[10] T. A. Ryan, M. E. Bitterman, and C. L. Cottrell, Relation of critical fusion frequency to fatigue in reading, *Illum, Engng.,* 48, 1953, 385-391.

Osgood and Heyer have suggested that cortical cells innervated by retinal on-off fibers adapt as a negatively accelerated function of their rate of excitation.[12] These cortical cells and their ganglion fibers have maximum rates of firing when the retina is stimulated at frequencies below *CFF*.[13] The greatest amount of cortical adaptation should, therefore, occur with a light flashing at frequencies below *CFF*. Let us assume that the perception of flicker depends on activation of these cortical cells, which in turn depends on a certain minimum rate of firing in their ganglion fibers. Adaptation in the cortex would necessitate a higher rate of firing in the nerve for cortical excitation to occur. This could be provided by a lower flash rate. Thus, exposure to flash rates below *CFF* should result in a lower *CFF*; that is, in *LA*.

Summary

Local adaptation is the change in critical flicker frequency due to exposure to an intermittent light. It was measured by subtracting the critical flicker frequency following an adapting stimulus from the score obtained prior to adaptation. Adaptation increased as the adapting stimulus was lowered in frequency to 20 cycles per second below the critical flicker frequency. There was an increase of adaptation toward the periphery, leveling at seven degrees. It was suggested that the effect of frequency on local adaptation probably underlies both the ascending-descending difference in critical flicker frequency and the influence of the starting point, and that local adaptation depends on cortical cells that mediate the activity of retinal on-off fibers.

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[11] Christina Enroth, The mechanism of flicker and fusion studied on single retinal elements in the dark-adapted eye of the cat, *Acta Physiol. Scand.*, 27, 1952, Suppl. 100; O.-J. Grüsser und Otto Creutzfeldt, Eine neurophysiologische Grundlage des Brücke-Bartley-Effektes: Maxima der Impulsfrequenz retinaler und corticaler Neurone bei Flimmerlicht mittlerer Frequenzen, *Arch., ges. Physiol.*, 263, 1957, 668-681.

[12] C. E. Osgood and A. W. Heyer, A new interpretation of figural after-effects, *Psychol. Rev.*, 59, 1952, 98-118.

[13] Grüsser and Creutzfeldt, *op. cit.*, 668-681.

THE RELATION OF FITTINGNESS, WORDNESS, AND POLARITY OF CONCEPTS

By BERT ZIPPEL and JAMES R. DOTSON, University of New Mexico

Wertheimer hypothesized that words rated as 'fitting' are more extreme on Osgood's semantic differential scales than words rated as 'less fitting'.[1] 'Fittingness' is the judged appropriateness of the sound of a word to its meaning. Selected words judged as fitting and nonfitting were rated on 10 different semantic differential scales with the fitting words showing significantly greater extremity of ratings than the nonfitting words. More recently, Jenkins reported 'extremeness' ratings, which he called degree of polarization, for 360 selected concepts.[2] If there is a relationship between fittingness and polarity, as suggested by Wertheimer, concepts of known high polarity should be judged as more fitting than concepts of known low polarity. An alternative explanation of fittingness, however, would be that highly familiar words fit their meaning as the result of association and use. That is, fittingness is more closely related to 'wordness' than to the amount of meaning of the word.

It was hypothesized that: (1) fittingness is positively related to polarity; (2) fittingness is positively related to wordness; and (3) fittingness is more closely related to wordness than to polarity.

Method: Subjects. The *Ss* in the experiment were 100 undergraduate students. All of them rated the concepts chosen on the dimensions of fittingness and wordness. Semantic differential ratings of the concepts were also obtained from 31 of these *Ss*.

Design. A list of 30 concepts was chosen from Jenkins' *Atlas*. The polarization of the chosen concepts ranged from a high value of 8.35 through steps of approximately 0.24 scale-points to a low value of 2.24. The seven bipolar scales that best represented each of the major factors found by Jenkins, Russell, and Suci were used for the semantic differential ratings.[3] The 30 concepts were randomly ordered on mimeographed sheets with the 7 scales below each concept. To measure wordness and fittingness, the 30 concepts were randomly ordered on each of two mimeographed sheets with appropriate instructions for judging wordness and fittingness.

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* Received for publication July 2, 1965.

[1] Michael Wertheimer, The relation between the sound of a word and its meaning, this JOURNAL, 71, 1958, 412-415.

[2] J. J. Jenkins, Degree of polarization and scores on the principal factors for concepts in the semantic atlas study, this JOURNAL, 73, 1960, 274-279.

[3] J. J. Jenkins, W. A. Russell, and G. J. Suci, An atlas of semantic profiles for 360 words, this JOURNAL, 71, 1958 688-699.

Procedure. The 30 concepts were rated on the semantic differential scales by 31 *Ss*. All 100 *Ss* rated the 30 concepts on fittingness and wordness with the order of presentation of these two tasks randomly varied. For judging fittingness, the *Ss* were instructed to place a 'yes' after the concept if the sound of that concept was appropriate to its meaning and to place a 'no' after the concept if it was not appropriate. For judging wordness, the *Ss* were instructed to place a 'yes' after the concept if they felt it was a word, and to place a 'no' after it if they felt it was not a word.

Results and discussion. Table I contains the polarity ratings obtained for the 30 concepts based on the seven-scale semantic differential used and the frequency of '*yes*' responses for the judgements of fittingness and wordness. A product moment correlation, *r*, of 0.75 ($p < 0.0005$) was obtained between the polarity ratings obtained in the present study and those obtained by Jenkins. Thus, the obtained polarity ratings are comparable to those originally scaled by Jenkins.

TABLE I

WORDNESS, FITTINGNESS, AND POLARITY RATINGS FOR 30 SELECTED CONCEPTS

Concept	Wordness	Fittingness	Polarity	Concept	Wordness	Fittingness	Polarity
argon	68	16	2.01	heavy	94	62	1.39
bad	95	56	3.03	kitchen	96	23	2.71
beautiful	96	54	3.71	lady	92	35	2.86
bodkin	32	14	1.46	lemur	56	24	1.00
clean	96	63	2.86	lizard	92	52	1.70
cold	97	61	1.83	mild	96	77	2.58
delayed	94	35	1.66	music	93	42	2.94
doctor	91	29	2.66	pliable	95	71	2.52
dreary	97	84	2.73	pretty	92	49	3.45
elevated	95	44	1.76	shady	94	54	2.15
fat	95	55	3.06	short	96	58	1.13
fervid	56	38	2.49	stiff	97	80	2.39
fleecy	92	83	3.39	symphony	96	56	2.95
god	94	39	3.25	trees	94	28	2.12
gojey	5	15	1.08	wagon	95	20	2.92

The *r* obtained between polarity and fittingness was only 0.31 which is not significant with an *N* of 30. With Wertheimer's results in mind, it is reasonable to conclude that there is some small positive relation between the fittingness of a concept and its polarity, but the relationship is not very pronounced.

Although the concepts used in the study were distributed fairly evenly over the range of polarity-values, the obtained values of wordness were skewed with 25 of the 30 values being above 90%. Thus, rho, the rank order coefficient of correlation was deemed a more appropriate statistic than the product moment correlation to use with the distribution of wordness scores. The rho obtained between wordness and fittingness was 0.65 ($p < 0.001$). Thus, fittingness seems to be related to wordness. Concept names judged as words are perceived to fit their meanings better than concepts not consistently judged as words.

The value of rho obtained between polarity and wordness was 0.26 which is not significant. The values of wordness were classified further, however, into concepts rated 90% or above and concepts rated below 90%. The biserial correlation between these categories of wordness and the distribution of polarity scores was 0.71 ($p < 0.01$) indicating a gross relation between wordness and polarity.

These relationships indicate that polarity probably is related to fittingness but only with regard to gross divisions between high and low polarity concepts. Differentiation among meaningful concepts probably does not relate to fittingness, but differentiation between meaningful concepts and relatively meaningless concepts does differentiate between fitting and nonfitting names of concepts.

SUMMARY

Ratings of the fittingness of 30 selected concept names to their referents were obtained from 100 *S*s, who also rated whether these same concepts were words. Polarity scores based on a seven-scale semantic differential were obtained from 31 of these *S*s for the 30 concepts. Polarity did not correlate significantly with fittingness ($r = 0.31$). Fittingness did correlate significantly with wordness (rho = 0.65) indicating that concept-names judged as words are perceived as fitting their referents better than concept-names not consistently judged as words.

APPARATUS

A PHOTOVOLTAIC PLETHYSMOGRAPH CALIBRATED FOR TISSUE-DENSITY

By ROY J. KRUSBERG and HERBERT ZIMMER
University of Georgia

Digital blood flow as an index of peripheral vasometer activity has been of considerable interest to many investigators, because of its sensitivity to systemic and environmental influences. Three major methods have been employed to derive an index of blood flow from the intact human organism:

(1) Estimates of blood flow were originally based on volumetric changes in the displacement of air or water from a rigid vessel enclosing a limb. This method is cumbersome, time-consuming, immobilizes the *S*, and imposes some degree of discomfort on him.

(2) More recently, changes in body-impedance, secondary to blood flow and other volume changes, have been detected by an A.C.-coupled impedance-plethysmograph, which is subject to artifacts of movement, and which projects radio frequencies to other transducers.

(3) The photoplethysmograph does not impose any discomfort on the *S*, nor does it radiate interference to other transducers. The photoresistive cells, which have been commonly employed so far, are subject, however, to a substantial memory for light, which has a marked effect on their subsequent resistance, and usually requires an A.C.-coupled amplifier. Both of these factors limit such cells to the recording of relative changes in light transmittance, and rule out the possibility of calibrating them even in an interval-scale.

A detailed discussion of the relative merits of these three methods of plethysmography, together with illustrations, may be found in Brown, Giddon, and Dean's critical review.[1] Since the present paper proposes a new and different solution to the problem of plethysmography, it is not found in their review.

The limitations inherent in existing methods demanded a new approach

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* This study was conducted in the Bioelectronic Computer Laboratory of the University of Georgia, and supported by the Air Force Office of Scientific Research, Grant AF-AFOSR-257-65, and the Rome Air Development Center, Contract AF 30(602)-3380.

[1] C. C. Brown, D. B. Giddon, and E. D. Dean, Techniques of plethysmography, *Psychophysiology*, 1, 1965, 253-266.

to the problem. The photovoltaic cell appeared to have the desired stability, linearity, and lack of memory to make it suitable for calibration, if sufficient sensitivity could be derived from its very low voltage-output without introducing undue distortions in the cardiovascular phenomena under observation. Subsequent work with various photovoltaic cells confirmed their initial promise, leading finally to the design of a transducer which met the requirements demanded of it:

(1) It is calibrated in a derived ratio-scale with an absolute zero value: units of optical density of tissue $D = \log_{10}L/L$, which range from 0 to 5. D designates optical density, L is defined as the total illumination on the surface of the tissue, while L' is that fraction of the total illumination which remains after being transmitted through the tissue.

(2) Its voltage output is a linear function of tissue-density, and remains directly proportional to it at any time. Unlike photoresistive cells, its output is unaffected by previous levels of illumination.

(3) It tracks changes in tissue-density with a response-time of less than 20 µsec., which is fast enough to reproduce most physiologic wave-forms quite accurately.

(4) It does not modify the physiologic parameters under study, since it does not restrict the flow of the blood, is chemically inert, and maintains a temperature of less than 31°C. over a period of 6 hr. at the point of contact between the source of illumination and the skin.

(5) It operates on D.C., and is, therefore, not a source of radiational interference for other transducers and instruments.

(6) It has little or no influence on the reactions of the *S*s, since it can be worn for hours without creating discomfort.

(7) It is suitable for telemetry applications, since it does not interfere with the *S*'s freedom of movement, and introduces negligible movement-artifacts.

(8) It can be expected to retain its initial performance-characteristics through many years of use, since its components are exceptionally durable and are protected against ordinary mechanical and chemical damage by being hermetically sealed in epoxy resin.

(9) Its output (20v.) makes this transducer with its amplifiers compatible with most recording systems, and provides for a high resolution of the wave-forms of the pulse.

The transducer consists of a Solar Systems 404A2200LC silicon solar cell and a number 345 tungsten lamp, embedded in a block of solid epoxy resin, measuring 21 × 13 × 14 mm., which has been rendered opaque by the inclusion of finely powdered lamp black, except for two translucent areas covering the lamp and the solar cell. The 5 × 5 mm. solar cell is centered on the 21 × 13 mm. surface of the block, sunk 0.5 mm. beneath it, and covered with clear epoxy resin. A cylinder, 6 mm. in diameter, with its circumference 2 mm. from the solar cell at its closest point, encloses the lamp. A tungsten lamp, with a color temperature of about 2840° K, was chosen, since its spectral energy distribution between 400 and 1200 mµ covers the area of maximal sensitivity of the solar cell, thereby eliciting the most efficient response from the cell which then yields maximal voltage-output. The lamp is energized by 6-v. D.C., and draws 0.04 Å. The mode of attaching the transducer to the organism varies with the aspect of cardiovascular system which is to be monitored. Since the voltage- and illumination-levels of the lamp are held constant, all changes in reflected light impinging on the photovoltaic cell represent changes in the optical density or light-transmittance of the tissue. Fluctuations in line voltage are corrected by an automatic voltage-regulator to within 0.25% of the total voltage.

The selection of a silicon solar cell over cadmium sulfide or cadmium selenide cells for this application was determined by (1) its lack of light memory, which results in stable, consistent and repeatable density measurements at all times; (2) its very fast response-time, between 5 and 20 µsec., which is far in excess of any observed tissue-density changes, and therefore should provide accurate records of them; and (3) a low D.C.-voltage-output, which is compatible with the input-level requirements of high-gain, D.C.-coupled amplifiers. Light from a number 345 tungsten lamp transmitted through a stepped film tablet, which is calibrated in units of optical density, was used to determine the equivalent voltage-output from the silicon solar cell produced by the gradated steps of optical density. The resulting calibration curve is shown in Fig. 1. To obtain the largest number of calibated steps, the stepped film tablet should cover primarily the operating range of the transducer, rather than the complete spectrum of optical density. A tablet with over-all dimensions of 8 × 10 in., with an optical density range from 0.90 to 1.65, will yield 16 steps of 12.70 mm. length, in increments of 0.05 optical density units, ± 1% of each step. Such a tablet can be obtained from a producer of photographic equipment and films. The photovoltaic cell in conjunction with two D.C.-coupled amplifiers supplies a total

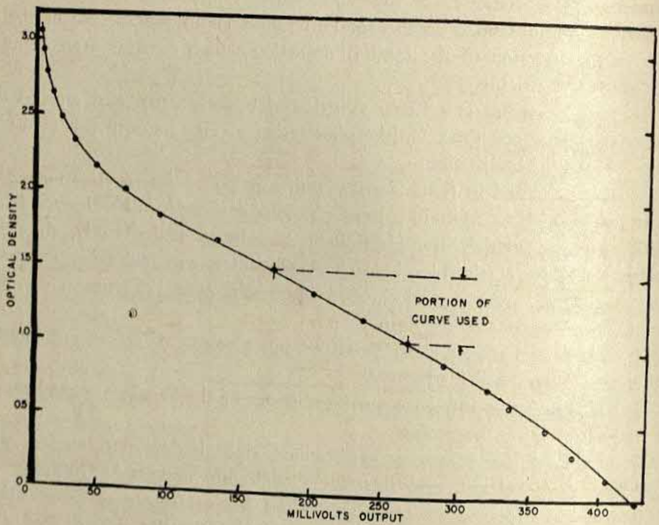

FIG. 1. OPTICAL DENSITY OF TISSUE CALIBRATED IN VOLTAGE-EQUIVALENTS

density reading of the tissue, as a baseline and point of reference, with the transient vasomotor changes superimposed on it. In this manner, a record is obtained, not only of the successive pulse-waves, but also of the slower shifts in vasodilation and constriction, always in terms of the optical density of tissue. The main disadvantage of photovoltaic cells is their low voltage-output, in this case about 100 mv., which necessitates careful shielding of the transducer and the *S* against noise, since considerable amplification is required. The cell-output is loaded by an 18 KΩ resistor, to obtain a near maximal voltage-output, which is linear with respect to light input. The choice of an 18 KΩ resistor also places the cell in an operating range within which temperature-fluctuations have the smallest influence on voltage-output. Measurements of *S*s with average melanin-distribution on the volar surface of the ungual joint of the dextral, middle digit yield readings of optical density between

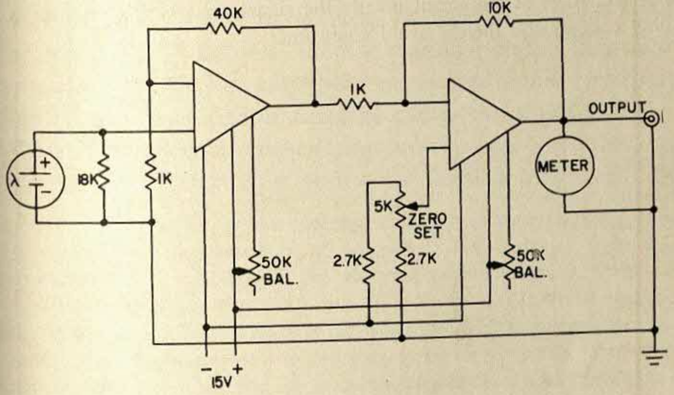

FIG. 2. DIAGRAM OF CIRCUIT FOR PHOTOVOLTAIC PLETHYSMOGRAPH

1.05 and 1.55; a portion of the curve for which a linear relationship between density and photovoltaic output obtains.

The computer-system for which this transducer was designed requires an input of ±10 v., or a range of 20 v. This requirement is met by using two operational amplifiers in series, to produce a total gain of 400. While the needed gain could be obtained from a single amplifier, better stability, accuracy, and regulation can be expected from the modest gain of each stage of amplification, since the appropriate input and feedback resistors represent moderate values and have impedance-characteristics which match transistor amplifiers. Furthermore, changes in open-loop gain of the amplifiers with age and other degradation will have less effect on the total system gain, when the amplification derived from each stage is low. The photovoltaic cell with its 18 KΩ load resistor is connected to the positive input of the first amplifier, which provides an amplification-factor of 40. The negative input is connected to the common bus through a 1000 Ω resistor, and to the amplifier output through a 40 KΩ resistor. Therefore, the feedback-voltage from the amplifier-output to the summing point will be one-foretieth of the output-voltage when the negative and positive inputs are at equal potential, resulting in an amplification-factor of 40. The second stage of amplification is accomplished with an identical operational amplifier, which (1) inverts the signal, and (2) provides an additional gain of 10 through a 10 KΩ resistor in its feedback network. The second stage is equipped with a zero-set control to facilitate the placement of the output-voltage exactly within the range required by the computer-system, which records differences as small as a 0.0001 optical density-unit. Such accuracy is five times that warranted by the approximately 0.1% accuracy of the transducer and its amplifiers. The transducer is equally useful with other recording devices of more limited resolving power.

The voltage-output of the photovoltaic cell is greatest when the light transmitted from the lamp is at its peak, which coincides with the end of the diastolic portion of the pulse-pressure wave. Likewise, voltage-output is at a minimum when the systolic portion of the pulse-pressure wave reaches maximum. Therefore, the waveform derived from the photovoltaic cell is upside down with respect to traditional graphic representations of the pulse-wave. For this reason, the operational amplifier in the second stage of the circuit inverts the signal to make it consonant with the prevailing custom, and thereby avoid confusion.

A SIMPLE METHOD FOR PREPARING SILVER-SILVER CHLORIDE ELECTRODES FOR RECORDING OF SKIN-POTENTIAL

By REGINALD E. QUILTER and WALTER W. SURWILLO, National Institutes of Health

Although stable, high-impedance, D.C. amplifiers in the 0.1 mv. range have made recording of the Tarchanoff effect available to most laboratories, electrodes continue to pose a problem. In order to measure accurately electrical potentials of the skin, the *bias* potential of the electrodes must be negligible at the level of amplification used and stable (relatively free from *drift*) over the period of time the electrodes are attached to a subject. While the Ag-AgCl electrode has usually been preferred over all others, numerous different types and variations have been used.

The best available, as far as bias-potential and drift-characteristics are concerned, are the Ag-AgCl sponge-electrodes of O'Connell and Tursky.[1] Referred to as "thermal-electrolytic" electrodes in the electrochemical literature, their excellent electrical characteristics are apparently due to the very large effective surface area between electrode and jelly which is afforded by the chloridized sponge.[2] Unfortunately, they are not easy to prepare outside of the chemist's laboratory. Ford and Leonard described a method of preparing Ag-AgCl disk-electrodes in which the usual solder junction between disk and stem is eliminated and replaced by a welded joint, with no flux or foreign metal present.[3] Although details concerning bias and drift were not reported, the electrodes were apparently made for use in the µv range. Special equipment and considerable practice is required to fabricate them.

The Ag-AgCl electrodes described in this communication are easy to prepare. At the same time, they have bias- and drift-characteristics that are suitable for most applications.

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* From the Gerontology Branch, Baltimore City Hospital, Baltimore, Maryland

[1] D. N. O'Connell and Bernard Tursky, Silver-silver chloride sponge electrodes for skin potential recording, this JOURNAL, 73, 1960, 302-304. For data in which electrical characteristics of Ag-AgCl sponge electrodes are compared with those of other types of electrodes, see D. N. O'Connell, Bernard Tursky, and M. T. Orne, Electrodes for the recording of skin potential, *Arch. gen. Psychiat.*, 3, 1960, 252-258.

[2] G. J. Janz and Harry Taniguchi, The silver-silver halide electrodes: Preparation, stability, reproducibility, and standard potentials in aqueous and non-aqueous media, *Chem. Rev.*, 53, 1953, 397-437.

[3] Adelbert Ford and J. L. Leonard, Techniques for recording surface bioelectric direct currents, *U. S. Navy Electronics Lab., San Diego, Calif., Res. Rep. 839*, 1958. Elimination of the solder junction and of the various kinds of paints, varnishes, and plastic coatings that have been used to cover it is the outstanding feature of the Ford-Leonard electrodes.

Construction of electrode. Each electrode was formed from a piece of high, fine-grade silver (99.99% pure) wire, 0.020 gauge and approximately 13 in. long. The length of wire was first cleaned with very fine silicon grit paper; then approximately 9 in. were wound into a flat spiral of 6 turns. A spool-like jig, made from lucite, was used to accomplish the winding. Fig. 1 shows details of the construction of the jig as well as the manner in which the spiral was wound. The finished electrode was ⅝ in. in diameter, with a 4-in. stem that originated from its center.

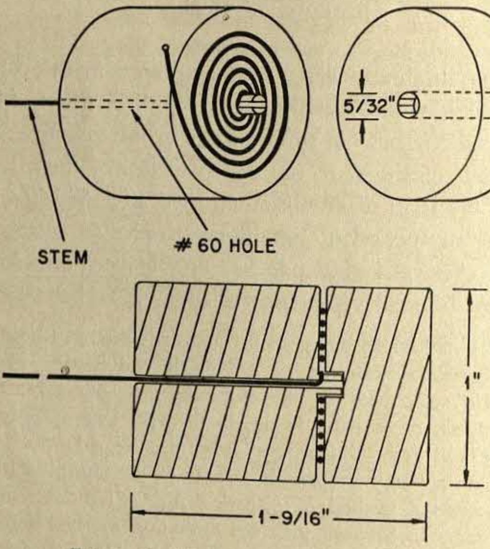

FIG. 1. JIG USED TO WIND ELECTRODES
The two halves of the jig were held tightly together and the wire was wound by hand in the slot.

Surface area of the spiral was 0.44 sq. in., which is equal to the area of one side of a disk-electrode ¾ in. in diameter. Since more turns are easily added to the spiral, the surface area of the electrode may be readily increased if desired.

After an electrode was formed, it was immediately washed with ether, then rinsed in a 1% solution of HCl, and finally placed in distilled water prior to electroplating. In all cases the distilled water used in the preparation of electrodes had been distilled in glass; all chemicals used met A.C.S. specifications.

Chloridization. Electrodes were suspended vertically by the tip of their stems in a glass cylinder that contained a 1% (by volume) aqueous solution of 12N HCl. The level of the solution was approximately 1½ in. above the spiral so that a portion of the stem could also be chlorided. With the electrodes serving as the anode and a piece of high, fine-grade silver as the cathode, a constant (electronically controlled) current of 4 ma. per electrode was allowed to flow for 90 min. To prevent formation of bubbles on the electrodes, the electrolyte was stirred continuously. The electrolytic cell was enclosed in a dust-free box that excluded all light, and great care was taken to avoid contaminating it with foreign, metal ions. The cell we used could accommodate 6 electrodes at one time.

After the electrodes were removed from the electrolytic cell they were washed with distilled water and allowed to dry in air. The chlorided portion of the stem was coated with tygon paint[4] to protect the AgCl layer from damage when the electrode was inserted into its cup. A dab of tygon was also applied to the end of the outer turn of the spiral so that the end could be anchored to the adjacent turn. It is important that the electrodes be handled only by the unchlorided portion of the stem; great care must be taken to avoid touching the chlorided spiral, either with the fingers or with any kind of tool. Electrodes were suspended vertically by the tips of their stems whenever they were left to dry or were stored.

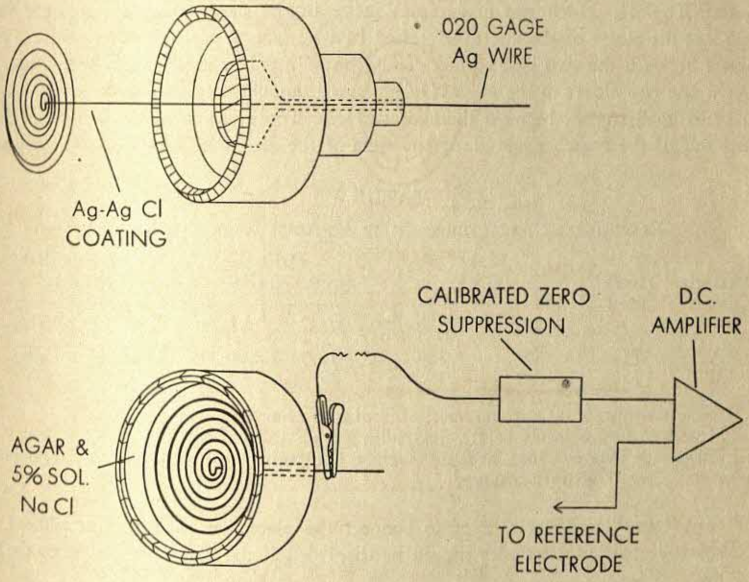

Fig. 2. Ag-AgCl Wire Electrode and Polystyrene Cup
Upper portion shows construction of the cup and the electrode, while the lower portion shows how both parts are assembled and the manner in which the electrode is connected to the D.C. amplifier.

Preparation for use. An electrode was used only in conjunction with its polystyrene cup, which held a contact medium as well as the electrode. The cups were turned from 1 in. round stock and had a chamber that was ¾ in. in diameter and 9/32 in. deep. A hole, #60 gauge, was drilled through the center to allow the stem of the electrode to pass through. The other dimensions are not critical; they depend on the particular method that is used to attach the cups with electrodes to the *S*.

When the electrode had been placed carefully into its cup, the chamber was completely filled with an agar paste that was made by mixing together approximately 2 gr. of agar and 10 cc. of a 5% (by volume) aqueous solution of NaCl. Care should be taken that the paste is not too thin, since the NaCl solution then has a tendency to seep out of the chamber and along the stem of the electrode. Fig. 2 shows an electrode and its polystyrene cup, and the manner in which both were assembled.

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[4] Obtained from United States Stoneware Co., Akron, Ohio.

To check a pair of electrodes prior to use in a test, the two cups with their electrodes were placed in a jig which allowed the agar surfaces to be brought into contact. The jig, which was made from lucite in the shape of a 'U,' so held the cups that the stems of the electrodes rested in a horizontal position. The pressure of contact between the two agar surfaces could be adjusted by means of a screw which moved one cup closer to the other. Only a small amount of pressure is necessary to maintain good contact between the agar surfaces. Too much pressure will force the paste out of the chamber and along the stem of the electrode. Miniature-type alligator clips (see Fig. 2) were used to connect the electrodes to a D.C. amplifier. A freshly-made pair of electrodes should be discarded if the bias potential exceeds ± 0.1 mv.

TABLE I

ELECTRICAL CHARACTERISTICS OF AG-AGCL WIRE ELECTRODES

Electrode pair	Times* used	Bias† (m.v.)		Drift (m.v./20 min.)		Change in Bias‡ (m.v.)	
		M	*SD*	*M*	*SD*	*M*	*SD*
A	71	.20	.15	.05	.06	.23	.21
B	75	.10	.10	.05	.05	.37	.35

* Each use represents approximately 90 min. of recording.

† Measured immediately before electrodes were attached to *S*.

‡ Difference between bias measured before electrodes were attached to *S* and bias measured after they were removed.

After a pair of electrodes has been used, the electrodes should be removed from their cups, washed in distilled water, and stored by suspending them vertically in a 1% aqueous solution of NaCl. Care must be taken that the level of this solution does not rise over the portion of the stem that is coated with tygon. Each pair of electrodes should be shorted together and the glass vessel with the NaCl solution stored in the dark.

If the method is followed carefully, Ag-AgCl wire electrodes with bias potentials less than 0.10 mv. (absolute value) are readily obtained. Of greater importance, however, is the stability of this value over-time, and over the successive times that the electrodes are used. Long-term electrical characteristics of two pairs of electrodes made in this laboratory are shown in Table I. Pair 'A' in Table I was used for 28 weeks until one of the electrodes developed a crack in the chlorided surface. Pair 'B' remained in service for 39 weeks, when a large (nearly 2.0 mv.) bias potential suddenly developed and the electrodes had to be discarded.

O'Connell *et al.* have tested the Ag-AgCl sponge, the ordinary Ag-AgCl disk, and the chlorided *EEG* electrodes, and have listed their electrical characteristics.[5] In terms of bias and drift, the Ag-AgCl wire-electrodes described are far better than ordinary Ag-AgCl disk or chlorided EEG electrodes. They also compare favorably with the Ag-AgCl sponge electrodes.

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[5] O'Connell, Tursky, and Orne, 1960, *op. cit.*, 254-255.

A SIXTEEN-UNIT PROJECTOR WITH MICROSECOND TURN-ON TIME

By KENNETH C. MYLREA, University of Michigan

Visual stimuli produced by the projection of 35-mm. slides often are used because of the versatility offered in selecting stimulus-materials. The projector described here can select and project up to 16 slides, individually or in combination, in less than 1 microsec. The system is composed of 16 individually-controlled rear-projection units with a common 2 1/2 in. screen-area (see Fig. 1).[1]

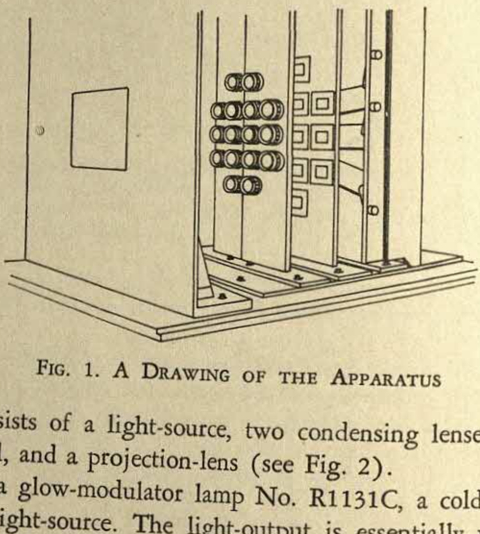

FIG. 1. A DRAWING OF THE APPARATUS

Each unit consists of a light-source, two condensing lenses, the slide, a filter if desired, and a projection-lens (see Fig. 2).

The Sylvania glow-modulator lamp No. R1131C, a cold-cathode tube, is used as a light-source. The light-output is essentially white, and its intensity varies linearly with tube-current. The light leaving the glow-modulator tube has approximately a 60° spread. Two unsymmetrical condensing lenses,[1] placed together 2 1/2 in. from the lamp, produce a cone

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* The design and construction of this apparatus were made possible by Contract No. AF 49(638)-1235 with the Human Performance Center. The writer is indebted to the late Professor Paul M. Fitts, who was director of the Center and for whom the unit was built, to Dr. Matthew Alpern of the University of Michigan, and to J. A. Van den Broek of Conductron Corporation, Ann Arbor, for their valuable suggestions.

[1] Diameter 2 in., focal-length 3.5 in.

of light which covers the slide and reaches a focal point at the projection-lens. The light-source and condensing lenses are held in telescoping concentric aluminum tubes. Adjusting the distance from the lamp to the condensing lenses enables the focal point to be located in the center of the projection-lens. To permit precise adjustment of the source and direction of the light-beam, the aluminum tube is fastened through a swivel to the framework.

The slide is set as closely as possible to the condensing lenses to take advantage of the larger light-area at this point. Transparencies are mounted in standard 35-mm. mounts with the 24-mm. sq. area in the center being used for projection. The slides are arranged in four columns within an 11-in. circle and held in position against an aluminum plate by spring-clips. Screwdriver-adjustments allow the slide to be tilted, or moved vertically or horizontally, and make possible registration of two or more slides. The slides must be placed parallel to the screen to minimize distortion.

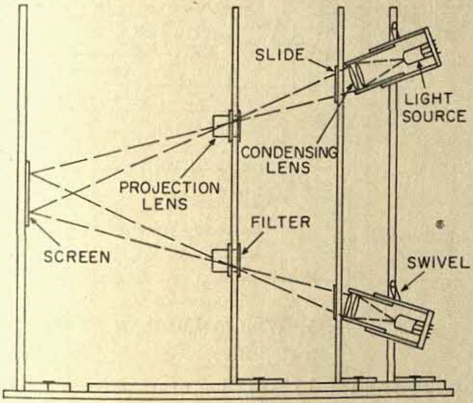

FIG. 2. SIDE-VIEW OF THE APPARATUS

The projection-lens is placed approximately 4 in. from the slide and is fastened to an aluminum plate by a mounting ring.[2] The ring-to-plate distance is adjustable to permit compensation for the different lengths of light-path corresponding to the various angles of projection. To produce uniform focusing over the entire image, it is necessary to have the lens parallel to the screen. Spring clips are attached on the opposite side of the aluminum plate to hold filters in position.

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[2] A 90-mm., 2.25 × 2.25 anastigmatic projection-lens was selected to give the necessary half-angle with a reasonable projection-distance.

Polacoat LS60 lenscreen was selected for screen-material. It has a half-angle of 22° and a gain of 200% as compared to a matte-white reflective surface. The maximal viewing angle from the projector is 20°, which would produce a difference in brightness between slides of less than 2:1; this variation can be corrected by adjusting tube-currents. This maximal variation in brightness across a single slide is 1.4:1, well within the 3:1 ratio given as a tolerable nonuniformity for larger projection-systems.[3] The projection-distance can be adjusted slightly to obtain sharp focus.

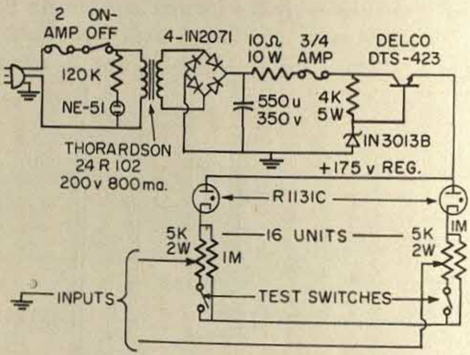

FIG. 3. WIRING DIAGRAM

Power is supplied from a 175-v., 1-amp., zener-regulated supply (see Fig. 3). Each lamp is maintained in a partially-on state by a continuous 10 μamp. of current flowing through the 1-MΩ resistor. This practice serves a twofold purpose: it reduces transients produced when the current is suddenly increased to 50 ma. for full brightness, and it alleviates the necessity for a high starting-voltage by maintaining the tubes in a fired state. The 50 ma. of current can quickly cause overheating, which significantly decreases tube-life. For this reason, it is recommended that on-times of less than 1 sec. be used.

Sixteen parallel inputs provide individual control for the projectors. A low-resistance path is completed through the appropriate tube when an input is grounded. The rise- and decay-times of the lamp are less than 1 μsec. The turn-on and turn-off times of the projector therefore can be less than 1 μsec., but will be determined essentially by the external circuitry used to switch the inputs. For preliminary set-up and registration, adjustment-switches are provided to activate each of the projectors at a low current level (20 ma.). In this mode, there is sufficient light for initial set up but the current is small enough to be maintained without causing significant tube deterioration.

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[3] Vlahos Petro, Selection and specification of rear-projection screens, *Soc. Mot. Pict. Telev. Engin.*, 70, 1961, 89-95.

The complete assembly consists of two eight-projector units which can be separated to produce two projection-areas on the screen. This feature also facilitates changing the center slides. The two sub-assemblies and the front plate which holds the 4 × 8-in screen are fastened to a common aluminum base. A finished appearance is provided by a plywood covering which fits over the unit.

TACHISTOSCOPE FOR X-RAYS

By JOHN GARCIA, Harvard Medical School,
J. R. SCHOFIELD and DON OPER, VA Hospital
Long Beach, California

This apparatus was designed to deliver extremely brief x-ray doses to serve as stimuli or signals during behavioral tests with laboratory rats. Since the purpose of these tests was to determine whether rats could detect radiation via sensory receptor or an immediate physiological effect of the exposure, the major technical difficulty lies in necessity to expose the animal without visual or auditory cues which could inadvertently elicit a false response. This apparatus represents a solution to the problem which is inexpensive, portable and does not require modification of the x-ray machine. Responses to 20 milliroentgens delivered in 0.1 sec. have been demonstrated using this device.[1]

The apparatus (Fig. 1) consists of three components: (A) First a moveable shutter constructed of plywood and ⅛ in lead (A) which is suspended by nylon cords from the ceiling so that it "closes" the beam when it hangs at rest. In our laboratory this shutter was manually "opened" from outside the x-ray room by means of another cord via a baffled opening in the wall. Under this arrangement the x-ray machine can remain in constant operation during the experimental session and the exposure can be administered without the usual on-off sounds of the x-ray machine.

(B) The second component (B), a "tachistoscope" or circular shutter makes it possible to time the exposure much more precisely than with the suspended shutter alone. This is essentially a table-like ¾-in. plywood disk 48 in. in diameter bordered by 8 in. of ⅛ lead which has an adjustable slot to permit exposure of the animal. A ¼ hp. electric motor connected to a variable-speed gear box transmits torque via a belt drive and worm and gear box to the circular shutter which is mounted in the horizontal plane on a 31-in. pedestal.

(C) The third component is (C), a device to detect radiation, which consists of a photocell, a D.C. bridge circuit, and an amplifier. The photo-cell is covered with a fluorescent material which glows under the influence of x-radiation. This luminescence is detected by the photo-cell which in turn causes the bridge circuit to unbalance. A photo-cell with a peak spectral response at 5500 Å was selected in an attempt for optimal sensitivity as the fluorescent material (Depont B-2 fluoroscopic screen) gives a yellow-green light. Tests of the system using visible light showed a response lag of less than 0.5 m.sec. with a maximal response at less than 1 m.sec. The signal was recorded on one channel of an Offner model R dynograph. As our x-ray machine is pulsed at the rate of 60 pulses per second, the output is an alternating signal at 60 cycles per sec. Thus, it is possible to measure exposure-time by counting the peaks on the record, as each peak represents one-sixtieth of a second.

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[1] John Garcia, N. A. Buchwald, C. D. Hull, and R. A. Koelling, Adaptive responses to ionizing radiation, *Bol. Inst. Estud. méd. Biol. Méx.*, 22, 1964, 101-113.

In our experiments, the circular shutter was rotated without interruption at a constant speed during the entire experimental session to eliminate 'off'-'on' sounds. For brief exposures, we employed a speed of 20 r.p.m. which allowed ample time to open and close the suspended shutter during one revolution, thus allowing a single exposure through the slot in the episcotister. A wide range of exposures can be precisely administered by adjusting the size of the slot in the episcotister as well as the r.p.m. Flickering x-ray can also be administered when long stimulation periods of high intensity are required, but the lethal effects of high total doses must be avoided.

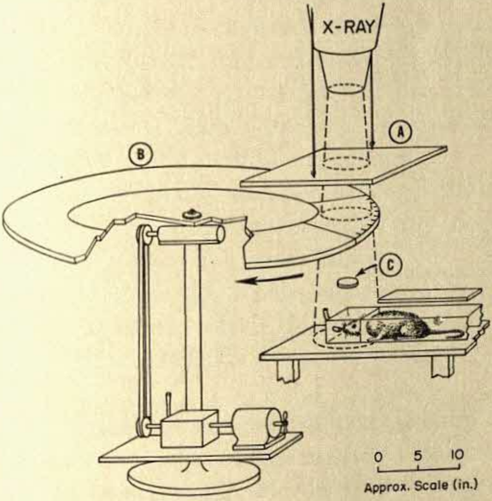

FIG. 1. DIAGRAM OF THE X-RAY TACHISTOSCOPE

We employed some further precautions to eliminate stimulus-artifacts. The animals were tested in an acoustically and electrically shielded compartment equipped with a speaker which emitted a "white" noise masking sound. Thirsty animals were trained to enter a narrow tunnel to obtain water, thus confining themselves to the area of the beam as illustrated in Fig. 1.[2] Lapping at the water spout was recorded electronically with a drinkometer.[3] A final precaution is a sham irradiation-test, which is identical to an actual exposure except that the copper filter in the x-ray machine was replaced with a lead plate to block the x-ray. Operation of all the apparatus under these conditions will reveal if a stimulus-artifact is operating as a signal to produce false responses.

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[2] John Garcia, N. A. Buchwald, B. H. Feder, R. A. Koelling, and L. F. Tedrow, Ionizing radiation as a perceptual and aversive stimulus: I. Instrumental conditioning

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studies, in T. J. Haley and R. S. Snyder (eds.), *Response of the Nervous System to Ionizing Radiation; Second International Symposium.* 1964, 673-686.

[3] Eliot Stellar and J. H. Hill, The rat's of drinking as a function of water deprivation, *J. comp. Physiol. Psychol.*, 45, 1952, 96-102.

APPARATUS NOTES

A MINIATURE MULTI-LEAD ELECTRODE PEDESTAL FOR SMALL ANIMALS

None of the various methods of implanting and connecting electrodes in small animals is at once simple to construct, small in size, and easy to implant and connect. The electrode-pedestal assembly described by Valenstein, Hodos, and Stein requires many steps to build and the utilization of special equipment.[1] Moreover, each assembly is so big that two of them cannot be placed closer than 4.5 mm. to one another.

The cannula-electrode apparatus discussed by Vogel and Carlton, while a good cannula, suffers from the disadvantage that only monopolar recordings are possible with it.[2] Also, it is rather bulky.

Bull and Collins have noted that bipolar electrodes may be cast in cold-curing acrylic in sipping straw molds.[3] However, the system does not use a good connecting device (alligator clips are required) and it too, is relatively large.

Recently, a "wire-bridge" technique has been described by Radloff.[4] Each end of a stainless steel wire (0.015 in. diam.) is carefully cemented into a hole drilled at a sharp angle into the skull to form a wire bridge between the two holes. Implemented electrodes are then supported by flowing the acrylic over to the bridge. A connecting device must then be soldered to the electrodes and cemented to the bridge.

Because these assemblies are too massive, they tend to be unwieldy, and some kind of anchoring is necessary: thus, the problem is compounded. This disadvantage may be avoided by the method described here.

Ordinary, side-by-side, stainless steel (0.007 in. diam.), bipolar electrodes, and a miniature connector formed by cutting a large connector across its width with a jig saw, are used. The continental 26-mm. connector, a 26-pin connector often used in implanting cats, is easily divided into four compact 5-pin connectors and one 3-pin connector, or two 7-pin connectors and one 6-pin connector. In this manner small female connectors may be made with as many pins as desired. Similar crosswise cuts are made in the male half of the connector, fine insulated wire soldered to the pins, and the wires attached to the recording or stimulating equipment.

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[1] E. S. Valenstein, William Hodos, and Larry Stein, A simplified electrode-assembly for implanting chronic electrodes in the brains of small animals, this JOURNAL, 74, 1961, 125-128.

[2] John R. Vogel and Peter L. Carlton, A simple and inexpensive chronic cannula for intracerebral injections into small animals, this JOURNAL, 77, 1964, 647-649.

[3] John A. Bull, III, and Thomas B. Collins, A simple procedure for making small electrodes, this JOURNAL, 78, 1965, 676.

[4] W. P. Radloff, Stereotactic implantation for small animals: A "wire-bridge" technique, *Psychon. Sci.*, 4, 1966, 47.

Implanting and wiring is conventional, except that no anchor screws are tapped into the calvarium. A bipolar electrode, cut to the requisite length beforehand, is implanted through a *small* hole (size eight or nine dental burr) and held in place with a small quantity of cranioplastic cement. After all electrodes have been implanted, each is soldered to a pin on the female connector which is then bonded to the calvarium with acrylic cement. The acrylic should be flowed thinly to the perimeter of the calvarium to provide a wide gripping surface. The incision should then be sutured. With care, the seven-pin pedestal will occupy little more volume than that consumed by the eraser on a new pencil. Electrodes can be placed in the same structure, if desired, less than a millimeter apart.

These pedestals have been cemented to the calvariums of over 50 rats and pigeons, where they have remained solidly affixed for many months. The tension of the pins in their sockets holds the male and female sections together: if too tight the sockets may be enlarged with a small pin-file. This assembly eliminates the necessity for anchor screws, reduces surgical trauma, provides a reasonable number of electrodes, and requires little special manufacturing. It is highly suitable for chronic preparations in rats, pigeons, hamsters, and other small animals.

North Dakota State University A. JOSEPH RAY, JR.

A PEN FOR MEASURING RESPONSE-LATENCIES

An electrowriter transmitter-pen, normally used in conjunction with a transmitter and receiver for interoffice communication, provides an effective means of measuring the response-latencies of a number of *S*s simultaneously.[1] Each *S* responds by writing with a transmitter-pen. Pressure on the ball-point of the pen activates a switch in the pen which in turn can be used to stop a chronoscope or a counter. These reliable pens have several advantages over voice-activated relays. Not only are they less expensive, but they make it unnecessary for *E* to record the content of *S*'s response.

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[1] The pens are sold by the Victor Business Machines Co., 3900 North Rockwell Street, Chicago, Illinois.

State University College, GEORGE R. MARSHALL
Plattsburgh, New York LARRY CARLSEN

A SIMPLE METHOD OF RESTRAINING RATS FOR *EEG*-RECORDING

The apparatus, shown in Fig. 1, consists of a *T*-shaped plastic platform elevated on metal rods 6 in. above a stainless-steel base. Three leather straps inserted into slits in the stem of the *T* are used to secure the animal, its legs hanging over the sides of the stem in the spaces between the straps. The principal advantage of this apparatus is that the rat can be secured easily and quickly. After a period of adaptation to restraint, the animal remains relatively motionless. There is, however, sporadic struggling that diminishes in frequency from one session to the next.

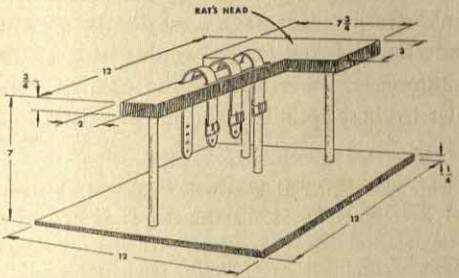

FIG. 1. A SKETCH OF THE RESTRAINING TABLE
(All dimensions are given in inches.)

Indiana University (Kokomo) MELVIN L. GOLDSTEIN

NOTES AND DISCUSSIONS

LIGHT SENSED THROUGH RECEPTORS IN THE SKIN

In recent years, a number of claims have arisen regarding the ability of man to perceive various colors, objects, and printed words via the skin. Youtz recently reports that a subject was able to discriminate and to identify colored materials with her hands under lighted or dark conditions.[1] Subsequent testings, however, were less successful.[2] Youtz attributes such an ability to an extension or variation of the temperature-sense in the skin. While some Soviet investigators would share Youtz's view of the dermal temperature-sense, others are of the opinion that light sensitive substances similar to those found in the retina exist and are spread over the skin.[3]

Supporting evidence of a dermal light-sense of any sort, though, is scarce and unconvincing. Oppel and Hardy found that human *S*s could sense dermally visible light.[4] Serious methodological defects, however, such as failure to exercise any of the controls in the psychophysical *method of limits*, render their findings suspect. These authors also neglected to report the data obtained on all *S*s. Only the results of a few *S*s were reported; these were said to be characteristic of the entire group tested. In a different sort of investigation, it is reported by Pick that Leontiev, using light as a conditioned stimulus on the hand of a single blindfolded human *S*, obtained conditioning with electric shock as the unconditioned stimulus.[5] Since, however, Leontiev's original study is unavailable to this writer, evaluation of this research is not possible.

Such a notion as the existence of light-sensors in the skin is not beyond credulity, however, when one considers that many eyeless or blinded animals are sensitive to illumination of their body surfaces and respond to such stimulation in a variety of ways. According to Steven, a dermal light reaction is found in every major phylum of metazoa, in some aquatic and amphibious organisms, perhaps in the lizard and octopus, and rarely in terrestrial anthropods.[6] In any case, our

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* This study was supported by an NICHHD grant, number HD00914-03S1. My thanks are due Drs. A. L. Diamond, Charles E. Osgood and Joseph Church for their assistance.

[1] R. P. Youtz, Aphotic digital color sensing, *Amer. Psychologist*, 19, 1964, 734.

[2] Youtz, The case for skin sensitivity to color, Reported at the Psychonomic Society, Oct. 9, 1964.

[3] N. D. Nyuberg, "Sight" is in the fingers, *Prirods* (Nature), Moscow, May 1965 (no. 5), 61-67.

[4] T. W. Oppel and J. D. Hardy, Studies in temperature sensation: (1) A comparison of the sensation produced by infra-red and visible radiation, *J. clin. Invest.*, 16, 1937, 517-540.

[5] H. L. Pick, Jr., Perception in Soviet psychology, *Psychol. Bull.*, 62, 1964, 21-35.

[6] D. M. Steven, The dermal light sense, *Biol. Rev.*, Cambridge, England, 38, 1963, 204-240.

knowledge of the structures in the human skin is meager enough that it is surely indefensible to declare categorically that none of the receptors could be sensitive to light.[7]

The present investigation attempts to assess how prevalent, if at all, is the ability of humans to discriminate light from darkness with their hands and arms. A discriminative learning situation is employed toward this end.

METHOD

Subjects. One hundred *Ss* were obtained from introductory courses in psychology at the University of Hawaii. All were volunteers.

Apparatus. *S* was seated in one room (*S*-room), *E* and his testing apparatus were located in an adjoining room (*E*-room). Two 6-in.-sq. holes were cut through the common partition. In the *S*-room black velvet sleeves were so attached to each of the openings that they were light-tight. In the *E*-room a Jewelex projector with a 300-w. bulb was placed in a closed black box. A vessel 2.5 in. × 1.3 in. × 8.5 in. high filled with water was placed inside the box between the projector's lens and the box's aperture to serve as an infrared and ultra-violet ray filter. The box had an aperture of the same diameter as that of the projector's lens. A black velvet curtain was attached to the box in such a way that it could be silently raised or lowered over the aperture with a rod to control the introduction of light. The curtain covered the aperture keeping the *E*-room in darkness at all times, except when it was raised for the *light* condition. The projector fan and light were both kept on throughout the entire experiment so that the little noise which they produced was kept constant for all conditions.

E used a Hunter Timer to measure 15-sec. trials. A device was so arranged that when *E* pressed a key, the timer would begin its measurement and also a click would be heard by *S* in the next room by means of a small 2-in. speaker. At the end of 15 sec. the timer would shut off automatically and generate another click. *E* measured a 5-sec. rest interval between trials with a stop-watch. A bell was operated manually by *E* to provide reinforcement. *E* used a tiny shielded pen-light to see his response sheets and the apparatus which he had to operate.

Procedure. The *Ss*, while in an anteroom, were given a magazine clipping to read which described the feats of a Russian who was able to 'see' through her fingertips, as well as a set of instructions regarding the experiment. *S* was afterwards brought into the *S*-room and seated by the sleeves in the wall. Chair height was so adjusted that when *S* put his arms through the sleeves, his elbows were resting comfortably on the counter on *E*'s side of the partition. *E* so positioned *S* that his arms stood upward on his elbows, about 1 in. from the wall and parallel to it on *E*'s side. *S*'s hands were bent slightly downward toward *E*. Light thus struck the forearms, palms and fingertips at approximately right angles. All jewelry was removed from *S*'s hands. *E* blindfolded *S*, gave him instructions, and then retired to the *E*-room. The lights in the *S*-room were left on, to mask any occurrence of a light leak through the blindfold or the sleeves.

In the *E*-room the projection of light was so adjusted that the most intense light would center about the *S*'s palms. A few clicks (indicating the beginning or ending of a trial) and bell sounds (reinforcement) were produced as samples for *S*. *E* then presented 40 trials, 20 *light* and 20 *dark* in random order. (The *Ss* had been instructed that the *light* and *dark* trials were placed in *random* order.) Each trial was 15 sec. long, after which *S* was obliged to give a response of either "light" or "dark." If *S*'s identification was correct, *E* sounded a bell; if incorrect, there was silence. A rest-interval of 5 sec. followed each trial. During the rest interval the curtain covered the light-source aperture and there was darkness in the *E*-room. Total test-time was about 14 min. After 40 test-trials, each *S* was briefly interviewed by *E*.

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[7] D. Liddle, Fingertip sight: fact or fiction? *Discovery*, Sept., 1964.

Light conditions. The projector light was displayed as a bright square on the wall. The distance to the wall from the aperture of the box was 28 in. Light readings were taken with a Welch Densichron at various distances and positions, and indicated that under the *light* condition none of the *Ss* could have received light beyond a range of from 2.56 to 1.31 log foot candles. It is estimated that all the *Ss* received between 2.42 and 2.54 log foot candles of light on their palms and fingers. Regarding the *dark* condition, however, the Densichron (with zero filters) was not able to give a reading. At maximal sensitivity, the Densichron gave a reading of −3.14 log foot-candles. This finding was not surprising to *E*, since he was unable to see *S*'s hands and arms except during the *light* trails. The minimal difference between the *light* and *dark* conditions exceeded, therefore, 4.50 log foot-candles.

Temperature-conditions. Temperature readings were taken with a Hartmenn-Braun Thermocouple at various distances and positions between the aperture of the black box and the target wall. The measuring instrument showed no increase in temperature, as compared to the *dark* condition, along the level of highest light intensity until the thermocouple approached to about 11 in. from the black box, where a rise of 0.4° F. was noted. Since *S*'s extremities did not approach within 16 in. of the light source during the experiment, according to the above temperature measures, no change of temperature between the *light* and *dark* conditions occurred. Room temperature was approximately 74° F. during testing.

RESULTS

The *Ss*' performance was scored according to the number of correct identifications made. A maximum of 40 was possible. The sum and mean score for each of four blocks of 10 trials, as well as for the total 40 trials are shown in Table I. A non-significant *F* was found in an analysis of variance for the number correct. On the other hand, a trend analysis on the four blocks of trials resulted in a significant *F* of 5.51 ($p < 0.05$) indicating a linear slope significantly different from zero. Similarly, a testing of the difference between all pairs of block means with the Duncan Multiple range test indicated a significant difference in means between the first (4.85) and fourth (5.37) blocks of trials, ($p < 0.05$).

A frequency-distribution of the scores obtained by the 100 *Ss* is shown in Table II. The scores range from 14 to 27 with a mean of 20.66. Those

Ss who obtained extremely low scores (14) or high scores (26, 27) were retested. In addition, four other *Ss*, three of whom had scores of 25 and one of 24 were also retested. The situation at retesting was identical to that of the first testing except that the order of presentation of *light* and *dark* trials was reversed. The results of the retesting is shown in Table III.

TABLE I

SUMS AND MEANS OF CORRECT JUDGMENTS FOR EACH OF THE FOUR BLOCKS OF 10 TRIALS, AND THE SUM AND MEAN OF THE ENTIRE 40 TRIALS

$(N=100)$

	Blocks				All
	1	2	3	4	
	1–10	11–20	21–30	31–40	all 40
Total	485	517	527	537	2,066
Mean	4.85	5.17	5.27	5.37	20.66

A trend analysis on blocks of trials yielded an F of 5.51, significant at the 0.05 level. The difference between the means of Blocks 1 and 4 was significant , $p<0.05$.

TABLE II

FREQUENCY DISTRIBUTION OF SCORES OF 100 *Ss*
Maximal possible score 40

Total No. Correct	14	15	16	17	18	19	20
No. of *Ss*	3	2	4	7	9	14	13
Total No. Correct	21	22	23	24	25	26	27
No. of *Ss*	6	10	9	13	6	1	3

TABLE III

ORIGINAL AND RETEST SCORES FOR SOME *Ss* WITH UNUSUALLY HIGH OR LOW SCORES

	Scores												
	Unusually low $(N=3)$			M	Unusually high $(N=8)$								M
Original	14	14	14	14	24	25	25	25	26	27	27	27	25.8
Retest	18	20	22	20	25	21	21	27	25	16	24	26	23.1

The mean of the low scorers rose from 14 to 20, while that of the high scorers dropped from 25.8 to 23.1; the mean of the three highest scorers dropped from 27 to 22.

The *S* who obtained the highest average score on both testings (27 and 26) was able to return again for four further retestings on two different days. For two of these retestings (each on separate days) the number of *light* and *dark* trials were varied unequally (10 *light* with 30 *dark*, and 30 *light* with 10 *dark*). The *S* obtained a score of 19 on both testings. Following each of the other two testings *S* was given 20 trials (10 *light* and 10 *dark* in random order) but without reinforcement. His score on the first day was 8 out of 20, and on the second, 11. On the retestings, the *Ss*' scores approximated chance.

Interview data indicated that most *Ss* said that they were definitely able to feel heat on many of the light trials, including the *Ss* who were low scorers. The 11 retest *Ss* did not differ in the nature of their comments from the other 89. The exceptionally high scorer who was tested six times in all said that he had felt "nothing special" and that he had made his judgments by "intuition."

DISCUSSION

The finding of a significant trend and also of a significant difference in means between the first and last blocks of trials would indicate that *Ss* did learn to discriminate between the *light* and *dark* conditions. The significant difference between the means of the first and last blocks of trials supports this conclusion. With regard to the retesting of the *Ss*, although the performance of the high scorers dropped on the retest, it still remained above the chance mean of 20, while that of the low scorers rose to chance.

This investigation suggests that some sort of receptors in the skin of human beings may be capable of sensing the electromagnetic radiation of the visible spectrum. The question of whether such receptors operate on an energy-temperature or retinal-type basis cannot be determined from this study. The basing of any sort of temperature hypothesis on *S*'s report would appear, however, to be unsound in view of the interview finding that most *Ss* reported feeling heat on many of the *light* trials. Such data suggests that the *Ss* had felt an imaginary heat since their performance in general was not nearly as accurate as their reports might imply.

SUMMARY

To determine whether light, appropriately controlled for infrared and ultraviolet rays, could be sensed by the hands or forearms, 100 *Ss* were tested in a discriminative learning situation. An analysis of trend was performed and a statistically significant linear trend was found. *A posteriori* testing of pairs of means showed that the difference between the first and last blocks of trials was significant. It was concluded that *Ss* learned to discriminate very slightly between *light* and *dark* conditions, and hence were able to sense light dermally.

University of Hawaii

DANNY D. STEINBERG

SUCCESSIVE TEXTURAL CONTRAST: A TACTUAL AFTER-EFFECT

This Note reports a tactual after-effect of successive contrast which parallels Locke's well-known demonstration of temperature contrast in which one hand is placed in a bowl of cold water, the other in a bowl of warm water, and then both hands are placed together in a bowl of water of neutral temperature.[1] The neutral water feels warm to the hand that was in the cold water and cool to the one that was in warm water.

A similar contrast effect—a tactual after-effect—is demonstrated with three grades of sandpaper. A coarse grade (No. 60) is inspected by the fingers of one hand, a fine grade (No. 120) by the fingers of the other hand, then the fingers of both hands are brought to bear upon two identical pieces of sandpaper (No. 80), and *O* asked to indicate which of the two pieces feels the coarser. The intermediate sample feels 'finer' to the fingers that had inspected the coarse sample and 'coarser' to the fingers that had inspected the fine sample.

This result was obtained from 15 of 16 *O*s—graduate students and faculty members of the Department of Psychology. The apparent differences in texture reported by these *O*s ranged from slight to striking.

University of Colorado
JAMES T. WALKER

THIRTY-SEVENTH ANNUAL MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

The Eastern Psychological Association held its thirty-seventh Annual Meeting April 14-16, 1966 at the Statler-Hilton Hotel in New York City. Total registration at the meeting was 3,809, including 1,661 members of the Association. The membership of EPA at the time of meeting totalled 3,965.

Virginia Staudt Sexton was chairman of local arrangements, assisted by Lawrence Zeitlin, Edward I. Gavurin, Rita Y. D'Angelo, Bernard Seidenberg, Anthony J. Summo, James Wilson, and Barbara Wilson. The Program Committee comprised L. Starling Reid, Chairman, Bernard Saper, and Patricia C. Smith. The Committee scheduled 502 papers, 13 symposia, 4 special sessions, 3 film sessions, 5 special group meetings, and 2 invited addresses.

Of the 97 paper-reading sessions, 12 were devoted to topics in physiological psychology, 16 were on sensation and perception, 21 dealt with animal-experimental, and 19 were concerned with human-experimental problems. Some of the specialized titles of sessions in these areas were as follows: stimulus controls, aversive controls, fear-conditioning, animal emotion, chemical stimulation, brain lesions and avoidance behavior, physiological-audition, physiological-visual system, imprinting, somatic senses, psycholinguistics, and cognitive evaluation.

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[1] John Locke, *An Essay Concerning Human Understanding*, 1690, II, Chap. 8, §21.

Eight paper sessions related to developmental and child behavior. Social psychology was covered by seven sessions, clinical research by six sessions, and personality studies by seven sessions. One remaining session was entitled industrial psychology.

A special memorial symposium was held in honor of Kurt Goldstein. Brief titles of other symposia were "Transfer models in developmentally oriented research," "Phenylketonuria," "Research in curriculum and training," "Social judgment and attitudes," "Homeless man," "Stimulus control of aversive processes," "Hypnosis research," "Behavior in educational experimentation," "Experience-based learning in classroom settings," "Decision and behavioral change," "Parent and child treatment," "Structure in language and thinking," and "New concepts in the area of stimulus control."

Eliot Stellar delivered the Annual Presidential Address, "Hunger in man: Comparative and physiological studies." Christian Astrup of Gaustad Hospital, Oslo, Norway gave an invited address, "Studies of higher nervous activity in the functional psychoses." By special invitation, Vincent G. Dethier of the University of Pennsylvania spoke on "The hungry blowfly: a study of analogies and homologies of behavior." Neal E. Miller spoke to the Psi Chi meeting on "Psychophysiological experiments on hunger and thirst."

Lila Mosesson of Queens College received the President's Award for presenting the outstanding paper among undergraduate students who reported at the meeting on their individual reesarch.

At the Annual Business Session, James Deese was announced as President-elect and Eleanor J. Gibson, David C. McClelland, and Richard J. Herrnstein as new Directors of the Association. Gorham Lane was re-appointed to a five-year term as EPA Historian. According to a mailed ballot (December 1965), the membership approved a revision of the By-Laws. The change requires the Treasurer to disburse funds and to have custody of securities. The Executive Secretary remains responsible for collecting and depositing receivables and for approving itemized expenditures.

The Association will meet April 6-8, 1967 in Boston, with the Sheraton-Boston Hotel as meeting headquarters.

Queens College

MARVIN A. IVERSON

AN ACKNOWLEDGMENT: THE PORTRAIT OF MICHOTTE

The portrait of Professor Albert Michotte, which appears as the frontispiece of this number of the JOURNAL, was kindly supplied by one of Michotte's younger colleagues and successors, Professor Joseph R.

Nuttin, now Director of the Institute of Psychology at the University of Louvain. In fact, Professor Nuttin sent us three photographs: the one that is reproduced, taken about 1960, and another of eight years earlier which shows a half-consumed cigarette in Michotte's mouth. Michotte liked the latter especially because it illustrated a fact of visual perception: the cigarette stuck out in the third dimension and was not reduced to the plane of the paper. The third picture was a photograph of an oil portrait of Michotte in a Louvain gown and a Cambridge hood for science, with medals and insignia, a portrait that now hangs in the Faculty Room of the Institute of Psychology at the University of Louvain. The informal portrait without the special demonstration of the visual third dimension seems the most appropriate for the JOURNAL's series of famous experimental psychologists. We have also chosen the simple signature that Michotte used for his scientific publications. On other occasions, he used the full signature of the Baron. We have Professor Nuttin to thank for the photographs and for examples of both signatures.

E.G.B.

Albert Edouard Michotte van den Berck: 1881-1965

Within a few months of each other, psychology's two "grand old men" in the French-speaking world—Henri Piéron and Albert Michotte—have left us* Younger by three months than Piéron, Michotte died seven months after him, at the age of 83 in Louvain, where he had spent more than half a century in his life as professor and researcher. A work of his on perception is still in press and is due to appear soon in the *Handbuch der Psychologie* to be published in Germany. Of the two last doctoral theses he directed, one was presented a few months ago and the other is yet to be presented. That shows how active Michotte remained until a few weeks before his death—slowed up, it is true, and housebound for the last three years.

Born in Brussels on October 13, 1881, Albert Michotte was enrolled at the age of sixteen years as a student of philosophy at the University of Louvain. He obtained his *licence* in 1899 with a study of the physiology and psychology of sleep, and his *doctorat* in philosophy in 1900 with a thesis on Spencer's esthetics. Yet his interest drew him to experimental research, and he next enrolled in the department of natural science where he worked for two years at the laboratory of the famous neurologist, Arthur Van Gehuchten at Louvain. His first two scientific publications are contributions to the histology of the nerve cell (1904). It was around 1903—after a conversation with Professor Désiré Mercier, the founder of the laboratory of experimental psychology at Louvain—that Michotte decided to dedicate himself to that young science. Under the direction of Professor Armand Thiéry, who had been that laboratory's director since 1894, Michotte began research on tactual sensation, which resulted in the publication in 1905 of his first experimental work.

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* Translated from the French by Mollie D. Boring.

From 1905 to 1908, Michotte worked for one semester of each year in Germany—first with Wundt at Leipzig, then with Külpe at Würzburg—while giving the course in experimental psychology at Louvain in the other half of the year. His first work (1906-1914), before the First World War, was concentrated upon logical memory and voluntary choice. It employed principally the method of "systematic experimental introspection," as influenced by Külpe.

After the burning of Louvain at the beginning of the First World War, Michotte left his country, as did many other Belgians, and remained in Holland until 1918. There he worked on the measurement of acoustic energy at the Utrecht laboratory of his friend, the physiologist Zwaardemaker.

Back in Belgium after the war, Michotte resumed his teaching and research at Louvain. Due to his enterprise, psychological teaching and research underwent a progressive expansion. Several new professors were appointed. In 1944 he organized an Institut de Psychologie, which could grant the degree of *docteur en psychologie.*

All through these years—even during the period of the Second World War—Michotte's life was completely devoted to his work and offers little of interest besides his scientific activities. He traveled a great deal to present papers at congresses and at many foreign universities. He became professor emeritus in 1952, but continued to offer his course in perception until 1956. He came regularly to the laboratory through 1962, when a small cardiac attack sent him to a clinic for several months. There he continued to write and to direct experiments with the assistance of his colleagues. He died Wednesday, June 2, 1965, in the house in which he had lived since his marriage and where he had received, in the course of his professional life, a great many psychologists who will not forget his "seignioral" hospitality.

* * *

The main theme of Michotte's research is perception: With it he commenced his experiments at the beginning of the century, and it was to this same field—although in a quite different and quite new perspective—that he devoted practically all his work after 1940.

His first experimental work in 1905 dealt with the problem of the tactual perception of space.[1] We know that tactual sensations coming from different regions of the body present qualitative differences, varying with the area of the stimulation (the *local sign*).

By examining different regions of the cutaneous surface of the human body by means of a new esthesiometric technique, Michotte demonstrated that the values of the spatial threshold are very different for certain neighboring areas, although previous research had shown that cutaneous sensitivity was equally acute at these different places. The difference between the values of the spatial threshold does not therefore indicate here a difference in the delicacy of the tactual sensitivity, but a certain *discontinuity* in the sense of touch. Michotte thus succeeded in proving that the cutaneous surface is divided into regions of specific extent and shape, each of which possesses a "local sign" *proper to that region.* He calls this the *regional sign.* He held that, for the surface of the palm of the hand, the regional sign accords with the fact that certain points of the skin do not move with respect to each other, for they always remain in the same relation in movement or in contact with objects.

Even in this first work Michotte showed a characteristic which marked much of his further research: his ingenuity in inventing new techniques and instruments.

Whereas in the classical esthesiometrical technique of this period, one gave the observer (*O*) a *successive* series of double contacts with different spacing between the two points of the esthesiometer, Michotte invented a new type of instrument that made it possible to adjust the two points on *O*'s skin in a continuous manner. One of the points was held still, while the other could be progressively moved along by making it *slide* upon the skin until *O* said he felt two points. This method allowed Michotte to study better the influence of inattention upon the spatial threshold. In the classical method, the successive applications attracted *O*'s attention each time at the moment of contact.

Michotte also invented or perfected several other experimental techniques and apparatus. There is, for example, his "comparison tachistoscope" which he displayed at the International Congress at Geneva in 1909.[2] With it, one can present to *O* two tachistoscopic exposures of different objects at the same point on the retina with a controllable interval between the two. The durations of the exposure and the interval can be controlled within a fraction of a thousandth of a second; the conditions of accommodation and adaptation can be held constant, and the two exposures can be presented either once in a given time or periodically. All the psychologists who have been able to see the final research on the perception of causality in Michotte's laboratory have been struck by the elegant simplicity, together with the ingenuity, of his techniques. His apparatus was simple, built at modest cost either by himself or with the help of his technician, thus at the same time initiating his young collaborators into this instrumental side of research. His laboratory, which included two big rooms, seemed like an artist's or artisan's large studio where apparatus is set up or dismantled according to the needs of the moment.

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[1] Albert Michotte, *Les signes régionaux: nouvelles recherches expérimentales sur la répartition de la sensibilité tactile dans les états d'attention et d'inattention,* 1905, 1-195.

[2] Michotte, Description et fonctionnement d'un nouveau tachistoscope de comparaison, *Arch. de psychol.,* 12, 1912, 1-13; also in brief in *Comptes rendus, VIme Congrès internat. de Psychol.,* Geneva, 1910, 733.

It was under the influence of Binet and of Külpe, and also due in part to his training in philosophy, that Michotte's interest soon led him to the experimental study of the "higher mental processes." For one thing, he studied the role of thought in logical memory and in association; for another, he undertook research on voluntary choice, a study which was widely acclaimed in the period preceding 1914. These various investigations employed Würzburg's "systematic experimental introspection," but Michotte's positive turn of mind led him at the same time to make various objective measurements which often turned out to be quite important in interpreting the data.

In respect of Michotte's research on logical memory, I stress one point of theoretical importance.

Michotte presented his *O*s with a series of paired words; each pair was exposed for six seconds with the instruction to find a *relation* between the two terms of the pair. When the series was completed, he interposed a variable interval, after which he presented *O* with either the first or the second word of each pair, with the instruction to reproduce the other word. This series was compared with other series where memorizing was required without making 'logical' associations between the terms, as in the classical experiments of that time. On the basis of these facts as determined, Michotte came to the following conclusion: When the stimulus-word is presented to *O* with the instruction to reproduce the other word of the pair, the relation between the two terms as determined by *O* at the time of the first presentation is aroused in his consciousness, and it is by way of the stimulus-word and the "consciousness of relation" that the second word is recalled and reproduced. Michotte admitted that the construction by *O* of the relation between the two terms during the initial presentation consists in the incorporation of these two terms into a unity of a higher order, a consequence of the integrating function of thought in memory. Thus, it is due to something more than a simple associative bond. Because of this integration, the terms to be memorized are fused into a relational unity that Michotte calls a "complexion." This "complexion" consists of the two terms and the relation itself, which form a "very intimate unity." These three elements are "united as parts of a whole; . . . they do not merely form an associative constellation." Michotte depicted the reproduction of the second term in the following manner: When "the stimulus-word and the relation are given, . . . *they present O with the greater part of a complexion for which it is necessary to recall the missing part.*" That is to say, the revival of a part of the "whole" then leads to the direct reproduction of the other part; it sets up a process of active search for the missing part, which leads to the reconstruction of the integral "whole."

It is important to note that this conception—which clearly involves a "gestalt" way of seeing the process—was worked out in the course of a study undertaken with Ransy in 1908, although it was not published until 1912.[3] The year 1912 is considered to be the year of the birth of Gestalt psychology in the field of perception, but Michotte had already elaborated a Gestalt conception in the field of memory.

In respect of Michotte's research on voluntary choice, which he published in 1910,[4] the important fact that he affirms is the existence, for *O* who is choosing, of an immediate experience of his own activity which may be called "consciousness of action."[5] This work has an especial historical value, for it has contributed greatly to freeing European experimental psychology from its static and elementistic character by stressing the motivational factor involved and the consciousness of action in "mental" life. Consciousness of *self,* which since Hume has ordinarily been replaced by simpler "elements," also reappeared independently within the frame of this "consciousness of action" and thus under a dynamic global aspect. What can still interest today's student in this early "experimental phenomenology" of voluntary choice is that one finds in it a very meticulous introspective analysis of such processes as *decision,* the execution of a *task,* and so forth.

This work on voluntary choice has had a great influence upon the further orientation of Michotte's work, for it drew his attention to the dynamical aspect of psychic functions and to *action* rather than static content. This point of view recurred periodically in his later lectures (Yale, 1929) and finally led to his studies on *perception of action.*

When, after the First World War and the resulting four years' absence from his laboratory, Michotte returned to Louvain in 1918, clearly he had lost his confidence in the introspective method of the Würzburg school. His research then took a new direction. Nevertheless, he continued to be inspired by the two major ideas that were born in the first period of his professional thinking. On the one hand, there was the idea of an activity of integration which constructs *structures of wholeness,* and, on the other, the idea that *action* is always central in the study of psychic functions.

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[3] Albert Michotte and Clément Ransy, *Contribution à l'étude de la mémoire logique,* Louvain, 1912, 26.

[4] Albert Michotte and Emmanuel Prüm, *Etude expérimentale sur le choix volontaire et ses antécédents immédiats,* 1910; *Arch. de psychol.,* 10, 1910, 113-320.

[5] In the same year Narziss Ach published *Ueber den Willensakt und das Temperament,* 1910, a book that agrees with Michotte on certain essential points in respect of *consciousness of action.* Although Ach's work was published several months before Michotte's, Michotte's experiments were done in 1908 and the work was first edited in German (his collaborator Prüm was German-speaking, and Michotte always accepted the language of his collaborators). It was the translation into French that delayed the publication. It is thus certain that Michotte and Ach reached similar conclusions and conceptions of consciousness of action independently.

Michotte did not become acquainted with the works of the German Gestaltists until fairly late. The complete destruction in 1914 of the Library of the University of Louvain seriously slowed up the arrival of publications, and besides Michotte was much absorbed in the material reorganization of his laboratory. He himself has remarked that it was not until 1923—during the VIIth International Congress of Psychology at Oxford, where he met Köhler and Koffka for the first time—that he took this new current in German psychology seriously into consideration. Actually, he himself had reached, fairly early in his study of logical memory, ideas quite similar to those of the German Gestaltists. Indeed he discovered the processes of structurization and organization of wholes in several new fields. It seems clear that Michotte came to these conclusions independently, very probably after considering Von Ehrenfels' studies, by which he was greatly impressed while he was at Würzburg.

The most important research accomplished at Michotte's laboratory in the period between the two wars (1918-1939) was that showing the formation of Gestalts, the *motor* and *temporal* structures formed in motor training. In a work on sensorimotor training, published under Michotte's direction by his collaborator Van der Veldt,[6] one is shown, among other things, how the *motor* forms are progressively derived from *perceptual* forms. These motor forms acquire their own structures and obey special laws. The different stages of the process by which the motor reaction develops from a series of simple movements into an organized 'whole' have been demonstrated very clearly by de Montpellier, who used a cinematographic technique in a series of experiments carried out under Michotte's direction.[7] This research failed to receive the acclaim it would have had had it been published in English.

Michotte also demonstrated the influence of rhythm on motor structures, an initial finding which led him on to a group of studies of *temporal* structures.[8] My own research on the law of effect also took its point of departure from Michotte's laboratory during these same years.

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[6] Jacques Herman van der Veldt, *L'apprentissage du mouvement et l'automatisme, Etude expérimentale,* 1928, xxi + 350.

[7] Gerard de Montpellier, *Les altérations morphologiques des mouvements rapides,* 1935. A third set of studies has been made in the same context on the influence of speed on the deformation of structure and on the accuracy of motor reaction: Harry V. McNeill, *Motor adaptation and accuracy,* 1934, ix + 303.

[8] Cf., among others, the research done in Michotte's laboratory by Paul Fraisse in

During this same period (1918-1939), Michotte's other principal idea was slowly maturing, the idea that came to inspire his entire work; namely, that it is within the frame of *action* that we must study such other processes as perception and emotion.[9] It was in an important paper given at the IXth International Congress of Psychology at Yale University in 1929 that Michotte clearly expressed this view for the first time. At the same time, he announced a program of research that he only began to carry out ten years later, a program which then became his principal work.

> At Yale, Michotte said: "Perception . . . jockeys and directs our actions, and with good reason it can itself be regarded as a simple 'phase' of the complete process that is an action. As a phase of action, it develops under the pressure of tendencies and needs which dominate the individual at the moment of the 'stimulation,' and it must therefore be a function of these factors. . . . The 'perceptions of the laboratory' play only in a rudimentary manner the functional role of perception in real life. It is therefore necessary to study . . . systematically in what degree and according to what principles the effects of sensory stimulation are determined by the needs or tendencies of the individual." That was written in 1929. In respect of this matter, however, Michotte undertook no research of his own. Nevertheless, he doubtless exerted some influence on the numerous studies which have since then been directed upon this problem.
>
> A second point in Michotte's paper at the Yale Congress pled for the study of certain complex perceptual phenomena, especially "the perception of the physical action that things exert upon each other and the changes which result as, for example, in the perception of the impact of two bodies and the consequent mechanical effects, like breaking, deformation, etc." He went on to suggest that complex perceptions are probably "dynamic syntheses of a higher order, obeying laws more or less different from those which govern the simpler experiences," and he noted certain facts in support of his case. He called these phenomena "perceptions of process," stressing the fact that they play an important role in our experience of handling objects and in the genesis of the significance of objects. Furthermore, he said, "They determine our behavior in a large measure, since they constitute the basis for utilizing objects that surround us, like tools and other instruments of our actions."[10]

In the same year, Köhler was expressing the same idea by affirming that immediate causal impressions exist in external experience.[11] Systematic experimental research on this subject was not begun by Michotte until 1939, a date that signifies the start of a new period in his work. It was in

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1935 and published later: Paul Fraisse, Contribution à l'étude du rythme en tant que forme temporelle, *J. Psychol. norm. path.*, 39, 1946, 280-304. This work was developed later independently by Fraisse in his own laboratory in Paris.

[9] *Action* is the term Michotte used currently in this period for *active behavior.*

[10] Michotte, Quelques aspects de la psychologie de la perception négligés dans les recherches expérimentales, *Proc. IXth International Congress of Psychology,* 1929, 307 f.

[11] Wolfgang Köhler, *Gestalt Psychology,* 1929, 320.

this field of the perception of action and the related problems that Michotte carried out a great variety of experiments in the course of the last twenty-five years of his life, experiments which resulted in an impressive series of publications and have become a source of inspiration for several investigators in other countries.[12]

The important point in these studies of impact, quite apart from the dispute about nativism and empiricism, is that Michotte demonstrated under exactly what conditions of stimulation the *O*s do not perceive a *succession* of two movements (the movement of Object A which, at the moment of impact, is *followed* by the movement of Object B), but *perceive* directly the *causal influence* exerted by Object A upon the displacement of Object B. Certain slight modifications in the over-all constellation of the stimuli can make this perception of causal influence disappear. The fact of "knowing" that this influence does not actually exist *in reality* does not alter the fact of a *perception* of causality.

The perception of causal influence thus seems to be closely linked with precise constellations of stimulation. Michotte elaborated a theory to explain the specific perceptual structure that produces the different forms of causal impression. Space does not permit the summarization of this very refined theory, but I mention the essential features.

The process by which an object in movement pushes or drags another object, which was not moving at first, can be divided into two phases: (1) where both objects (A and B) are present, but where only A is moved toward B which still remains stationary; and (2) where B, after the impact, is displaced in its turn with or without A. These two phases present two very different perceptual structures, each dominated by its own laws. The conflict between these two structures would have the effect that the second phase is perceived in two ways at the same time (phenomenal doubling): on the one hand, as a *continuation of the movement* of A (a result of the tendency toward phenomenal permanence of the first phase) and, on the other hand, as simple *displacement* of Object B. In other words, the displacement of B appears to be a *prolongation of the preëxisting movement of A.* The displacement of B is thus identified phenomenally with the continuation of A's movement, so that it is A's movement which seems also to displace B. This process of apparent extension of A's movement to B Michotte calls *ampliation of the movement,* a structure that is very fleeting and corresponds with the magnitude of a "ray of action" that can be measured experimentally. The question of the role of the acquired experience and of tactual-kinesthetic structures in the origin of these causal visual structures remains open. Michotte was convinced upon certain grounds, that these perceptual structures are established in an autonomous manner as soon as a certain degree of maturation is reached and a particular constellation of stimulus is presented to the subject.

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[12] Michotte's first publication on the perception of causality was in 1941. His major book is *La perception de la causalité,* 1946, first French edition; second edition, 1954, viii, 306. The English translation is *The Perception of Causality,* 1963. The later research of Michotte and his collaborators on this subject and the related themes were brought together in a volume published on the occasion of Michotte's 80th birthday: Michotte and collaborators, *Causalité, permanence et réalité phénoménales: Etudes de psychologie expérimentale,* 1962, and includes a full bibliography. His last work was published in 1964: *Les compléments amodaux des structures perceptives, Studia Psychologica,* 1964.

This research on the perception of action and causality led Michotte to investigate various other aspects of perception; for instance, the study of animal movements including the 'intentional' character of certain movements and their affective or emotional value (as in flight or attack) in relation to projection of phenomenal characters into the perceived object; also the phenomenal permanence of objects which are partially or even totally hidden with respect to the 'amodal' complement of perceptual structures, and the impression of phenomenal *reality* with respect to perspective in drawing or as applied to cinematography.[13]

In all his research on perception Michotte showed that the fundamental structures of our phenomenal world—the behavioral world as it appears to man, according to his verbal descriptions—are linked with certain clearly determined conditions of stimulation. Michotte was able to reduce many of these conditions to general laws which govern the organization of the perceptual field. And that is, indeed, the main import of his work. At the same time he has shown that many *notions* which intrude into our construction of the behavioral world, like *causality, reality,* and *permanence,* are already "foreshadowed" at the level of perception.

* * *

Michotte was an excellent teacher and a brilliant lecturer. Never content with reading from a written text, he gave —often without notes— a lively and well-prepared discourse. He never liked to edit and publish his work.[14] Much did he prefer to report his results orally, with demonstrations, in his many lectures, some at psychological congresses and others at the many universities where he was invited to speak. Above all, he liked to receive foreign colleagues in his laboratory and to show them—often for hours on end, and sometimes over several days—what he was busy doing. He taught as visiting professor at Stanford University in 1929 and at the University of Toronto in 1952.

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[13] See the publications cited in Note 12.

[14] A relatively important part of the research done under Michotte's direction has remained unpublished. He writes: "If I have experimented greatly during my life and thereby learned many things for use in my teaching or in lecturing abroad, a relatively important part of our work has never been published. I must confess moreover that, dominated as I was by a true passion for research, my joy of having pursued it to its conclusion and of having found the answer to the question that had aroused my curiosity led naturally to the waning of my enthusiasm. Hence the idea of next giving myself up to the fastidious work of editing which (it seemed to me then, for later I realized my error) would teach me nothing new did not appeal to me in any way."

The complete list of works carried out under Michotte's direction in his laboratory up to 1946 is given in the *Miscellanea psychologica Albert Michotte,* 1947. The more recent research is cited in the volume, *Causalité, permanence et réalité phénoménales,* 1962. In 1912 Michotte founded a *Psychological Series* under the title "Etudes de Psychologie" for publishing the research of his laboratory. Six volumes were published between 1912 and 1946. Actually, the series has been continued under the name *Studia Psychologica* (under the editorship of Albert Michotte and Joseph R. Nuttin). About thirty volumes have appeared in many languages (some in English).

It is proper also to mention what Michotte did for the international organization of psychology. From 1909 on he was a member of the International Committee for the International Congresses of Psychology, and he played an active role in the Congresses from the Fifth in Rome (1905) to the Sixteenth in Bonn (1960)—twelve in all. He presided over the Fifteenth Congress at Brussels in 1957. He was one of the founders of the International Union of Scientific Psychology at the Stockholm Congress in 1951, and was President of the Union from 1957 to 1960. He was a cofounder and President of the Association de Psychologie Scientifique de Langue Française in 1952. In his own country, he founded in 1946 the Société Belge de Psychologie and was its first president. He was also the recipient of many honors in the scientific world. He was named Foreign Associate to the National Academy of Sciences in Washington in 1956, member of the Pontifical Academy of Sciences at Rome in 1936, of the Royal Belgian Academy in 1946, of the Bayerische Akademie der Wissenschaften in 1952, and of the academy of Madrid in 1947. He received honorary doctoral degrees from the Universities of Cambridge, Montreal, and Nijmegen, and he was an honorary member of a great many national psychological societies. In 1954 he received from the king the title of Baron.[15] During the celebration in 1947 of his jubilee of forty years as professor, a volume of psychological studies was published in his honor, with the title *Miscellanea psychologica Albert Michotte,* to which forty-one distinguished psychologists from thirteen countries contributed.[16]

About his own personality and temperament, Michotte ventured the following description in an address on the occasion of his jubilee: "A good fairy—and I am grateful to her—has endowed me with a constitution that Kretschmerian typologists would classify as pyknic type and cyclothymic temperament, with a strong propensity towards optimism, oscillating perhaps between what Kretschmer calls hypomanic and syntonic forms." The hypomanic side was manifested in the great abundance of new ideas and experimental projects with which he arrived almost every morning at his laboratory during his most active years (1939-1952). He was possessed, moreover, of such grace and distinction that he never, I am sure, made enemies. He was a warm friend to his many colleagues and disciples, always interested in their lives.

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[15] In 1939 the name of one of Michotte's ancestors, *Van den Berck,* had been added to the family name *Michotte,* so that his complete name became Michotte van den Berck. In his scientific writings, however, he continued to use simply the name *Michotte.*

[16] Among the articles in English we mention those of F. C. Bartlett, Cyril Burt, James Drever, W. S. Hunter, Wolfgang Köhler, H. S. Langfeld, R. B. MacLeod, T. V. Moore, Edgar Rubin, William Stephenson, L. M. Terman, and E. L. Thorndike. Among the articles in French are those by F. J. J. Buytendijk, Paul Fraisse, Agostino Gemelli, Pierre Janet, Henri Piéron, and Géza Révész. A small number of copies of this volume are still available at the Laboratoire de Psychologie of the University of Louvain.

It is my belief that the influence which Michotte exerted by his personal contacts was fully as great as the dispersion of his ideas by his publications. This effulgence that originated with him was limited, nevertheless, pretty much to that group of researchers who were not solely concerned with external behavioral reaction but who also took into account the whole meaningful situation and phenomenal world in which behavior develops. To our scientific knowledge of the construction of this phenomenal world Michotte brought an original and important contribution.

University of Louvain JOSEPH R. NUTTIN

BOOK REVIEWS

Edited by T. A. Ryan, Cornell University

A Study of Children's Thinking. By Margaret Donaldson. New York, Humanities Press, 1964. Pp. viii, 263. $6.00.

One of the more puzzling features of the current psychological scene is the sharp cleavage existing between the educational practitioners who construct group ability tests for various applied purposes and the academically oriented psychologists who conduct laboratory research on problem-solving and thinking processes. This rigid separation has fortunately been breaking down over the past few years, and, in the reviewer's opinion, the Donaldson book should speed the process along. The major aim of the book is to show that better tests of intelligence would result if the test builders complemented their psychometric approach with a consideration of how children actually go about solving the problems that are used to assess their ability and predict their future performance. In the author's words, "We want to know not only *that* an item works for a given predictive purpose but *how* it works: what there is about it which makes it successful, and what different successful items have in common" (p. 21). To implement this aim, Donaldson selected a variety of problems from current intelligence tests and administered them to children (ages 9 to 14) under "thinking out loud" instructions. The major portion of the book consists of verbatim accounts—spontaneous and elicited by the examiner's questions—of children's "thought products." By the author's own confession, the book is a *clinical* study of children's thinking.

The vast quantity of qualitative data generated by Donaldson's clinical method is reduced and classified on the basis of types of error committed in problem solving. Three basic types of error are distinguished—arbitrary, structural, and executive. The common feature of the arbitrary form of error is "a lack of loyalty to the given" (p. 183). In such cases, the child often introduces considerations drawn from his "real-life" experience which violate the constraints of the problem. Structural errors "arise from some failure to appreciate the relationships involved in the problem or to grasp some principle essential to solution" (p. 183). A child's failure to handle negative statements in formal syllogistic reasoning would produce errors of a structural sort. Another such error is the conversion of information stated in relativistic terms into information denoting class membership. Finally, executive errors "arise not from any failure to understand *how* the problem should be tackled, but from some failure in the actual carrying out of the manipulations required" (p. 184). Such errors generally come about when the child, though potentially capable of solving the problem at issue, forgets, overlooks, or distorts some part of the information contained in the problem that is critical for correct solution.

Donaldson attributes a logical progression to the manifestation of the three forms of error. If an arbitrary error appears in the course of solving a problem, then there is less opportunity for a structural or executive error to emerge. Similarly, the occurrence of a structural error sharply reduces the probability of an executive error. The developmental trend in the error data corresponds to the foregoing sequence. In Donaldson's terms, the process is one of "error replacement." The youngest children yield the highest frequency of arbitrary errors. As the children get older, those errors decline and structural errors increase. The latter, in turn, show a decrease in the oldest children, for whom executive errors are most typical. The foregoing observations are based upon both cross-sectional and longitudinal data (retesting of children after a two-year interval).

In the concluding chapter of the volume, Donaldson makes a commendable effort to relate her findings on error sequence to other relevant investigations of cognition and thinking. Thus, selected aspects of the work of Piaget, Goldstein and Scheerer, Luria, and George Miller are introduced in an effort to give some theoretical perspective to the results of the study. The interpretations offered in this portion of the book have a distinctly *post hoc* quality, though the discrepancies between Donaldson's findings and what other investigators (especially Piaget) might predict are quite intriguing and highly suggestive of further research.

Given the atheoretical perspective from which the Donaldson study originated, it is very much to the author's credit that she was able to articulate her findings with various cognitive theories. The construction of items for most standardized intelligence tests is not governed after all by any formal theory of human ability, but rather proceeds largely by "hunch." Though Donaldson acknowledges this fact in her book, she nevertheless selected her problems from current tests on the basis of their "predictive power." Since no indication is given of just what these problems are supposed to predict, a theoretical rationale is lacking for the choice of the particular types of problems employed in the study.

Has the author accomplished her major practical purpose of pointing the way toward better tests of intelligence for children? Donaldson expresses great concern over the likelihood that children will arrive at correct answers for wrong reasons. The test constructor who knows that children commit distinctive kinds of errors at different age levels could presumably build distractors for test items which would tempt the child into committing errors typical for his age group. By this means, the discriminating power of test items might be improved. This reviewer does not doubt that Donaldson's results can be put to practical use in the foregoing fashion. surprising, however, to find a book devoted to errors in children's think-It is most ing whose major practical import, in the author's view, is the construction of standardized tests that will facilitate the expression of such errors. One forms the impression that children are being accused of fooling the test maker, and that the time has come to put a stop to it. At a time when many psychological and educational researchers are experimenting with techniques intended to raise the level of cognitive functioning in children, it is odd to read through an entire book on children's thinking containing no reference to the possibility of teaching or training children to avoid the types of error to which they are especially susceptible. There is no indication that Donaldson is committed to a "stage" conception of cognitive development in which characteristic error tendencies are presumed to be imminent at particular age levels. In this reviewer's judgment, the complete neglect of the instructional implications of her research gives the Donaldson volume a distinctly anachronistic quality.

This is not a book for those who like concisely stated hypotheses and statistical tests of significance. Donaldson justifies the absence of the latter on the grounds that her work consists of a clinical search for hypotheses. In the reviewer's judgment, the application of two-tailed significance tests to the descriptive statistics provided would at least have given the reader a general idea of which differences deserve further study and which deserve to be ignored. It is also difficult to understand why no between-problem correlations are reported. Without such correlational evidence, we are left completely in the dark regarding the consistency or generality of the children's error tendencies. As a substitute for such analysis, we are treated to page after page of children's thoughts during problem-solving. Approximately one-half of the book is comprised of these verbatim protocols. In working one's way through this mass of material, interest is not easily sustained. Judicious editing and selective deletion of the verbatim response protocols would have produced a briefer, but much more readable, volume.

Despite its numerous stylistic and methodological shortcomings, the Donaldson volume does offer a number of unique psychological insights into children's thought processes. It is a book well worth the attention of experimental, developmental, and educational psychologists who are concerned with problems of children's thinking.

Educational Testing Service NATHAN KOGAN

Factor Analysis of Data Matrices. By PAUL HORST. New York, Holt, Rinehart, and Winston, 1965 Pp. xix, 730. $15.00.

The title, flyleaf, and preface of this book will give some readers a rather misleading impression concerning its contents. In one respect the book is broader than many readers will at first assume, while in another respect it is narrower. The book discusses many more types of factor methods than is implied by the title; it covers all the best-known traditional methods which begin with correlation or covariance matrices, as well as methods which begin with data matrices. The descriptions of most of the methods are considerably less complete, however, than many readers would wish. A typical discussion of a factor method includes several pages of computational directions and numerical illustrations, a computer program in an appendix, mathematical proofs of several properties of the method (not always the most important or useful properties), some comments about the amount of labor involved in hand computation or the seriousness of rounding error, but practically no discussion of the uses or limitations of the technique.

Most of the other topics essential for understanding the role of factor analysis as a scientific tool are also covered only briefly or not at all. Although the book contains a general mathematical introduction to factor theory, this reviewer found it considerably less useful than Guttman's 1944 *Psychometrika* paper. The book also contains little or no discussion of significance-tests or of sampling problems in factor analysis, which have long been known to be extremely serious. The discussion of analytical methods of rotation is considerably less informative than discussions in the better previous texts.

Many readers will find the omission of a serious discussion of the uses or limitations of factor analysis especially disappointing, in view of the general agreement that factor analysis is one of the most widely misused statistical techniques in modern psychology, the field of its birth and its broadest use. Perhaps related to this failure to consider deeply the uses of factor analysis is Horst's cavalier treatment of the communality problem. Although he discusses methods of estimating communalities (in a chapter on Scaling), he makes it clear throughout (pp. viii, 468) that he feels the problem is best ignored. Since communalities in some form are essential for estimating the minimum number of common factors in a matrix, Horst apparently holds that this question is never of interest. One of the clearest applications of factor analysis, however, is its use in rejecting theories on the ground that they postulate fewer common factors than can be shown to exist. Spearman's single-factor theory of intelligence was rejected by such factor studies, as one aspect of Hullian theory was rejected by a factor study by Humphreys (Measures of strength of conditioned eyelid responses, *J. gen. Psychol.,* 1943, 29, 101-111). When more than one common factor is predicted by a theory, it can be disproved only by a study which somehow uses the concept of communality.

From the above list of topics omitted or treated lightly, it is clear that the book is best viewed as a reference work for advanced workers rather than as a text. Probably most of the purchasers who will be disappointed with the book will be those who bought it as a general text on factor analysis, misled perhaps by ambiguous statements in the flyleaf and the preface, and by the fact that the early part of the book contains over 60 pages of exposition of matrix algebra for the beginning student. These pages are a condensation of Horst's *Matrix Algebra for Social Scientists* (1963). As a means of preparing to read the rest of the present book, several Cornell students found the earlier book more useful than the condensation, due to the necessary omission of explanations and proofs, many of which are quite simple and give a far greater feeling of mastery of the material.

Finally, evaluating of the book as a reference source, one sees that it makes generally available for the first time many little-known or completely new factor-analytic techniques. The contributions include a modification of the centroid technique which makes it completely objective, an interesting extension of the Varimax rotational method, plus many more. The computer programs are, as Horst states, one of the most important parts of the book. The computing directions are, for the most part, clear, but are inadequately discussed in the text. For example, the computing directions for the multiple group factor method include a rotation to orthogonality, but this fact is never mentioned in the text. This was discovered by the reviewer only when he tried to derive the necessary equations after a student had pointed out that the directions were inconsistent with those given in other texts.

The large number of mathematical proofs adds much to the completeness of the book, but there are some curious omissions. Two of the most fundamental theorems in all of factor theory are those which state that the principal component method maximizes the variance accounted for by a given number of factors, and simultaneously gives the best least-squares low-rank approximation to a covariance matrix. Neither of these proofs is as difficult as many proofs given, but Horst dismisses them with the erroneous statement that they require use of hyperspatial geometry. Other important omissions could be cited.

For a book full of computing directions and computer programs which most users must accept on faith, there is an occasional disturbing impression that the book was written too quickly. Some of the points which convey his impression have already been mentioned. Often important references to papers on a topic are given at a point where the topic happens to arise for a few sentences, rather than in the chapter dealing primarily with that topic. On p. 138 the reader is referred to the bibliography for a reference to the 1944 Guttman paper mentioned above, one of the most important papers in the history of factor theory, but then upon turning to the bibliography the reader finds that the paper is not listed. At several points, provocative and puzzling statements are tossed in with no attempt at defending them, such as the comment that "statistical analysis is a branch of factor analysis" (p. 7), or the statement that "unless one is concerned with determining whether or not there is a relationship between two or more things which vary, his interest is not scientific" (p. 6). If this latter statement means anything, it means that the hypothesis that a given coin comes up heads 50% of the time is not a scientific hypothesis.

Except for this nagging doubt that every detail is correct, which exists to some extent in any book, the criticisms mentioned should not detract attention from the fact that this is in many ways a uniquely useful work, which should be of great interest to specialists in factor theory and its applications. It is without peer as a reference work for computational aids and as a source of advanced factor analytic techniques. It is not, however, recommended as a text.

Cornell University RICHARD B. DARLINGTON

Mental Retardation: A Family Study. By ELIZABETH W. REED and SHELDON C. REED. Philadelphia: W. B. Saunders Co., 1965. Pp. v, 719. $16.50.

This monumental study from the Dight Institute for Human Genetics of the University of Minnesota reports the rationale and family history data of a project of the Minnesota Human Genetics League. The publisher's descriptive folder states that: "The results of this study show how, by use of methods presently available, the total number of mentally retarded persons in the United States could be reduced by about one-third in a single generation." If warranted, this startling claim would place this volume in the forefront of the current spate of national concern for the retarded along with so many other optimistic assertions of recent date.

The scope of the project is described by the publisher as follows:

> This prodigious study meticulously presents vital data on six generations of relatives of mentally retarded patients. The book provides a mine of information for every geneticist, psychiatrist, psychologist, sociologist, family counselor—or any researcher interested in the problem of mental retardation.
>
> More than 600 pages are devoted to full pedigrees of 289 mentally retarded patients, accompanied by brief histories of the patient and others in the family of special interest. This monumental project encompasses exhaustive data on 82,217 individuals. Some of the families are traced back for over a hundred years. The hundreds of kinship charts concisely present the mental status of the various descendants through succeeding generations. Nowhere else in the world today does there exist a comparable genetic study of the incidence of mental retardation in entire families—*in terms of comprehensiveness of documentation* (publisher's italics).

For seven years, beginning in 1911, Sadie Deavitt and Marie Curial traveled the length and breadth of Minnesota. Despite inconvenience and often at great personal risk, they investigated the families of 549 patients at a state school for mentally retarded. They interviewed relatives, physicians, school officials, employers, and neighbors, carefully and accurately recording their findings. Now these findings have been amplified and extended by a follow-up of monumental proportions on 289 of these families. More than 80,000 individuals were investigated to determine the present social, economic, and intellectual status of the descendants of the grandparents of the original patients. This work presents an unprecedented in-depth picture of the relationship between kinship and mental retardation.

A first difficulty which troubles this reader is the authors' orientation toward mental retardation. They "were not primarily concerned with the social inadequacies of either the retarded or their intellectually normal relatives" (p. 1). And (p. 2) "The only persons to be classified as mentally retarded in our study are those who scored below 70 on standard intelligence tests, *or those so classified by careful subjective judgment*" (italics ours). Bearing in mind the period covered (1911 to recent date) and the many published studies between these dates on social competence, *IQ*, and the relation of mental retardation to mental deficiency, we find this orientation extraordinarily naive. Certainly, the review of the literature on the related issues impresses us as grossly inadequate in relation to their vital bearing on the validation and classification of the probands of the study and its implications. Without convincing validation of the propositi in the clinical aspects of mental retardation and its simulative counterparts, any critical evaluation of this study must be discouraging or disparaging. Hence the genetic aspects are correspondingly unconvincing.

The book is in two parts; five chapters (82 pages) expound the project (introduction, organization, relatives, family dynamics, and implications) and four appendix chapters (the remainder of the book, 630 pages) present charts and data for what are classed as the primary genetic category, the probably genetic, the primarily environmental, and the category of unknown causes. There is a brief list of references and an extensive index.

The numbers of the original probands in these four categories were, respectively, 84, 55, 27, 123, total 289. Their assigned *IQ*s by sex (p. 16) were:

IQ	0-24	25-49	50-69	T
M	42	58	38	138
F	36	63	52	151
T	78	121	90	289

It is futile to attempt a critical review in short space of such a prodigious study. Analytic examination of the charts is fascinating, but one is constantly returned to the validity and selectivity of the probands and their classifications. These "worries" keep troubling the reader, whether applied to concepts, data, or etiological groupings in the light of other evidence on the same issues in a wealth of other publications.

The book's appearance is particularly timely in view of the virtual absence of current discussion of familial causation, and the substitution of environmental-economic-cultural deprivation for it, a serious confusion of cause and effect. This study inevitably involves many controversial problems of concept, diagnosis, classification, etiologies, clinical varieties, cultural status, IQ validity, and more, each of which after many years is still subject to vigorous argument. Inevitably, therefore, there will be conflicting opinions about the value of the data and the conclusions of this study; and these will no doubt be heavily influenced by the ambitious nature and extensive scope of the project itself.

Bellingham, Washington EDGAR A. DOLL

Thought and Choice in Chess. By ADRIAAN D. DE GROOT. The Hague, Mouton and Company, 1965. Pp. xvi, 463.

Thought and Choice in Chess is a translation of Dr. A. D. de Groot's dissertation presented to the faculty of the University of Amsterdam in 1941, revised in 1943-1944, and published in 1946 under the title *Het Denken van den Schaker: Een experimentelpsychologie Studie* (Amsterdam, North-Holland Publishing Company, Ltd., 1946. Pp. 315). The author—a mathematician, a chess-master and player in international tournaments, and a student of psychology—served as the principal experimenter in this study. His *Versuchspersonen,* listed on p. 87, were all expert chess-players—masters, grandmasters, or world champions.

During the preparation of his dissertation for publication, De Groot adopted Otto Selz's concept of thinking—"an hierarchically organized linear series of operations" (p. vi)—as the basis of his analysis and interpretation, with which he had, until then, "but superficial knowledge" (p. 53). His study of Selz's "voluminous and difficultly written works"[1] made him "realize more and more his admiration for and indebtedness to Selz" (p. 53), whom he had come to regard "as the most important and the most generally understood psychologist of the *Denkpsychologie* group" (p. 52). That this opinion is not widely shared by psychologists is due, as De Groot states, to the fact that Selz's works "have been read by few, misunderstood by many, and completely overlooked by most" (p. 53).

In recognition of his debt to Selz and to win for him his rightful place in the psychology of thought, Dr. de Groot set himself the task of "using the terminology and applying the laws of Selz's *Denkpsychologie*" (p. 14) in reporting his experiments in chess. "To disentangle his (Selz's) constructions and to penetrate his meaning" proved, however, "a puzzling and sometimes rather frustrating task" (p. 54). Dr. de Groot persevered, however, and he now thinks that he has resurrected, revived, and confirmed Selz's theory.

In the introductory chapters (I-III), Dr. de Groot sets his stage. He opens Chapter I with a report of Binet's study of blindfold chess and a short discussion of talent in chess—items of immediate interest which capture his reader's attention. He then explains the purpose of his study and meticulously outlines his book that his readers may follow his exposition, closing with an explanation of the choice-of-move problem, the subject of the book. Chapter II is devoted to the theories of thinking, and includes a brief biography of Selz and an exposition of his *Denkpsychologie.* Dr. de Groot's account here is strongly biased; he completely ignores the work done on that subject in America. Titchener's name is noticeable by its absence and the American studies on thought involving chess are not mentioned.[2] Chapter III describes his experimental procedure.

These preparatory chapters lead to the main part of his book. Chapter IV (misnumbered "Chapter VI" in the Table of Contents) contains a detailed description of his protocols; Chapter V deals with the general structure of the chess-problem, treating it in terms of Selz's hierarchical structure; Chapter VI turns to the dynamics of the processes involved in the game, in which the author's main theme is developed; and in Chapter VII, the analyses previously made are reassembled, reviewed, and brought into line with Selz's work.

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[1] Otto Selz, Ueber die Gesetze des geordneten Denkverlaufs, Stuttgart, Spemann, 1913, Pp. viii, 320; Zur Psychologie des produktiven Denkens und das Irrtums, Bonn, Friedrich Cohen, 1922. Pp. xxviii, 688.

[2] A. A. Cleveland, The psychology of chess and of learning to play it, this JOURNAL, 18, 1907, 269-308; E. E. Southard, An introspective account of a chess game, in R. M. Yerkes' *Introduction to Psychology,* 1911, 189-192; K. M. Dallenbach, Blindfold chess: The single game, *Studies in Psychology: Titchener Commemorative Volume,* 1917, 214-230.

The book is complete with Chapter VII as De Groot writes: "Having come so far . . . it seemed quitenatural to keep going" (p. 16). Chapter VIII is a popular discussion of chess varying from the social status, education and training, and the extra-chess occupations of the grandmasters to Ernest Jones's psychoanalysis of Paul Morphy and the motivation, intelligence, race, and personality of the players. De Groot divides the grandmasters into two types: "fighters," like Steinits, Lasker, and Alekine; and "players," like Capablanca and Euwe. Not all of the factors that determine the outcome of match-play depend, however, upon the players personality. The challenger appears to have a strong advantage. Not once in the modern history of chess, as Euwe is quoted, has a world champion defended his position against a challenger regarded as of equal ability. (Cf. Footnote 30, p. 361). De Groot gives us in this chapter a wealth of interesting chess history and lore. As welcome and refreshing an interlude as it is, it has, however, little bearing upon the topic of his study.

The concluding chapter, Chapter IX (misprinted "XI" in the Table of Contents), is entitled "Epilogue 1963." It was written, after the revision and translation of the book had been completed, to defend the "introspective method" that he had perforce used. The method was anathema to the behaviorists and Gestalt psychologists at the time his study was undertaken (1938). His defense of introspection was not written, however, to gain converts but to clarify his own point of view. He did not, in his discussion, differentiate between *Kundgabe,* the statement of meaning, and *Beschreibung,* the description of process—distinctions that the introspectionists themselves strongly emphasize.

The book closes with two Appendixes: I. Reproduction of the games from which test-positions were taken; and II. Collection of the Protocols, and with two bibliographies: A. of Chess, and B. of Psychology; and Indexes of Subject Matter and of Names.

The University of Texas CHARLES V. BROOK

Cognition and Thought: An Information Processing Approach. by WALTER R. REITMAN. New York, London, Sydney, John Wiley & Sons, 1965. Pp. xiii, 312. $7.95.

The trouble with the study of thinking has always been that psychologists literally did not know what they were talking about. Thought is a stream, a series of transformations, a pattern . . . of what? It has long been obvious that the images and feelings and inner speech are not themselves the real stuff of mental life, but the candidates that have been proposed in their place are all unsatisfactory. The Gestalt psychologists suggested that thinking was like visual perception, the Behaviorists treated it as if it were a chain of responses, and Freud likened it to a flow of energy, but none of these metaphors is fully appropriate. The problem cannot be avoided by taking refuge in the nervous system either: the principles of synaptic transmission do not help distinguish problem-solving from rote memory, nor do they usefully explain how a mathematician finds a proof.

The major contribution of computer technology to psychology is its answer to this question. *Information* is what is transformed, lost or recovered, used or misused. The early enthusiasts for "information theory" believed that the quantification, the *measurement* of information would be psychologically important, but time has proven them wrong. This book, like most contemporary contributions to cognitive psychology, makes no reference to "bits." Instead, it is concerned with explicit or implicit *programs* for information processing. Psychology is finally free to speak about mental processes without indirect discourse about energy, perception, response, or neural action. There is no regression to mentalism in this: information can be processed by very tangible IBM machines as well as by men. Moreover, contemporary theorizing can finally be complex enough to do justice to its subject—the computer will keep track of all our assumptions even if we ourselves cannot.

Despite the importance of information-processing concepts for psychology, there have been virtually no books on the subject written by or for psychologists. (Hunt's *Concept Learning* and Green's *Digital Computers in Research* are the only exceptions that come to mind.) Thus, Reitman's work would be of great importance even if it were merely adequate. In fact, it is an original and creative contribution to the psychology of thought.

One of the most significant parts of the book is a sophisticated discussion of role of computer simulation in psychological theory. On the whole, Reitman is more optimistic about simulation than I am, but he does not hesitate to raise some difficult questions. The complexity of a computer program makes it far less accessible to potential critics than, say, a mathematical theory would be. How, then, can these models be discussed and criticized; Moreover, how can they be "tested;" Can we set up meaningful standards for the goodness of fit of a computer simulation; If we have no such standards—and we do not—of what value are the models?

It is with a lively awareness of these problems that Reitman undertakes (1) to expound the notion of "list structure," together with related concepts, as the basic constructs of a cognitive theory; (2) to present Newell, Shaw, and Simon's program, the "General Problem Solver" (GPS) as example of the power of this notion; (3) to describe his own program, "Argus," which is very different in spirit from GPS although it is also based on list-processing; (4) to apply the concepts involved to such diverse kinds of problem solving as composing a fugue, making a silk purse out of a sow's ear, and understanding the meaning of a question. He is careful to use psychologically-relevant examples in illustrating every point. List structures are expounded with the aid of Bruner's concept-formation paradigm and Yntema's keeping-track experiment; Newell, Shaw, and Simon's concern with symbolic logic in GPS is compared to Ebbinghaus' concern with nonsense syllables; the differences between Argus and GPS are discussed with reference to Hebb's theory and the possibility of multiple trains of thought. (Reitman has been deeply influenced by Hebb, and his book contains the most sophisticated and yet sympathetic critique of Hebb's views that I have seen.)

Many readers will use *Cognition and Thought* as a digestible introduction to the computer simulation of cognition. Although it will not disappoint them, they should be warned that it is far from a complete guide to the field. Reitman introduces only what he means to use. Heuristic programming is well covered, but there is little material on pattern recognition, game-playing, or mechanical translation. The book is even less adequate as a survey of cognitive psychology; many phenomena which historically fall within that rubric are never mentioned. This is hardly a fault, since

Reitman did not intend to write a general review, but the topics he does not treat testify eloquently to insufficiencies of his theory.

In pointing out that Hebb's cell-assembly theory cannot easily account for the processes of thinking, Reitman remarks that "'Thought,' in fact, has only one index entry for the body of Hebb's book; 'thinking' and 'problem-solving' have none at all" (p. 7). By the same token, the most glaring omission in his own work is the absence of a concept of *attention*. It gets only one index entry, which refers to a quotation from Hebb! A psychology of cognition which says nothing about attention, or about immediate memory, linguistic structure, repression, and the many other topics which Reitman does not treat, is still very far from its goal. But we are all far from it, and this book is a step on the way.

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Industrial Jobs and the Worker. By ARTHUR N. TURNER and PAUL R. LAWRENCE. Boston, Harvard University, 1965. Pp. xvi, 177. $4.00.

This research monograph by Turner and Lawrence could easily prove to be one of the most important books in industrial-social psychology to appear in the last decade. The investigators planned to study the responses (both attitudinal and behavioral) of a group of industrial workers to different aspects of their jobs. The original hypotheses were that workers would respond favorably (high satisfaction and low absence rates) to jobs which were more complex, had more responsibility, more authority, more variety, etc. In short, "good" responses would accompany high level jobs. The hypothesis concerning attendance was confirmed for a sample of 470 workers from eleven industries who were working on 47 different jobs. The hypothesized positive relationship between job level and satisfaction was *not* supported. This latter finding plus the presence of one or two curvilinear relationships led Turner and Lawrence to the conclusion that the sample workers had been drawn from two separate and distinct populations of workers whose members responded in different ways to similar job characteristics. The investigators, by splitting their group of workers on a succession of variables and analyzing the relationship between task attributes and job satisfaction were able to determine that workers from factories located in small towns responded dramatically differently from workers who came from more urban settings. The workers from small town settings tended to respond to task attributes in the manner predicted by Turner and Lawrence. Workers from cities indicated no relationship between task attributes and attendance and responded with *low* job satisfaction to supposedly desirable job attributes and with high satisfaction to such "undesirable" attributes as repetitiveness and required interaction.

It would be easy to criticize Turner and Lawrence for possibly capitalizing on chance in their search for the third variable to use in identifying the two subgroups in their sample, to raise questions concerning the representativeness of their sample of city workers (who had, among other things, a curvilinear relationship between job level and pay), and to point out that the confounding of ethnic-religious backgrounds with urban-rural settings (catholic-urban and protestant-rural predomination) may be a very serious problem. These problems eventually must be dealt with by investigators following up this research. The study contains enough information bearing on them, however, so that they appear to cause only minor problems.

The major importance of the monograph lies in the theoretical explanation of these somewhat surprising findings and in the implications they have for "striving" types of motivation theories and the entire human relations approach to industrial problems. Turner and Lawrence have invoked an alienation *qua* anomie explanation and have thus placed these results of what began as a straight-forward industrial psychology study squarely in the domain of sociology. The impact of cultural variables on the response of workers to their jobs has been clearly demonstrated. Seldom has a construct with a more impressive theoretical and empirical lineage been applied to a set of data from industrial psychology. The very respectability of the theoretical explanation may be enough to convince industrial psychologists that the days of lip service to cultural and environmental variables are over and cognizance must be taken of their importance in the design of studies. Finally, these data indicate that "human relations" theories of industrial behavior and "striving" theories of human motivation must be modified to take into account differences in cultural backgrounds.

University of Illinois

CHARLES HULIN

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(The books listed here have not as yet been noted in our pages. Listing here does not, however, preclude their later review).

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READING AND TALKING BILINGUALLY

By PAUL A. KOLERS, Massachusetts Institute of Technology

The probability of recalling some word from the interior of a long list is proportional to the number of times it appears.[1] This effect occurs even when the lists and the *S*s are bilingual: an item is recalled equally well when its translation is substituted for some of the repetitions, despite the fact that item and translation may be visually and phonetically different.[2] From this result it was argued that what is stored in short-term memory is not a word in a particular language—a visual or phonetic template—but a concept or meaning. The present experiments explore this notion with tasks other than the recall of lists; namely, reading and talking.

A language is a coding system which has both taxonomic and functional properties: it provides names (or codes) for classes of objects and concepts, and it indicates relations among the names. The rules of relation are specified by the grammar of the language, each language having its own.[3] Since every language is a coding *system*, we might expect that elements in a bilingual person's two languages are not randomly intersubstitutable. This expectation has been confirmed by field studies among bilinguals, the typical finding being that *S* tends to keep his two coding

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[1] Nancy Waugh, Immediate memory as a function of repetition, *J. verb. Learn. verb. Behav.*, 2, 1963, 107-112.

[2] P. A. Kolers, Interlingual facilitation of short-term memory, *ibid.*, 5, 1966, 314-319.

[3] See, for example, G. A. Miller, Eugene Galanter, and Karl Pribram, *Plans and the Structure of Behavior*, 1960, 1-226; Noam Chomsky, *Aspects of the Theory of Syntax*, 1965, 1-251.

systems relatively separate. While intrusions from one language sometimes occur when the other is spoken, they are reported to occur principally at syntactic boundaries rather than randomly, and items from the second language usually are deformed into the syntactic or phonological arrangements of the principal one.[4] It appears then, that when one is transmitting information in some language L_1, using elements of L_2 involves something more than mere element-substitution. The 'something more' may be thought to represent the sets of rules specific to each language that *S* normally, and often unconsciously, uses—rules associated with the syntactic, semantic, and phonological properties of language.

From these considerations, and others about code-switching, one might expect that requiring a bilingual to switch languages while he is working on a single message would impair his performance.[5] One implication of this expectation is that the facilitative effect described above[6] may be restricted to list-learning; alternatively, however, different kinds of language-use—*e.g.* reading, talking—may involve substantially different processes in the use of linguistic codes. Another way of stating this point is in terms of the symmetry of operations involved in the use of coding systems. The question is whether the decoding and encoding of language are substantially similar processes that run off in reverse order ('analysis by synthesis'). At least one model of language-use suggests such a 'mirror symmetry' of operations for inputting (decoding) and outputting (encoding) language.[7] In these experiments we show that the effect of the linguistic form of material varies with the nature of the task *S* is required to perform.

COMPREHENSION

In the first experiment, bilingual *S*s were tested for comprehension of material presented in several linguistic forms.

Materials. Six non-consecutive passages were selected from each of six stories in the dual-language Bantam Edition of *French Stories;* they were each approximately 110 words long. Each passage was prepared in each of six linguistic forms. In the unilingual form, the entire passage was in English or in French; in the alternating form, alternate sentences were in English or French, in half of the passages the first sentence in English and in the other half the first sentence in French; and in the linguistically mixed form, words were haphazardly in English or French, half of the passages favoring English word-order and the other half favoring French. The two examples of a linguistically mixed passage illustrate a predominantly English and predominantly French word-order.

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[4] A. R. Diebold Jr., Code-switching in Greek-English bilingual speech, *Georgetown Univ. Monog. Ser. Lang. Ling.*, No. 15, 1963, 53-62; Uriel Weinreich, *Languages in Contact*, 1963, 1-148.

[5] A. M. Treisman, Verbal cues, language, and meaning in selective attention, this JOURNAL 77, 1964, 206-219; Contextual cues in selective listening, *Quar. J. exp. Psychol.*, 12, 1960, 242-248; D. A. Broadbent, *Perception and Communication*, 1958, 1-338.

[6] Kolers, *op. cit.*, 314-319.

[7] Morris Halle and K. N. Stevens, Speech recognition: A model and a program for research, *IRE Trans. Prof. Group Inf. Theory*, 8, 1962, 155-159; Remarks on analysis by synthesis and distinctive features, in W. Wathen-Dunn (ed.) *Symposium on Models for the Perception of Speech and Visual Form* (in press).

English word-order: His horse, followed de deux bassets, faisait la terre résonner under its even tread. Des gouttes de verglas stuck to his manteau. Une violente brise was blowing. One side de l'horizon lighted up, and dans la blancheur of the early morning light, il aperçut rabbits hopping at the bord de leurs terriers. Tout de suite the two hounds rushed sur eux and, vivement throwing them back and forth, brisaient leur échines. Bientôt il came to a forest. In a tree, au bout of a branch, un wood grouse, numbed par le froid, dormait with its head under l'aile. D'un revers of his sword he cut off its deux pattes, et continua sa route sans picking it up. Trois heures après, il was on the top of a montagne.

French word-order: Son cheval, suivi by two hounds, en marchant d'un pas égal, made resound the earth. Drops of ice se collaient à son cloak. A wind strong soufflait. Un côté of the horizon s'éclaircit; et, in the whiteness de crépuscule, he saw des lapins sautillant au edge of their burrows. Les deux bassets suddenly se précipitèrent on them; and ça et là, quickly of them broke the backs. Soon he entra dans un bois. At the end d'une branch, a coq de bruyère, engourdi by the cold, slept, la tête sous its wing. With a stroke d'épée, il lui faucha the two feet and without le remasser went on his way. Three hours later, il se trouva sur la pointe d'une mountain.

Each passage appeared once in each of the six forms, and each was assigned one to each of six sets of passages. Six different passages in the same linguistic form constitute a packet; six packets, one for each of the linguistic forms, make up a set of 36 different passages. The passages were placed in the same order within sets; the order of the linguistic form in which they appeared was counterbalanced across sets by means of a 6 × 6 Latin square. Half of the passages in each packet were followed by questions in English, the other half by questions in French. In addition to the 36 test-passages, six more were chosen from newspapers and magazines. These were prepared in the same way as the others and were assigned one each to the appropriate packets, but were used only to give *S*s practice.

Tests of comprehension. It is difficult to measure 'comprehension' with paper-and-pencil tests, for very often one has to supply *S* with some of the information for which he is being tested. In preliminary studies we tried true-false tests, sentence-completions, fill-ins, and direct answers, without obtaining significant differences in the relations among the results. The data reported here were obtained with two different tests. Half of the *S*s wrote answers to each of eight questions about the contents of each passage they read. These were written in the language in which the questions were asked. The other half took a more objective test, only indicating with a *plus* or *minus* whether each of 15 questions was answerable on the basis of the passage just read. Eight of the 15 were the same questions presented to the preceding group and seven were filler-items.

Procedure. The *S*s were allowed exactly 3 sec. per line to read each passage. The passages were 11±2 lines long, allowing 33±6 sec. to read. The *S*s who answered *questions* were allowed exactly 2 min. to answer the eight questions to each passage. The *S*s who indicated *possibilities* (recognition-test) were allowed exactly 45 sec. for each set of 15 questions. How much time to allow was determined in preliminary experiments; the values selected gave *S* enough time to read each passage but not enough time to study it carefully or memorize it.[8] Rest-periods were provided after each packet. The *S*s were tested individually.

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[8] The time allowed approximates a reading rate of about 200 words per minute, while unilingual undergraduates read short stories at about 300 words per minute on the average. In preliminary tests in which *S* was allowed less time to read, all scores were lower, but the relations among them were not different from those

Subjects. Twelve Americans whose native language is English and 12 Europeans whose native language is French were the *Ss*. The Americans had lived in a French-speaking country for at least nine months and reported having a good knowledge of French. The Europeans had been in the U.S. for at least nine months. All of the *Ss* were students at Harvard or M.I.T., and English was more actively used by the Europeans than French was by the Americans.

Results. The answers the *Ss* wrote were scored first with a strict criterion (exactly correct) and again with a more lenient one (approximately correct). The results of the tests are presented in Table I in terms of the percentage of correct answers to each of the six linguistic types of material, separately by form, scoring criterion, and group of *Ss*.

TABLE I

PERCENTAGE OF ANSWERS CORRECT FOR PASSAGES IN VARIOUS LINGUISTIC FORMS

	Unilingual		Alternating		Mixed	
	English	French	English*	French*	English†	French†
Strict:						
Americans	62.5	49.0	62.5	62.5	57.5	51.2
Europeans	45.0	57.5	50.0	55.0	48.8	53.8
Lenient:						
Americans	67.5	52.5	67.5	66.2	62.5	55.0
Europeans	51.2	61.0	56.0	59.0	55.0	57.5
Recognition:						
Americans	86.0	85.3	90.0	88.0	86.7	83.3
Europeans	82.0	84.0	85.3	86.7	82.7	86.0

* Language of the first sentence.
† Favored word-order.

The principal fact of the table is that the numbers within rows are approximately of the same size; differences due to linguistic form are small. Among the *Ss* who wrote answers, the largest differences are always between the unilingual passages in native language and foreign language; performance on the alternating and mixed forms falls between. Analysis of variance of the scores summarized in Table I revealed that the differences between groups (Europeans and Americans) and between types of text were not significant statistically ($p > 0.10$); but the interaction between groups and linguistic forms was significant ($p < 0.05$), indicating that the *Ss* do better with texts that favor their native syntax.

Scores on the objective test of recognition are higher than scores for the questions; yet in this test, too, no significant difference between the

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reported here. Hindsight suggests that it would have been better to find each *S*'s own reading rate and present the materials accordingly.

materials or the groups of *S*s exists, and the interaction between group and linguistic form that approached significance with the questions test is here nonsignificant.

Since half of the passages in one language were followed by questions in the other, the scores were analyzed with respect to the linguistic sequence of passage and question; the results are shown in Table II. Scores are highest when both passage and questions are in the native language. When passages are in one language and answers are written in the other, the *S*s do slightly better when reading in the native language and writing in the foreign language than the reverse. Analysis of variance reveals, however, no statistically significant difference among the four sequences, although again the interaction between linguistic type and group is significant ($p < 0.05$).

TABLE II

PERCENTAGE OF CORRECT ANSWERS IN THE VARIOUS PASSAGE-QUESTION SEQUENCES

	English-English	French-French	English-French	French-English
Strict:				
Americans	65	59	61	40
Europeans	44	64	48	51
Lenient:				
Americans	69	61	68	42
Europeans	50	68	52	56
Recognition:				
Americans	85	88	87	83
Europeans	81	85	83	82

READING ALOUD

The linguistic form of connected discourse has only a trivial effect upon comprehension of textual material. In this and the remaining experiments, we studied whether other kinds of language-use were immune to influences of the form of the material.

Method. Twelve of the *S*s who participated in the experiment on comprehension were tested further by reading aloud two passages in each of the six linguistic forms they had previously seen. The passages were among those for whose comprehension they had tested—in each case, the first two passages in each linguistic form. The *S*s were instructed to read aloud as rapidly as they could without regard to the fine points of phrasing, expression, or meaning. Their performance was recorded on tape for subsequent analysis. Half were Europeans, half Americans.

Results. The amount of time the *S*s needed to read the passages aloud varied markedly with the linguistic form of the passage. Table III shows the results, averaged for the two passages each *S* read, and normalized for a passage of 11 lines. Passages in the native language were read most rapidly, and mixed material most slowly. Passages of alternating sentences fall exactly midway between the unilingual ones. Analysis of variance of these data yielded significant *F*-ratios for linguistic form and for the form x group interaction ($F = 18.15$ and 12.59, respectively, $df = 5/50$, $p < 0.01$).

The textual material was so composed that one page of French and one page of English between them have approximately the same number of words in each language as two pages of mixed material. Averaging the results separately for unilingual, alternating, and mixed text relates performance to a fixed quantity of material in each language. These averages also are shown in Table III.

TABLE III

TIME (IN SEC.) REQUIRED TO READ ALOUD PASSAGES IN VARIOUS LINGUISTIC FORMS‡

	Unilingual			Alternating			Mixed		
	English	French	Mean	English*	French*	Mean	English†	French†	Mean
Americans	30	47.4	38.5	38.4	39.6	39.0	49.2	51.6	50.4
Europeans	41.4	30.0	35.7	36.6	36.0	36.0	45.0	45.0	45.0

* Language of the first sentence.

† Favored word-order.

‡ Raw data were obtained using a stop watch calibrated in hundredths of a minute. Rounding errors create minor discrepancies between Tables III and IV.

Approximately 29.5 transitions between sequences of French and English words occurred in each passage of mixed material, and about 4.5 transitions in the alternating material. (A transition in the latter case is between sentences.) We calculated for each *S* the average amount of time it took him to read unilingual material (French and English), subtracted that from the average amount of time for mixed material, and divided the difference by the number of transitions in the passage of mixed material. The values found by this calculation range from 0.24-0.66 sec. among the Americans, and from 0.12-0.48 sec. among the Europeans, with averages of 0.36 sec. for the Americans and 0.30 sec. for the Europeans.[9] The cost of a transition, the quantity of text that would have been read had no transition occurred, is given by $\delta\ (r_e + r_f)/2$, the average reading-rate for English and French multiplied by the switching time; it is equal to 1+ words.

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[9] An *SD* was calculated for each *S*'s four scores on 'switching time.' Their average was 0.16 sec. for Americans and 0.15 sec. for Europeans. Individual means and sigmas were not substantially correlated.

The relatively small number of transitions in a passage of alternating text precludes making a similar analysis for that material with any confidence.

There is a correlation between calculated switching time and speed of reading. The *S*s who read mixed material more slowly necessarily appear to have longer switching times. For example, the 12 *S*s who participated in the comprehension-tests but not in the reading tests just described were tested on other aspects of language-use. In the course of those tests they read aloud one passage in each of the six linguistic forms, the passages used as practice-material in the comprehension-tests. Those *S*s were not prompted to read rapidly. Table IV shows the reading-rates in words per minute for the *S*s of the preceding analysis who were reading as rapidly as they could, and for the unprompted *S*s. The rates for the unprompted *S*s are lower, but the relations in the data obtained from the different texts are identical in the two cases. Based on a smaller sample of passages, the switching time for the unprompted *S*s is 0.6 sec. for the Americans and 0.54 for the Europeans. Thus, when *S* paces himself, he generally proceeds more hesitantly, and his switching time may *appear* to be longer. Prompting induces him to read more rapidly and decreases his apparent switching time. Among the 24 *S*s we tested, switching time ranged from 0.1-0.6 sec., depending on linguistic competence and on motivational variables, with an average in the region of 0.3-0.5 sec.

TABLE IV

READING-RATES (WORDS PER MIN.) FOR PROMPTED AND UNPROMPTED *S*S

	Unilingual		Alternating		Mixed	
	English	French	English*	French*	English†	French†
Prompted:						
Americans	228	145	190	177	144	137
Europeans	160	219	180	189	147	149
Unprompted:						
Americans	171	128	156	136	112	110
Europeans	152	185	153	179	122	129

* Language of the first sentence.
† Favored word-order.

One factor that contributes to the variability of switching time is that the *S*s made errors when reading aloud. Some of these were corrected, more by Americans (59%) than by Europeans (22%). (These errors are subjected to detailed analysis in a later section of this paper.) Since many more errors were made in the mixed than in the unilingual passages, the time spent correcting errors affects the magnitude of the difference between reading time for unilingual and mixed passages. A less variable measure of switching time would have been obtained if all the *Ss* had been prompted to read as rapidly as possible and a non-biasing method of compensating for errors were available.[10]

PRECIS

The few studies of code-switching in bilingualism that have been made have been of the passive type, in which *E* only notes the intrusions from one language that occur while *S* is speaking the other. To study interference-errors in production, we required *S* to generate mixed speech. In preliminary experiments, *S* was requested merely to talk this way about any subject he wished, but most of the *Ss* found the situation too artificial and so generated only a few words. We subsequently used the method of giving the *Ss* passages to read from newspapers or magazines that they had not previously seen, and requested that they make precis of the passages in unilingual, alternating, or mixed form. The question under study was what the effect is of code-switching in conditions of 'free' production.

Method. Passages approximately 100 words were taken from feature articles in newspapers and magazines. Some were left in unilingual form, others were prepared in mixed form. *S* was allowed approximately 30 sec. to read each article silently and then made a spoken precis of it. Unilingual precis were made from mixed or alternating texts, and alternating or mixed precis from unilingual text. For practice in talking in the various forms, *S* read aloud one page of news article in each of the linguistic forms he was subsequently to use. Three European and three Americans were tested. They had taken part earlier in the comprehension-tests, but were not among those whose data constitute the principal part of the section on 'Reading Aloud.'

Results. The principal result describes differences in rate at which language is produced under the various conditions. These data are shown in Table V for the two unilingual passages separately, and for the alternating and mixed passages each combined. When reading aloud, the rate for alternating sentences falls midway between the two unilingual rates, as shown in Tables III and IV. For precis, the rate for alternating sentences is approximately the same as the rate for the unilingual foreign language, as shown in Table V, while the rate for mixed speech is considerably less than the average of the unilingual rates (approximately 30% less for

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[10] The possibility arises that 'switching time' is an artifact of the time spent correcting errors. To test this possibility, the passages were found on which *S* either made no errors or failed to corect them. The mean switching time computed from these passages (which comprised just 50% of the total read) was 0.29 sec. for Americans and 0.31 sec. for Europeans. Thus, correcting errors affects the calculated value of switching time but is not its cause.

Americans and 40% less for Europeans). The average length of string the *Ss* generate in one language before switching is 21 words in the alternating form and 5.5 words in the mixed form. Calculation of a switching time for the mixed text here yields a value of 1.44 sec. for the Americans and 1.08 sec. for the Europeans, about three to five times longer than switching time for reading aloud, and equal to 2.2-2.5 words of averaged unilingual text.[11] Thus, mixing languages while talking has an inhibitory effect on the rate. Another indication of the inhibitory effect of code-switching on production is suggested by a rank-order correlation between production-rate and string-length: The *Ss* whose rate is higher also tend to produce longer strings in one language (have fewer transitions), although Spearman's coefficient (0.66), is not significant ($p \cong 0.13$ with $N = 6$).

TABLE V

TALKING-RATE (PRECIS, WORDS PER MIN.) FOR VARIOUS LINGUISTIC FORMS

	English	French	Alternating	Mixed
Americans	122	84	82	71
Europeans	95	125	91	66

Four principal kinds of error occur in the precis: errors of word-choice, errors of tense-agreement, omissions, and redundancies. Since the detection of the first of these is somewhat subjective, a conservative criterion was used: only a marked departure from good usage was counted as an error, *e.g.* "Since 10 years ago," or "I wanted to maintain studying." Errors in tense-agreement occurred both within and between sentences. Omissions were typically of articles and prepositions. Redundancies were scored as repetitions of the same information within a single sentence.

The average frequency of occurrence per *S* of these four kinds of error is shown in Table VI for the various linguistic forms used in the precis. Word-choice errors are more frequent in the foreign than in the native language; their number, however, is, if anything, slightly smaller in the alternating and mixed forms than in the average of the two unilingual forms. Perhaps part of the process of speaking more slowly in the alternating and mixed forms (Table V) is a greater precision in the choice of words. Errors of omission and of tense-agreement show no consistent relation to the linguistic form of the spoken message, but *Ss* tend to be more redundant when using the alternating and mixed forms than when using the unilingual ones. When the frequency of occurrence of word-choice errors was tallied for the eight principal parts of speech, the errors were found to occur in approximately equal numbers among all of the parts of speech and for the mixed, alternating and unilingual forms.

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[11] This does not mean that a 1.3-sec. pause occurred at every linguistic transition. Often, pauses at a transition were no longer than pauses between words in a unilingual sequence, a fact which may have considerable consequence for models of language-production. Presumably, *S* prepares words he will speak at the same time that he is speaking others. The 1.3-sec. interval involves both preparation and execution, and may perhaps best be regarded as a general cognitive cost of switching.

One kind of distortion not heretofore reported in studies of bilingualism is intrusion within a relatively low-level syntactic unit. The spoken precis were examined for the occurrence of prepositional phrases of the form preposition-noun and preposition-article-noun (*e.g.* "of collisions, d'assurance, sur la route, with stationary objects"). About 90% of these phrases were unilingual; but about 1% of the two-element phrases were mixed linguistically, about 7% of the three-element phrases took the form *EFF* (the preposition in English, article and noun in French), and a few cases of *EEF* and *FFE* also occur. Thus, as will be shown again below, the syntactic boundary of a *prepositional* phrase, while strong, is not impervious to external influences.

TABLE VI

Average Number of Errors in Making Precis

	Word-Choice	Tense	Omission	Redundancy
Americans:				
English	4.0	0.5	0.7	1.0
French	4.8	1.2	1.7	1.5
Alternating	3.5	1.2	0.8	3.3
Mixed	3.5	1.6	0.8	2.5
Europeans:				
English	7.7	1.8	2.0	2.7
French	1.0	0.7	0.5	1.2
Alternating	3.8	0.7	1.6	2.3
Mixed	3.0	0.7	1.5	3.2

EFFECTS OF PRACTICE

Comprehension of text in a given time is independent of the linguistic form of the material; but the amount of time needed to make precis varies with the linguistic form used in speaking. At least two variables may be critical for the latter effect: availability of vocabulary or a motor difficulty involved in making the unfamiliar sequences of sounds that constitute mixed linguistic forms. In this experiment, the influence of vocabulary was studied by providing the *Ss* with the text in some form while they spoke in mixed form. Thus, *S* might have the English or the French text before him while he generated mixed speech; or he might have both texts with English and French arranged in alternate lines.

Method. The same material that was used in the preceding section was used again here. Passages appeared in English only, in French only, or in both languages interlinearly. *S* held the passage in hand while generating mixed speech. The *Ss* were the remaining six of the 24 who participated in the comprehension-tests. After performing in this way, with two passages in each form, *S* was invited to make a brief autobiographical statement in mixed speech.

Results. The first three columns of Table VII show the rate in words per minute at which the *Ss* generated precis in mixed speech while being able to see passages in the form indicated by the column-headings. Having the French text available facilitated the rate at which the Americans generated mixed speech, compared with having the English text available; and both texts available produced even better performance. The European *Ss* generated mixed speech at approximately the same rate, irrespective of the kind of text available. Furthermore, the practice provided by making precis with the text available did not produce an increase in the rate of generating mixed speech freely. The fourth column of Table VII shows the rate at which autobiographical material was produced in mixed form; the numbers there are about the same as the numbers in Table V for mixed speech, but the number of switches made in mixed speech is different for the two groups of *Ss*. The *Ss*, as shown in Table V, switched languages after every 5.5 words on the average, while the *Ss*, shown in Table VII, switched language every 3.9 words on the average when they were generating autobiographical material, and considerably more often when a text was available. In the latter case, they switched language every 2.9 words when unilingual text was available; and every 1.5 words when the text was present in both languages. Thus, when there are no problems of vocabulary-availability (*i.e.* memory-search), output-rate may not increase; but the effect of practice is to enable *S* to make the length of a string in one language shorter (*i.e.* to make more switches), and switches take time.

TABLE VII

PRODUCTION-RATE (WORDS PER MINUTE) OF MIXED SPEECH WITH TEXT AVAILABLE

	English	French	Both	Free
Americans	69	80	92	67
Europeans	63	62	63	73
String-length	2.8	2.9	1.5	3.9

The suggestion of a correlation between speech-rate and string-length mentioned in the preceding section is present here too; when the data for the two sets of *Ss* are combined, the one group making a precis in mixed speech and the other speaking autobiographically, $\rho = 0.55$ ($p < 0.05$). Furthermore, production-rate for the six *Ss* who had both texts available while they generated mixed speech also tended to correlate with string-length, though not significantly $\rho = 0.77$ ($p \cong 0.10$). These results imply that switching languages takes a finite time that is independent of vocabulary-search. What still is unknown, however, is whether memory-search is itself impaired independently of switching rate when *S* is required to generate mixed speech freely.

The fact that *S* tends to generate a longer string when speaking without text (the fourth column of Table VII) might suggest that certain sequences of words or complex syntactic forms would tend to be spoken in one language. The following selections show, however, that the latter is not inevitably the case, for no grammatical unit consistently remains inviolate. The first was spoken by an American, the second by a Frenchman.

> It has often occurred à moi sometimes dans le matin to look out of la fenêtre and listen to les oiseaux who were singing dans les branches of a tree ce qui se trouve very close à ma mansarde. When I took at cet arbre in the various heures du jour, I often think d'un pays lointain where in the midst d'une grande forêt vièrge one hears le sourd piétinement of animals and the sifflement of snakes as they glide les grands troncs to le sol.
>
> Well I have been ici for quatre ans maintenant and je m'y plais very well. I am going to retourner en France pour un length de je ne sais pas combien de temps but it is certain que je vais aller to this country because je m'y suis plus tremendously. . . . I wonder un petit peu what is the reason pour cet experiment. I do not see jusqu'à maintenant le but of mixed language parce que je crois it is very difficult à parler, to read, et à comprendre. It takes some effort and is a little bit amusant but I am sure il y a quelques très bonnes reasons behind it.

Note the predominantly English word-order in the first passage even when French words are used (with the exception of *forêt vièrge*) and the more French word-order of the second. In this part of the experiment, the six *S*s generated 60 two-element prepositional phrases; in only one was the preposition in one language and the noun in the other. Among 55 three-element phrases, 35 were unilingual, but 13 were *EFF*, while 3 were *FFE* and 4 were *EFF*. As in the preceding section, so here too the greater number of prepositional phrases are found to be unilingual, but if the unit is not unilingual, article and noun are more likely to be in the same language than preposition and article. This contrasts with what is reported below for reading aloud.

ANALYSIS OF READING-ERRORS

Having revealed asymmetries between the comprehension and production of mixed linguistic forms, we turn now to an analysis of errors in reading. Table III shows that the *S*s require more time to read linguistically mixed than alternating or unilingual material. In addition to requiring more time, they also make errors. These are of three kinds principally: phonological, translational, and order-errors.

Phonological errors. Two kinds of phonological 'errors' are made when the *S*s read aloud in mixed linguistic form. Words of one language often are pronounced as if they were members of the other language. For example, some Americans reading

"Deux trous in the wall servaient de fenêtres" pronounced the verb as the English verb *survey;* and reading "A small barbiche black" pronounced the last word as the English word *block.* Some Europeans reading "Plus tard came nuages de moustiques" pronounced *nuages* as the English *new ages;* reading "A peacock spread its queue sur le gazon" pronounced *queue* as the English *kew;* and in reading "solid en or hanap" pronounced the last three words as English. That bilinguals should pronounce foreign words to conform to their native language is not surprising; but that they should pronounce common words of their native language as if they were foreign words is a phenomenon of another order. Since detecting such phonological errors is a highly subjective task, only a few of the more outstanding examples have been cited, for they seem to identify a basic process in reading which is discussed below.

Another kind of phonological error occurs which is even more difficult to quantify. In the course of reading mixed linguistic texts aloud rapidly, both American and European *S*s frequently use an accent that is different from the accent they use when they are reading unilingual texts. This new accent involves features not noticeably present in their unilingual productions as well as features found there. Just as the pronunciation-errors instanced above do not necessarily favor *S*'s native language, so too with this new accent, with which the pronunciation of words both in their native language and in the foreign language is degraded. This new accent is heard only when *S* reads linguistically mixed material, and never when he reads alternating or unilingual text. Furthermore, it is heard only when *S* is reading aloud and never when he is making precis or speaking freely. Quantitative analysis of these errors would require a clear phonology for each *S*'s unilingual English and French accents. It is clear from an analysis of the tapes, however, that accent is more likely to be in error for some word when the preceding words are in the other language: an English word is more likely to be mispronounced when the antecendent string is in French, and vice versa. Thus, accent appears to be determined for sequences of words independently of their being recognized as members of a language.

TABLE VIII

TALLY OF TRANSLATIONAL ERRORS IN READING ALOUD FOR VARIOUS PARTS OF SPEECH

	Noun	Verb	Adverb	Adjective	Article	Conjunction	Pronoun	Preposition
Americans	12	4	2	4	18	5	7	11
Europeans	16	5	0	2	19	5	7	17

Errors of translation. A class of error more readily quantified is that of translation. Among the 48 passages of mixed text that were read aloud (four passages by each of 12 *S*s), 134 translations were made of individual words; there were, in addition, 10 in alternating text, and 2 in unilingual text. The Americans made 63 of the 134, and the Europeans 71. These errors do not favor one language; for both groups of *S*s, approximately the same number of translations was made from English to French as from French to English. A count of these errors according to the principal parts of speech is shown in Table VIII. Unlike the substitution-errors in free speech described earlier, these translational errors in reading aloud concentrate in particular parts of speech, namely, nouns, articles, and prepositions, which among themselves account for about 65% of all errors.

A few examples of translational errors are the following, the word in parentheses showing what was substituted for the italicized word: et battit les oeufs *for* an omelette (pour); a la porte *of his* cell (de sa); then a riotous *vent* soulevait (wind); assis at his desk, his *hand* encore nerveuse (main); and placed *in* un ombreux cachot *with* triple locks (dans, avec). About three-fourths of these errors were 'inertial'—a word in one language was translated into the language of the words preceding it. The first three examples are of this form. About 15% of the errors were 'anticipatory'—the last word of a sequence in one language was translated into the language of the words following it. The fourth example illustrates this error, and the fifth illustrates both kinds in one clause. The remaining 10% of errors were ambiguous—a single word in one language flanked by words in the other was translated to conform. One cannot say whether the antecedent or the subsequent words played the greater role in stimulating the translation.

Since more than half of the translations occurred among nouns, prepositions, and articles, we examined one simple syntactical unit—the prepositional phrase. As with the analysis of free speech, attention again was restricted to two-, three-, and four-element phrases of the form preposition-noun, preposition-article-noun, and preposition-article-adjective-noun. Translational errors among such prepositional phrases account for about 78% of all errors in which these four parts of speech occur.

There are four ways in which a two-element phrase may be permuted, letting *F* and *E* stand for English and French words: *EE, EF, FE, FF*. There are eight permutations for three-element phrases, and 16 permutations for four-element phrases. The text the *Ss* read did not, however, represent all the possibilities equally; more than half of the prepositional phrases in the text were unilingual, the greatest number in the three-element form. Consequently, the findings to be reported are restricted to the three-element form and must be read for their qualitative rather than their quantitative import.

The greatest number of translations were of unilingual prepositional phrases. Phrases of the form *FFF* and *EEE* in the text were translated in whole or part (*FFF* becomes *EEE* or *EEF*; *EEE* becomes *FFF, FFE,* and so forth). In every case in which these errors occurred, the word preceding the unilingual phrase was in the other language. When the text read *FEE* or *EFF* and a translation was made, the tendency was to translate the article to the language of the preposition: translations were made in one-third of the phrases of these forms and all went the same way. When the text read *FFE* or *EEF,* the tendency was to translate the noun to conform: translations were made in approximately 19% of the phrases having these forms, and in all cases but one this result was observed. Only two 'mixed' sequences occur in the text (*FEF* and *EFE*), and neither of these was translated. Thus, these results show that a syntactic boundary formed by a three-element prepositional phrase does not force all of the elements within it to be read in one language, nor does it insulate unilingual phrases from influences outside itself.

Order-errors. The text the *Ss* read contained 30 cases in which a sequence of words in one language violated normal syntactic arrangement. The following are examples of such structurally ambiguous phrases, the syntactically correct form shown in parentheses: in a cell dark (in a dark cell); their clothes wet (their wet clothes); leur mouillés habits (leurs habits mouillés); solid en or hanap (solid gold goblet *or* hanap en or massif). About 20% of such structural ambiguities were 'rectified' by the reader, who made the words conform to their normal syntax. In other cases the English *to* was dropped before a French infinitive, *e.g.* "qui semblait ne to correspondre" was read "qui semblait ne correspondre." In still other cases, a phonological error was made that deprived the text of semantic sense but preserved a correct syntactic arrangement, *e.g.* "leurs mouillés habits" was read with the last as an English word, making "their clothes wet" read as if it meant "their wet customs." Similarly, "solid en or hanap" was read as if all the words were in English. Thus, among the structurally ambiguous phrases in which errors were made, phonological and semantic sense often were subordinated to syntactic requirements.

DISCUSSION

The results may be summarized as follows: Bilinguals comprehend material in their native language better than material in the foreign language, but connected discourse made by sampling haphazardly from each of a bilingual's two dictionaries is comprehended virtually as well as the average of the unilingual texts. This result was obtained with tests based on *S*'s written answers to questions and also with objective tests (the recognition-test). The small variations that are found in the results are probably attributable to idiosyncrasies of knowledge and practice rather than to variations in the linguistic form of the passage.

When *S* is required to read passages aloud, however, he takes longer to read linguistically mixed material, while he reads alternating text in about the same time as the average of the two unilingual forms. The differences in time were used to compute a phonological code-switching time, which was found to be 0.3-0.5 sec., varying with motivational state and, presumably, with linguistic skill. When *S* speaks freely or makes precis of the text he has read, the switching time is three to five times greater, about 1.3 sec. Thus, code-switching is inhibitory for production but irrelevant to comprehension.

An analysis of the grammatical errors in free speech revealed that *S* does not make more errors when speaking in mixed form than he does in the average of the unilingual forms. Examination of the linguistic bond between the elements of spoken prepositional phrases shows that in 'free speech' the bond tends to be stronger between article and noun than between preposition and article. Reading linguistically-mixed material aloud, *S* made errors of three kinds—phonological, translational, and ordinal. Words in one language often were read with the accent of the other, and sometimes *S* invented a new accent that had features not normally heard in either of his unilingual productions. Many words presented in one language were translated into the other. The greater number of translations were inertial: S translated words from L_2 to conform to the antecedent string in L_1; but a moderate number were anticipatory. Ordinal errors were made when S rectified a deliberately disordered sequence in the text to conform to its normal syntactic arrangement. Examination of the prepositional phrases in which translations were made indicate again that intrusions may occur within such a linguistic unit, but, in contrast with what was found for free speech, the bond between article and preposition here appears to be stronger than the bond between article and noun. Thus, even simple syntactic boundaries are permeable.

The finding that bilingual *S*s can derive information from linguistically mixed text as well as they can from unilingual text can be taken as support for the belief that words are perceived with respect to their symbolic or conceptual relations, and not their linguistic form as templates or things in themselves. The skilled reader seems able to 'see through' the objective form of the graphemes to the concepts they represent and to the relation of the concepts to the others that form the message he is decoding. These results confirm and extend the earlier findings of an essentially semantic storage of concepts in short-term memory.[12] The limits of the extension are, however, not yet known. The present results indicate only that when the syntax of two languages is similar, as is the case with English and French, information can be derived as readily from a mixture of the two dictionaries as from one alone. This result contrasts with other forms of language-use described below and suggests a basic asymmetry between comprehension and production of language.

When reading linguistically-mixed text aloud, S usually cannot predict with probability greater than 0.5 the language in which a word will appear following a word he is articulating. The unpredictable phonological sequences slow him down. The increment in time to read mixed text aloud can be attributed at least in part to the time required to switch between an English-generating and a French-generating phonological program, which we find is of the order of 0.3-0.5 sec. Similarly, when speaking freely, S's bilingual speech is slower than his unilingual speech. One possible source of the slowing down in speech is the time required to make the decision of where and how to mix languages. Whereas in reading aloud S needs only to categorize a word into its language and switch in the appropriate rules of articulation, when speaking freely he must make decisions about what to say and about when and how to mix his speech, as well as switch in the appropriate rules of articulation. Thus, the 1.3-sec.

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[12] Kolers, *op. cit.*, 314-319.

interval found for free speech includes both switching time and the time to make decisions. Consequently, the lesser value, in the region of 0.3-0.5 sec., probably is a better estimate of switching time *per se* than is the greater.

When we provided *S*s with the total vocabulary needed to make precis, they made their phrases in one language shorter, *i.e.* they mixed more, and at least the American *S*s spoke more rapidly. Thus, a second possible source of the slowing down in free speech is given by the suggestion that memory search may itself be impeded by searching in two dictionaries. This notion would be consistent with an earlier finding that in some cases internalized information is in linguistically tied forms which have separate long-term storages.[13]

Practice in speaking a mixed language enabled *S* to generate it somewhat more readily (Tables V and VII). Mixture here contrasts with what is reported to occur among peoples living in border regions, who readily use forms from either of the languages they know.[14] In such cases, what appears to happen is that forms from the second language are intermixed with forms from the first, one language dominating; thus, in effect, the forms from the second language become vocabulary-items in the first, or alternate ways of phrasing information.

The process of reading aloud appears to be very different from speaking. One way of conceptualizing the reading process is in terms of the discrimination of graphemes and the automatic translation of grapheme into phoneme (symbol into sound),[15] but our results are inconsistent with this notion. To make a phonological switch, *S* first must recognize English words as English and French words as French. That is to say, to perform accurately *S* first must make a categorial identification, not a mere symbol-sound translation, and only then can he proceed according to the language-specific phonological rules. It is not now known what the criteria for categorial recognition are. Diacritical marks clearly distinguish French from English words; yet many visually-accented words were pronounced as though they were English, especially when the items differed in the two languages only by an accent-mark (*e.g. pâle, précaution*), although many uniquely French words were also mispronounced, (*e.g.*, a Frenchman pronounced *flânerie* as though it were the English word (*flannery*). In some cases, letter-sequences unlikely for a language were given the accent of that language (*e.g.*, an American made *warden* into *vahrdhan*, and a Frenchman said *gardera* as an English word). An American pronounced *moats* as the French *mots*, and a Frenchman pronounced *murs* as the English *moors*. These events suggest that the phonological rules for symbol-sound translation are relatively independent of the rules related to item-identification in using language; therefore, symbol-sound relations explain nothing about comprehension.

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[13] Kolers, Interlingual word associations, *J. verb. Learn. verb. Behav.*, 2, 1963, 291-300.

[14] Weinreich, *op. cit.*, 1-148. Diebold, Incipient bilingualism, *Lang.* 37, 1961, 97-112.

[15] E. J. Gibson, J. J. Gibson, A. D. Pick, and Harry Osser, A developmental study of the discrimination of letter-like forms, *J. comp. physiol. Psychol.*, 55, 1962, 897-906; E. J. Gibson, Learning to read. *Science,* 148, 1965, 1066-1072.

More-than-automatic symbol-sound translation also is suggested by the fact that *S* does not always say what is printed, nor does he proceed on a straightforward left-right scan. Rather, he translates words from one language into the other, normalizes disordered syntactic sequences, anticipates accent, and the like. Judged on the basis of the location of errors, however, reading aloud is predominantly serial, although not exclusively so, for 15% of translational errors can be attributed to the succeeding word. Thus, with respect to the skilled reader, the explanatory power of the grapheme-phoneme notion probably is restricted to the case of reading material of foreign or unknown vocabulary, *e.g.* an opera singer learning a libretto in a language he does not understand.

The translations described are detectable, of course only in reading aloud, not in free speech. While most of them occurred within a simple syntactic unit (the prepositional phrase), many crossed its boundary, either part or all of a prepositional phrase being translated to conform to the language of the word antecedent to the phrase. Even a full sentence is not a boundary strong enough to prevent translations across the period. When *S* read alternating sentences, the principal locus of translation was the first word of a sentence, which was translated to the language of the preceding sentence. Conversely, not all surrounded words in mixed text were translated. It remains to be determined whether there is a quantitative relation between the frequency of translation and the size of the syntactic boundary, and what the criterial attributes are that *S* uses to recognize a word as a member of a language.

The finding that comprehension is independent of the linguistic membership of lexical items, while reading aloud and free speech are not, is inconsistent with models of language-use that assume a mirror-symmetry of process for the various functions. It may, of course, be argued that the differences found here are due only to motoric facts. For example, the tongue lies lower in the mouth for French than English and facial muscles are tensed differently for the two. Mechanical problems in changing the mean position of the tongue and the facial muscles rapidly might account not only for the 'new accent' that *S* invents when reading aloud, but also account for the differences in time required to produce mixed and unilingual speech. This interpretation is inadequate for two reasons. First, qualitative differences in the kinds of error made mark reading aloud and talking freely; secondly, the nature of the bond between the constituent elements of prepositional phrases appears to be different in the two cases. Thus, even for the two tasks of reading aloud and talking, where both are dependent upon the same motor component, different kinds of organization of language are involved. These differences alone make it unlikely that mirror-symmetry accurately describes decoding and encoding routines. The processes involved in recognizing the informational content of a linguistic message may in fact be very different from the processes involved in generating messages.[16]

One interesting property of the mixtures which we have studied in an informal way is that *S* encodes the information contained in a linguistic sequence (*e.g.* in the case of alternating sentences), but, except for those sentences that contain markedly distinctive vocabulary-items that act as cues, does not remember the language in which the message arrived. Doob has made a similar observation among speakers of various African languages.[17] Another qualitative observation that bears on the essentially informational mode of language use is that, while preparing the linguistically mixed texts, we would lose the ability to identify to which language a given word belonged. The meaning of the words was always present, but whether they were French or English rapidly became a difficult decision to make. Thus, the ability to categorize words with respect to their language fades rapidly, syntactic anomalies are missed, but the meaning of the sentence resists these distortions.[18]

The nature of the psychological bond between elements in a sentence and of the 'thought units' they are presumed to represent has been studied introspectively, but the results presented here have left us unable to identify any linguistic unit larger than a word whose boundaries are not permeable.[19] To the degree that syntactic boundaries are linguistically specific, these results suggest that none can be identified as a dependable marker of a 'thought unit.' The fact that in naturalistic use of language by bilinguals syntactic units tend to be formed in one language may be taken as the result of over-learned motor sequences rather than as indices of cognition.

The actual process of generating mixed speech cannot, however, be identified from these experiments, although some conjectures are possible. One, developed from Chomsky, is that the 'deep structure' of the message is formulated and transformed according to rule, *S* substituting words in L_2 for some in L_1 at the level of surface-structure; in this case, deep structure is regarded as virtually synonomous with conceptualization.[20] Alternatively, *S* may make his 'decisions' about mixing prior to the formulation in deep structure, either by choosing individual words around which to build sentences, or perhaps by using some metalinguistic process of which deep structure is already a linguistic or proto-linguistic expression; in these cases, conceptualization is distinguished from deep structure. There also is the possibility that transitions are dictated by the availability of words rather than by 'decisions'—whether *S* switches his free speech may depend upon whether he has a word available to fill out an expression within a certain interval of time. The question seems to be amenable to experimental analysis with the kinds of mixed linguistic forms used here; an analysis of the location and duration of pauses in the production of mixed speech may help to solve it. The contributions of the transformational linguists make newly possible an experimental attack on these classical problems in psychology concerned with the 'language' of thought, and with the relation of thought to speech and other behavior.

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[16] Chomsky has pointed out to me that the difference in the bonds between the elements of prepositional phrases found in reading aloud and talking may indicate a greater dependence on 'surface structure' in the first case and 'deep structure' in the second.

[17] L. W. Doob, The effect of language on verbal expression and recall, *Amer. Anthropol.*, 59, 1957, 88-100.

[18] Cf. Jacques Mehler and G. A. Miller, Retroactive interference in the recall of simple sentences, *Brit. J. of Psychol.*, 55, 1964, 295-301, who found the semantic content of unilingual messages preserved in memory, despite distortions of their syntactic form.

[19] This does not mean that words should be thought of as impermeable. Our data contain only a single case—one *S* said *éstablishement* for *établissement*—but others come to mind in unilingual speech.

SUMMARY

Bilingual *Ss* were tested in several linguistic tasks with different kinds of material. Passages were prepared in unilingual, alternating, and mixed-language forms. The *Ss* were tested for comprehension, for ability to read aloud, to make precis, and to speak freely in these forms. Comprehension was found to be unaffected by the linguistic form of a message, but the other tasks showed decrements of the order of 20-40% when mixed text was articulated. The equivalence for comprehension of the form of the text, the occurrence of unique kinds of error in reading, and the problems of memory-search in generation were taken as evidence that the encoding and decoding of language are not symmetrical operations. The kinds of error made in reading aloud demonstrate, in turn, that reading cannot be described accurately only in terms of grapheme-phoneme translations.

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[20] Chomsky, *op. cit.*, 1-251.

FACTORS CONTRIBUTING TO THE DELAY IN THE PERCEPTION OF THE OCULOGRAVIC ILLUSION

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U. S. Naval Aerospace Medical Institute

If an observer (*O*) is seated in a fixed position on a human centrifuge at some distance from the center of rotation, he becomes aware of a change in spatial orientation as the velocity of the rotating platform changes. Thus, if he is accelerated from zero-velocity in darkness, he will report that he is being tilted away from the center of rotation, his apparent position assuming roughly the direction of the vector sum of the force of gravity and centripetal force. He will also report that dimly illuminated objects in his visual field will move and assume new positions in space (Fig. 1). But during this dynamic phase of these visual effects, which have been called the oculogravic illusion,[1] the change in the perceived horizontal does not keep pace with the changes in the force environment acting on the *S* unless the change is quite slow.[2] For example, in a typical experiment a change in resultant force may take no more than 5 or 6 sec. whereas the full visual reorientation may require as much as 1 or 2 min.

More recently, Clark and Graybiel have shown that similar lag will occur simply by manipulating the visual field with the gravitational environment held constant.[3] The *O*, rotating in darkness at a constant velocity and having set a luminous line approximately to the gravito-inertial horizontal, is suddenly exposed to the lighted cubicle. The strong vertical and horizontal cues in the visual field cause him to reset the luminous line to a position nearer the gravitational (and visual) horizontal than the gravito-inertial horizontal. If the room lights are subsequently turned out, the luminous line will appear to rotate *slowly* and resume its former position in space. This lag-effect also lasts for about 2 min. It was the

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[1] Ashton Graybiel, Oculogravic illusion, *Arch. Ophthal.*, 48, 1952, 605-615.

[2] Brant Clark and Ashton Graybiel, Visual perception of the horizontal following exposure to radial acceleration on a centrifuge, *J. comp. physiol. Psychol.*, 44, 1951, 525-534.

[3] Clark and Graybiel, Contributing factors in the perception of the oculogravic illusion, this JOURNAL, 76, 1963, 18-27.

purpose of the experiments reported here to investigate further the effects of certain selected conditions which might influence the course of the phenomena.

METHOD

(1) Apparatus. All of the observations were made in the Pensacola Slow Rotation Room which is, in simple terms, a carousel which rotates counterclockwise. *O* was seated in an erect position 5.5 ft. from the center of rotation and, in each experimental trial, was accelerated quickly to a velocity to produce a change in the direction of resultant force (*phi*) of either 20° or 30° (Fig. 1). He was seated in a lightproof cubicle within the room, and was strapped into position in a chair held rigidly to the floor, and a Fiberglas holder was used to maintain his head firmly in position. A collimator mounted directly in front of him contained a luminous line which could be rotated about its center by either *O* or *E*. *E* was seated in the room directly behind *O* where he could read the position of the luminous line to the nearest 0.5°.

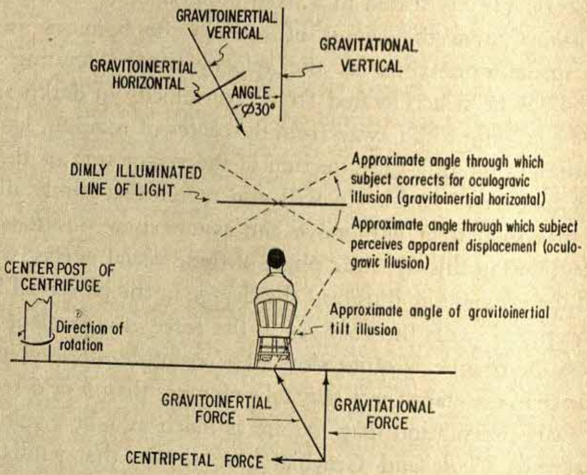
GRAVITOINERTIAL VERTICAL
GRAVITATIONAL VERTICAL
GRAVITOINERTIAL HORIZONTAL
ANGLE ∅30°
DIMLY ILLUMINATED LINE OF LIGHT
Approximate angle through which subject corrects for oculogravic illusion (gravitoinertial horizontal)
Approximate angle through which subject perceives apparent displacement (oculogravic illusion)
CENTER POST OF CENTRIFUGE
Direction of rotation
Approximate angle of gravitoinertial tilt illusion
GRAVITOINERTIAL FORCE
GRAVITATIONAL FORCE
CENTRIPETAL FORCE

FIG. 1. SCHEMATIC REPRESENTATION OF THE FACTORS IN OCULOGRAVIC ILLUSION. Diagrammatic representation of *O* and 'illusions' he experiences when exposed to a change in gravitational force relative to himself in the frontal plane (Angle $\emptyset = 30°$)

(2) Observers. Five experienced men were studied, all of whom had normal thresholds to caloric stimulation, normal responses to rotation, and, under proper experimental conditions, had perceived the oculogyral illusion.[4] Eight deaf men with bilateral labyrinthine defects (*L-D*) also served as *Os* in Experiment I.

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[4] M. E. McLeod and J. C. Meek, A threshold caloric test: Results in normal subjects, NSAM-834, NASA Order R-47, Pensacola, Fla., U.S. NavSchAviatMed, 1962, 1-9.

Detailed clinical findings on this group are described elsewhere.[5] All were deaf and had abnormal labyrinthine responses.

(3) Procedure. The same general procedure was used in the four experiments to be reported. Each series of observations began with the room stationary. The line was offset clockwise, and *O* set it to horizontal and maintained it in a horizontal position for 50 sec. *E* made a reading of the position of the line every 10 sec. Following different pre ëxposure conditions, the room was accelerated quickly to the desired velocity. *O*'s task was merely to maintain the luminous line in a horizontal position throughout the trial. *E* again made regular observations of the position of the luminous line every 10 sec. throughout the trial. Rest-periods of 2 min. were taken between successive trials. This procedure produced a record of *O*'s perception of the horizontal as a function of time following a change in magnitude and direction of resultant force acting on him. The measure of the oculogravic illusion at each point in time was the deviation of the absolute setting from the gravitational horizontal corrected for any constant error shown in the preliminary, static settings.

(4) Experiment I. The first experiment was planned to determine whether the nature of the pre ëxposure visual field would influence the lag-effect. It was predicted that pre ëxposure to a lighted room would produce a greater lag-effect in the perception of the oculogravic illusion than pre ëxposure to darkness. The normal and the labyrinthine defective (*L–D*) *O*s made continuous settings of the line to horizontal for 2 min. following a 2-min. exposure to a lighted room and then after a 2-min. pre ëxposure to darkness.

(5) Experiment II. The second experiment was identical with Experiment I except that only the normal *O*s were used and that instead of permitting *O* to set the line freely throughout the trial, the line was offset clockwise every 15 sec., and the *O* reset it.

Results: Since the results of the first two experiments were similar, they are considered together. An examination of Figs. 2–4 shows the effect of lag both in normals and *L–D Os*; *i.e.* the deviation of the settings from the gravitational vertical increased with time. In Fig. 2 which shows the mean data for normals with continuous settings, there is a clear tendency for the oculogravic illusion to be greater following preëxposure to darkness after about 30 sec. of rotation. When the offset method was used (Fig. 4), the curves overlap at both velocities. Similar lag-effects are shown for the *L–D O*s, but the preëxposure to the lighted cubicle shows a greater effect than the preëxposure to darkness at the slower velocity (Fig. 3).

To determine the significance of the differences in the light and dark conditions, the mean performance for each *O* for the final 30 sec. of the trials was determined. It was found that only one of the six comparisons

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[5] Ashton Graybiel and W. H. Johnson, A comparison of the symptomatology experienced by healthy persons and subjects with loss of labyrinthine function when exposed to unusual patterns of centripetal force in a counter-rotating room, *Ann. otol., etc., St. Louis*, 72, 1963, 357-373.

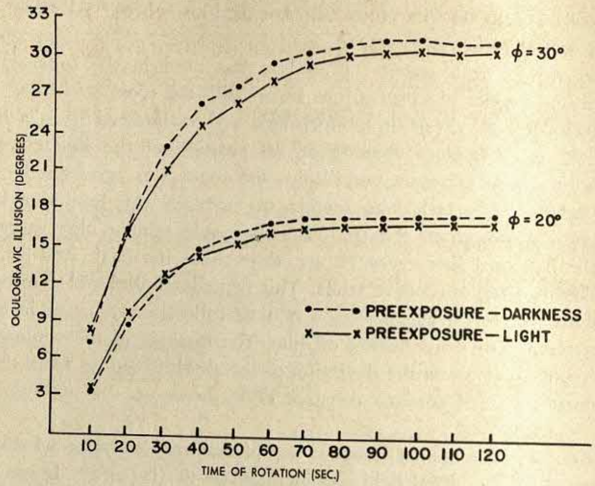

FIG. 2. MEAN VALUES FOR THE OCULOGRAVIC ILLUSION OF FIVE NORMAL *O*s
Experiment I; continuous settings.

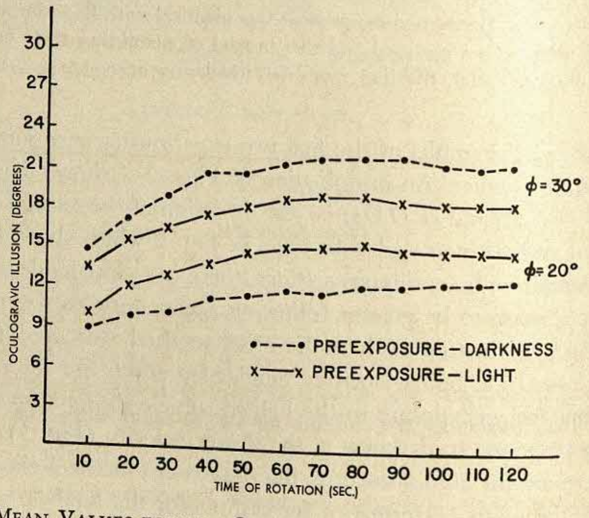

FIG. 3. MEAN VALUES FOR THE OCULOGRAVIC ILLUSION OF EIGHT *O*s WITH LABYRINTHINE DEFECTS
Experiment I; continuous settings.

was significant at the 0.01 level while for all the others the *p*-values were greater than 0.10. Thus, the only significant difference was in the predicted direction, but all of the differences were quite small. For example, in the case of the lower velocity for Experiment I for the normal *O*s, it can be said at the 0.05 level of confidence that the difference is between 0.5° and 3.2°.

Experiment III. Some incidental observations following Experiments I and II suggested that if there was a delay in presenting the luminous line *following* acceleration to a constant velocity, much of the illusory effect would occur in the absence of the luminous line. This experiment was planned to compare the effects of selected delays in presenting the luminous line following the beginning of rotation with the lag-effect when the line was visible continuously throughout the trial.

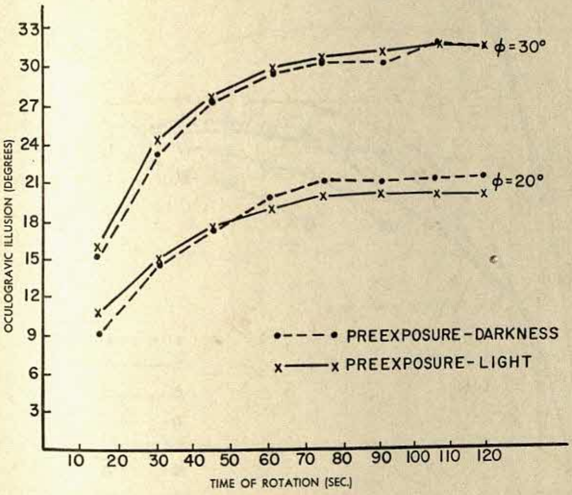

FIG. 4. MEAN VALUES FOR THE OCULOGRAVIC ILLUSION OF FIVE NORMAL *O*s
Experiment II; offset method.

The procedure was similar to that followed in Experiment II in which only the normal *O*s were tested. Immediately after the static observations were completed, *O* was exposed to 2 min. of darkness, and then the room was quickly accelerated to produce a change in the direction of resultant force of 20° or 30°. In these trials, however, seven conditions of delay in exposure of the luminous line were used. In the first condition, the luminous line was turned on as soon as the room began to rotate as in the previous experiments. For the second condition, the room rotated with the *O* in darkness for 10 sec., and then the luminous line was turned on and readings began 10 sec. later at 20 sec The seven delay conditions were: 0, 10, 20, 30, 50, 70, and 120 sec. The room rotated for 3 min. during each trial, offsets and settings being made every 10 sec. The order of presentation was randomized, and five trials were taken for each delay-period at each of the two velocities of rotation.

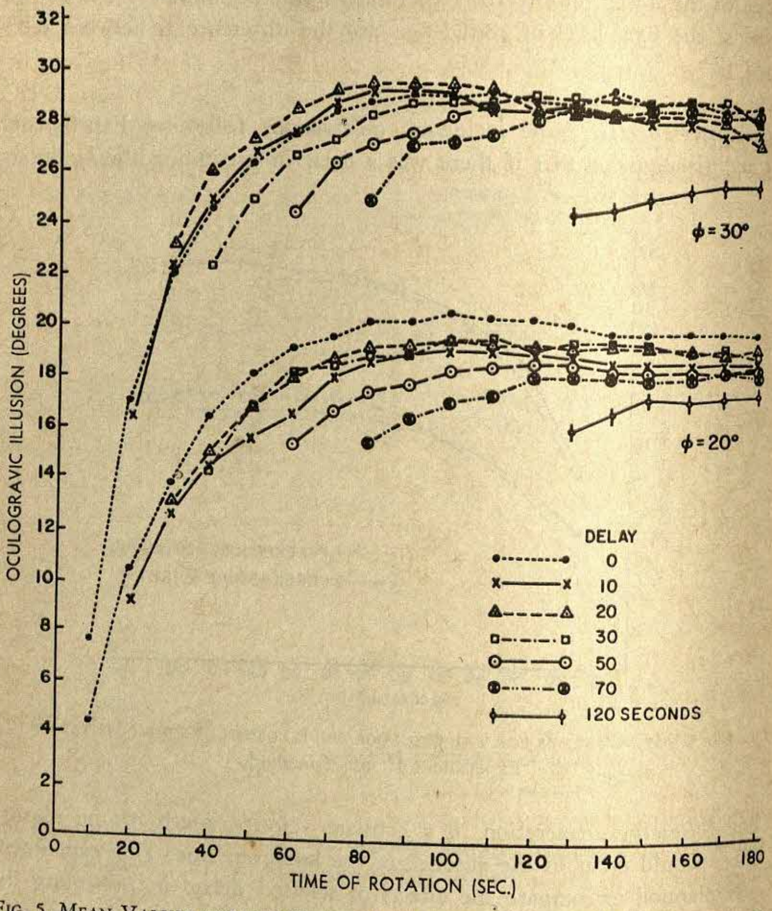

FIG. 5. MEAN VALUES FOR THE OCULOGRAVIC ILLUSION OF FIVE NORMAL *O*s WITH PROGRESSIVELY LONGER DELAY-TIME IN PRESENTING THE TARGET

Experiment III; delay series, continuous settings.

Results: Experiment III. The changing pattern in the perception of the visual horizontal as a function of the delay in presenting the target is shown by the two sets of curves in Fig. 5. It is apparent that with increasing delay, an increasing amount of the illusory effect had already occurred by the time of the first setting. When the time of delay was short, the subsequent pattern was similar to the settings of the zero-delay curve. With longer delays, the nonvisual antecedent effects reached the end of their influence before the potential magnitude of the illusion was reached. Nevertheless, with delay-times beyond 70 sec., the illusion may still be quantitatively increased somewhat even after the target has been presented. At *phi* = 20° this declining influence of the presentation of the visual target was present for delay-times up to the maximum of 2 min. On the other hand, for *phi* = 30° delays longer than 70 sec. appeared to have little additional influence on the setting of the line. This suggests that the magnitude of the effect producing the phenomenon are important in influencing thé pattern of this lag-effect.

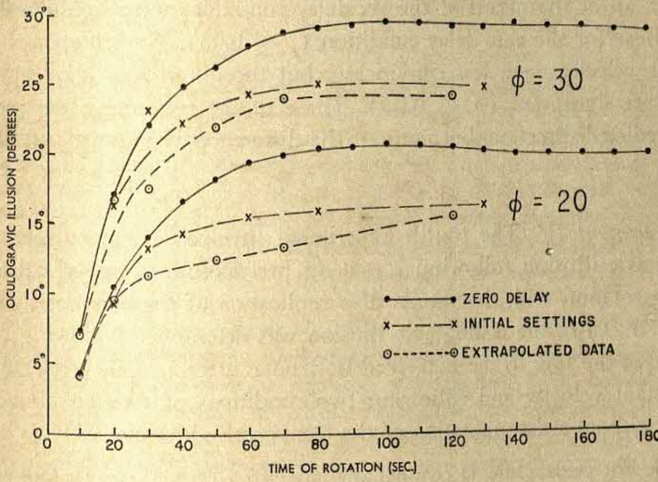

FIG. 6. THE NONVISUAL CONSTITUENT OF THE LAG-EFFECT COMPARED WITH THE ZERO-DELAY CONDITION

An attempt to give a quantitative approximation of the nonvisual constituent of the oculogravic illusion is found in Fig. 6. The upper curves of the two sets of curves are identical with the two zero-delay curves, one at *phi* = 20° and the other at *phi* = 30° (Fig. 5). The two middle curves are plots of the *initial* settings of each of the six longer delay conditions. These curves give an approximation of the nonvisual constituent of the phenomenon, but they are overestimations of the amount of the nonvisual constituent because at the time the reading was made, the light had been on for 10 sec. These points, therefore, include the nonvisual influence plus 10 sec. of visual influence. A second approximation was then made by extrapolating the six curves backward in time for 10 sec., the time when the light was turned on. These extrapolations are shown in the two lower curves which give an estimate of the nonvisual constituent alone. It is apparent that the two lower curves at each level of the *phi* are very similar to the zero-delay curves for the first 30 sec. This would appear to indicate the strong influence of the nonvisual constituent during this period. Thereafter, the zero-delay curve becomes and remains well above both estimates of the nonvisual effect. For a change in direction of resultant force of 20°, all of the means of the six delay conditions were significantly less than those for the zero-delay condition ($p \leq 0.05$). The differences at the greater velocity were less convincing, but the differences at 50 and 120 sec. were significant ($p \leq 0.05$). Three of the remaining four were in the predicted direction, but none of the differences was statistically significant.

Experiment IV. The fourth experiment involved a comparison of the oculogravic illusion following 2 min. of preëxposure to two visual targets during rotation with the results of a replication of the zero-delay Experiment III. Thus, the oculogravic illusion was determined following 2 min. of setting the line to the perceived horizontal after a rapid acceleration to a constant velocity and following two conditions of fixation of a visual target without a determination of the *O*'s perceived horizontal.

In the first preëxposure, *O* continuously observed a 1/16-in. circular spot of light placed 17¼ in. in front of him. Following 2 min. of constant rotation, the spot was turned off, the line of light was turned on, and settings of the line to the perceived horizontal were made as in the zero-delay condition. During the second condition of fixation, *O* viewed the luminous line set to the mean of the preliminary settings made before rotation began. Therefore, *O* observed the oculogravic illusion during the 2-min. delay period since the line appeared to be rotated clockwise from the horizontal. At the end of 2 min. of continuous observation of the line, *O* began settings to the horizontal as before. The order of the presentation of the three conditions was randomized, and the final setting was made after 3 min. of rotation. It should be noted that the first reading of the oculogravic illusion for the two delay conditions was made at 130 sec. following 2 min. of continuous fixation of the target. Again, five trials were taken for each *O*, for each fixation, and for the two velocities of rotation.

Results: Experiment IV. The mean oculogravic illusion at 130 sec. following 2 min. of setting of the line to horizontal for a *phi* of 20° was 17.6° and for a *phi* of 30° it was 30.8° (cf. Fig. 5). Following the fixation of the spot, the corresponding means were 15.0° and 23.3°, and following the fixation of the line the means were 15.3° and 26.9°. The following five settings showed a small increase as in Experiment III. The differences in the results at 130 sec. were, however, not statistically significant following the fixation of the spot ($p > 0.10$ at both velocities). Following the fixation of the line, the difference was not significant for *phi* equals 20° ($p > 0.05$), but at a *phi* of 30°, it was barely significant ($p < 0.05$). These data when compared with the results of Experiment III suggest that mere visual fixation during the delay-period has a very small influence, if any, on the lag effect of the oculogravic illusion as observed in this experiment.

DISCUSSION

The results of these experiments give additional evidence of a visual and a nonvisual constituent involved in the lag in the perception of the oculogravic illusion. They also shed some light on the factors involved and show that in this situation the nonvisual constituent predominates. Graybiel and Brown speculated that the lag-effect might have its genesis either in the end-organ or in the time required for the integration of incoming impulses which contribute toward spatial orientation.[6] Regarding the first possibility, deVries has shown that the time necessary for the stones of the utricle to assume their new position following a change in direction of resultant force is a small fraction of a second.[7] Another possibility at the end-organ level would be a prolonged change in the frequency of neural discharge following stimulation. Löwenstein and Roberts have reported such a change in the frequency of discharge of single twigs from the otolith organs of the Thornback ray which terminated within 30 sec. after the cessation of movement of their preparations. They also reported that when the preparation is held stationary following a tilt that "the increased or decreased discharge frequently reverts somewhat toward the initial level remaining, however, significantly above or below it for a matter of minutes." They also point out, "It is clear that the discharge frequency returns in every case to a basic level and that it therefore is unsuitable to furnish a position signal."[8] That is, the change in frequency of discharge of individual fibers *per se* would not appear to be an adequate cue to produce the lag-effect. Consequently, it becomes necessary to look elsewhere for factors contributing to the lag-effect, and the time necessary to integrate information from various mechanisms appears to be a likely source. This notion would also be supported by the lag-effect at a constant velocity of rotation when passing from a full visual framework to darkness.[9]

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[6] Ashton Graybiel and R. H. Brown, The delay in visual reorientation following exposure to a change in resultant force on human centrifuge, *J. gen. Physiol.*, 45, 1951, 143-150.

[7] Hl. de Vries, The mechanics of the labyrinth otoliths, *Acta Otolaryng., Stockh.*, 38, 1950, 262-273.

[8] Otto Löwenstein and T. D. M. Roberts, The equilibrium function of the Thornback ray (Raja clavata), *J. Physiol.*, 110, 1950, 392-415, esp. 410.

[9] Clark and Graybiel, *op. cit.*, 1963, 27.

It has been known for many years that both visual and postural cues interact in the perception of the visual vertical and horizontal, not only in the case of the oculogravic illusion but also in such effects as the A- and the E-phenomenon.[10] Nevertheless, Experiments I and II show that exposure to a visual framework *before* rotation begins does *not* have substantial influence on the course of the lag-effect. On the other hand, it has been shown that when the force environment is held constant and the oculogravic illusion is reduced by exposure to a complex visual framework, a lag-effect is again manifested when the visual framework is removed, and *O* observes the luminous line in darkness. That is, the apparent visual horizontal will shift *slowly* from the visual frame of reference to the gravitational frame of reference.[11] It is noteworthy that these two effects have the same general pattern of change over time. This would appear to explain why the pre-exposure to the visual framework did not substantially influence the lag-effect.

A comparison of Figs. 2 and 3 shows that the *L–D Os* have a smaller oculogravic illusion and less lag than the normal *Os*. This gives further support to the well-established relationship between the oculogravic illusion and the vestibular mechanism.[12] Furthermore the *L–D Os* exhibited a much greater variability of response. An examination of the individual curves (not shown) of the *L–D Os* showed much variability throughout the 2-min. trials for some *Os* while others showed a lag similar to that of the normal *Os*. These results correspond to the variability of the same group of *L–D Os* on a parallel swing.[13]

The data from Experiment III support the principle of interaction of visual and gravitational factors in producing the oculogravic illusion. It is also clear that in the absence of vision, certain nonvisual processes produce the major part of the effects which result in a modification of the perceived visual horizontal producing the oculogravic illusion. These nonvisual processes show their influence immediately upon the presentation of the luminous line (Fig. 5). It is equally evident that the nonvisual constituent increases for 60 to 80 sec. and then remains relatively constant at least up to 3 min. Following the presentation of the line, the illusory effect increases somewhat for 60 sec. or more until it approximates the level of the zero-delay with the exception of the 2-min. delay in which case it remains 2-3° less than that during zero delay for the final minute of the observations. Whether the curves for the prolonged periods of delay would continue to rise after 3 min. is not clear from these data, but it is reasonable to expect that they would not in the light of the fact that the major portion of the lag-effects is completed in 60 sec. The data suggest that the visual constituent of the lag would also be found for more prolonged periods of delay because these two curves also level off after about 1 min. (Fig. 5).

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[10] J. J. Gibson, The relation between visual and postural determinants of the phenomenal vertical, *Psychol. Rev.*, 59, 1952, 370-375; Graybiel, *op. cit.*, 11; H. A. Witkin, Perception of the upright when the direction of the force acting on the body is changed, *J. exp. Psychol.*, 40, 1950, 93-106.

[11] Clark and Graybiel, *op. cit.*, 1963, 26-27.

[12] Graybiel and Clark, The validity of the oculogravic illusion as a specific indicator of otolith function, *Aerospace Med.*, 36, 1965, 1173-1181.

[13] F. E. Guedry, Jr. and C. W. Harris, Labyrinthine function related to experiments on the parallel swing, NSAM-874, NASA Order R-93, Pensacola, Fla., U. S. NavSchAviatMed, 1963.

The basic neurophysiological mechanisms underlying the lag-effect are not known, but many studies have shown visual-vestibular coördination, and it is well known that certain neurons in the visual cortex respond to vestibular stimulation.[14] Two experimental findings appear clear from this and other studies. First, if a normal *O* observes a luminous line in darkness following an abrupt change in the direction and magnitude of resultant force acting on him, there is a *gradual* reorientation of the visual horizontal (lag-effect), the end result of which is roughly in accord with the gravito-inertial horizontal. Secondly, if the judgments of the horizontal are delayed, the lag-effect is even greater. These experimental results may be understood in terms of what Helson has called pooling, or in this case "cross modality interactions," in processing information from various sense organs.[15] Helson argues, within the framework of his theory of adaptation-level, that stimuli are not equally weighted in perception, and this would appear to apply directly to the oculogravic illusion in general and to the lag-effect in particular. He states that, although some investigators believe that the pooling of cues can involve only present and immediately preceding stimuli, the experimental evidence indicates that these interactions may operate over extended periods.

Helson's notion of differential weighting of cues over time would appear to apply to the lag-effect. It would assume that there would be a decrease in the weighting of visual factors and a corresponding increase in the weighting of gravitational factors over time, producing a shift in the perceptual norm. In the static situation, the retinal lines of the horizontal are in accord with the gravitational cues, and there is no conflict in information and hence no problem of differential weighting of cues. On the other hand, when there is a sudden change in the direction of the resultant force, the gravitational cues become disparate with respect to the retinal horizontal. At the outset, the visual cues may be said to have the heaviest weighting in the perception of the horizontal. This is supported by the fact that there is no measurable lag-effect when a luminous line is viewed in darkness during constant rotation and a visual framework is then presented.[16] The shift to the visual framework is immediate. Studies by Witkin (*e.g.* Witkin and Asch)[17] also support the notion of the heavy weighting of visual factors, and recently Hammer has found that there are relatively small constant errors in setting a luminous line to vertical in darkness under zero-gravity conditions.[18] *Ss* set a luminous line very close to vertical as represented by the position of the retinal receptors, but Hammer did find significantly greater average errors. In the case of the lag-effect, this heavy weighting of visual cues decreases with time, and the apparent visual horizontal changes in a series of steps over time as the weighting is shifted to the gravitational cues until the visual horizontal is perceived to be very close to the gravitational horizontal.[19] It would appear, therefore, that these results are an example of the pooling of information from different sensory processes and of changes in weighting over time as suggested by Helson.[20]

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[14] Richard Jung, Neuronal integration in the visual cortex and its significance for visual information, in W. A. Rosenblith (ed.), *Sensory Communication,* 1961.

[15] Harry Helson, *Adaptation-Level Theory,* 1964, 175-182.

[16] Clark and Graybiel, *op. cit.,* 1963, 21-22.

[17] H. A. Witkin and S. E. Asch, Studies in space orientation: IV. Further experiments on perception of the upright with displaced visual fields, *J. exp. Psychol.,* 38, 1948, 762-782.

[18] Lois Hammer, Perception of the visual vertical under reduced gravity, MRL-TDR-62-55, Wright-Patterson AFB, Ohio, 1962.

[19] Clark and Graybiel, *op. cit.,* 1963, 22.

SUMMARY

The purpose of this study was to observe the effects of factors which contribute to the delay in the change in the perception of the horizontal following a change in the direction of resultant force acting on an *O* (the oculogravic illusion). Five normal and eight labyrinthine defective men were studied in a Slow Rotation Room. Four separate experiments were conducted with changes in direction of resultant force of 20° or 30° acting on the *O*s. The results showed very small effects of preëxposure-conditions prior to the change in direction of resultant force. On the other hand, delays in the presentation of a luminous target following a change in the resultant force and before settings to the visual horizontal occurred produced major, systematic effects on the perception of the visual horizontal. These results are discussed in terms of the interaction of visual and gravitational cues in producing the lag-effect.

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[20] Helson, *op. cit.*, 89-94.

FUNCTIONS OF HINTS IN INDIVIDUAL PROBLEM-SOLVING

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A number of investigators have studied problem-solving by giving hints of various kinds to *Ss.*[1] A common question raised is, "What makes a hint good?" There is evidence indicating that hints which "explicate the goal" are more helpful than hints which do not; that "concrete hints" are more helpful than "abstract hints"; and that hints having no value, but which precede the appearance of a solution, are given credit for causing the solution, while hints known to be effective escape consciousness.[2] More needs to be known, however, about how hints help *Ss* in solving a problem. Why are some hints helpful, while others are not? Why is the same hint helpful to one *S* and not to another?

Maier has shown that *Ss* frequently try to incorporate hints into the approaches they have adopted in solving the problem, so that the hint is ineffective in changing their approaches.[3] Occasionally, where no part of the hint could be incorporated into the ongoing approach, the *Ss* seemed to reject it as irrelevant. Maier's hints were often tangential and ambiguous and were offered in a way which permitted *S* to ignore them if he failed to see their relevance to his problem.

In the present study we were interested in how the *Ss* used the information provided in hints. The hints were presented, therefore, in as clear and unambiguous form as possible and in an authoritative manner by *E*. Thus, all the *Ss* were obliged to attend to the hints and to try and use them in solving the problem. As a result it was possible to determine exactly what impact each hint had both on the success of *S* and on the approach he used in trying to achieve a solution.

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[1] Karl Duncker, On problem solving, *Psychol. Monogr.*, 58, 1945, (No. 270) 1-112; George Katona, *Organizing and Memorizing*, 1940, 81-107; A. S. Luchins, Mechanization in problem solving—the effect of Einstellung, *Psychol. Monogr.*, 54, 1942, (No. 248) 1-95; N. R. F. Maier, Reasoning in humans: I. On direction, *J. comp. Psychol.*, 10, 1930, 115-143; II. The solution of a problem and its appearance in consciousness, *ibid.*, 12, 1931, 181-194; Max Wertheimer, *Productive Thinking*, 1945, 14-78.

[2] These conclusions can be found in reports by J. W. Reid, An experimental study of "analysis of the goal" in problem-solving, *J. gen. Psychol.*, 44, 1951, 51-69; Benjamin Burack and Donald Moos, Effect of knowing the principle basic to solution of a problem, J. educ. Res., 50, 1956, 203-208; Maier, *op.cit.*, 1931, 181-194.

[3] Maier, *op. cit.*, 1930, 115-143.

In addition, the importance of timing of hints was explored by comparing the relative effectiveness of hints when given at the outset (before S attempted a solution) and when given after 30 min. of problem-solving (after one or more approaches to the problem had failed).

Method: (1) Subjects. The Ss were 234 men obtained from the introductory course in psychology.

(2) Problem. Maier's Hatrack Problem is a construction problem in which S is required to build a structure sufficiently stable to support a man's overcoat using only two sticks (one, 1″ × 1″ × 60″, the other, 1″ × 1″ × 43″) and a 2″ C-clamp.[4] The opening of the clamp is wide enough so that both sticks can be inserted and held together securely when the clamp is tightened.

Every S was asked to build the hatrack at a point in the center of the room. The only stable solution to this problem consists in clamping the two sticks together so that they can be wedged between the floor and the ceiling. The clamp handle functions as the hook on which to hang the coat. Since a basement room was used and the ceiling was made of concrete, there was no reason to reject the solution because it might damage the room.

Previous research with the Hatrack Problem has indicated two common incorrect assumptions the Ss tend to make in trying to solve the problem: (1) a hatrack is a vertical structure which rests on the floor; and (2) the coat must be hung at the end of one of the sticks.[5] Both of these assumptions are in conflict with finding the correct solution, which requires the use of the ceiling as a support for the structure and the clamp as both the coat hanger and a fastener for the two sticks.

(3) Hints. Hints designed to overcome each of these incorrect assumptions were used. One, named the Ceiling-hint, was designed to call attention to the ceiling as a possible means of support. It was stated as follows: "In the correct solution the ceiling is part of the construction." The other, named the Clamp-hint, was designed to call attention to the clamp's use as a way of holding the coat. It was stated as follows: "In the correct solution the clamp is used as the coat hanger." Some of the Ss were given both hints, stated as follows: "In the correct solution the ceiling is part of the construction and the clamp is used as the coat hanger." The hints used in this experiment supply elements which are part of the final solution and are incongruous with one or both of the two approaches most commonly adopted by the Ss.

(4) Setting. The experimental room was 26′7″ × 14′7″. The ceiling was 8′ high, but two beams jutted down from the ceiling about 2′, dividing the room into thirds. In addition, several electrical conduits crisscrossed the ceiling at several points; three large fluorescent lights and their accompanying electrical piping were attached to the ceiling; and both the ceiling and floor had minor irregularities. All of these characteristics of the room served as unique opportunities upon which various Ss tried to capitalize.

(5) Experimental conditions and procedure. The data to be reported here were gathered in two separate investigations. In the first study 102 Ss were given one of

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[4] Maier, Reasoning in humans: III. The mechanisms of equivalent stimuli and of reasoning, *J. exp. Psychol.*, 35, 1945, 349-360.

[5] L. R. Hoffman, R. J. Burke, and N. R. F. Maier, Does training with differential reinforcement on similar problems help in solving a new problem?, *Psychol. Rep.*, 13, 1963, 147-154.

the two hints if they failed to solve the problem after 30 min. Of these, 36 solved the problem before the hints were given and were eliminated from the experiment. Of the remaining 66 *S*s, 35 received the Ceiling-hint and 31 received the Clamp-hint. The assignment of the hints was determined randomly for each *S* before the experimental session and, therefore, was unrelated to *S*'s performance. Eight minutes were permitted for the utilization of the hint received.

In the second study the hints were given at the outset. The 132 *S*s were assigned randomly to one of the following conditions: (a) Ceiling-hint (N = 30), (b) Clamp-hint (N = 30), (c) Both hints (N = 30), (d) Control (N = 42), in which no hints were given. The hints used were supplied immediately after the problem was introduced, but before the *S*s had an opportunity to think about the problem. In all cases the session was terminated either when *S* produced the correct solution or at the end of 20 min. of effort.

All the *S*s were tested individually. Each *S* was told that *E* was interested in what people do in new situations. After *S* seemed to be at ease, the problem was introduced. *E* recorded the solutions attempted by *S* in 30-sec. intervals on an observational sheet. When it was not obvious what *S* was trying to do, *E* would ask him what he was thinking. A heavy overcoat was available for *S*'s use to test the effectiveness of his construction.

Results: (1) Effect of hints given during problem-solving activity. In the 8 min. following the hints (given after 30 min. of failure to solve the problem), correct solutions were produced by 45.7% of the *S*s who received the Ceiling-hint as compared to 16.1% of the *S*s who received the Clamp-hint, a percentage-difference significant at the 0.01 level of confidence. Thus, one might conclude that the Ceiling-hint is more effective than the Clamp-hint.

In an attempt to understand how the Ceiling- and Clamp-hints affected problem-solving behavior, the records of the solutions attempted were classified into five basic types.[6]

(a) Base: one stick is used as a support to hold the second one vertically, with the clamp joining them.

(b) Balance: the sticks are leaned against each other in an "X" or "T" shape with the clamp joining them.

(c) Support: any solution using a part of the room (walls, pipes, ceiling beams) to hold up the construction with one of the sticks remaining on the floor.

(d) Ceiling-Suspension: both sticks off the floor, being wedged between ceiling pipes and ceiling or ceiling beam, with the clamp joining them or appended to the joined ends.

(e) Floor-Ceiling: the correct solution in which the sticks are wedged between floor and ceiling and joined tightly by the clamp. The clamp serves as a hook for the coat in this construction.

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[6] This classification is a modification of one used by A. J. Judson, C. W. Cofer, and Sidney Gelfand, Reasoning as an associative process: II. "Direction" in problem solving as a function of prior reinforcement of relevant responses, *Psychol. Rep.*, 2, 1956, 501-507.

The effect of the hints on *Ss'* problem-solving process can be seen from a comparison of the types of solutions attempted immediately before and after each of the hints was given. The first and second columns of Table I show the proportion of *Ss* who were trying each type of solution just prior to the hint. This comparison shows essentially no differences in the types of solutions being worked on by the *Ss* in the two groups at that point. Columns 1 and 3 of Table I show that after the Ceiling-hint, the *Ss* attempted significantly *fewer* Base and Balance solutions and significantly *more* Support and Floor-Ceiling solutions. While the Ceiling-hint succeeded in making the *Ss* abandon Base and Balance approaches, a vast majority of these *Ss* shifted initially to an alternative incorrect solution (Support or Ceiling-Suspension). Only 22.8% of these *Ss* were prompted to find the correct solution on their first attempt.

TABLE I

TYPES OF SOLUTIONS ATTEMPTED JUST BEFORE AND JUST AFTER CEILING- AND CLAMP-HINTS

	Last solution attempted before hint				First solution attempted after hint			
	Ceiling-hint (N=35)		Clamp-hint (N=31)		Ceiling-hint (N=35)		Clamp-hint (N=31)	
Type of Solution	N	%	N	%	N	%	N	%
(*a*) Base	10	28.6	10	32.2	0	0.0§	3	9.7§
(*b*) Balance	7	20.0	9	29.0	0	0.0§*	13	41.9
(*c*) Support	4	11.4	1	3.2	11	31.4*‡	1	3.2
(*d*) Ceiling-Suspension	14	40.0	11	35.6	16	45.7	13	41.9
(*e*) Floor Ceiling	0	0.0	0	0.0	8	22.8†‡	1	3.2

* Percentage difference between Ceiling- and Clamp-Hint: significant at 0.01.
† Percentage difference between Ceiling- and Clamp-Hint: significant at 0.05.
‡ Percentage is significantly higher (0.05) after than before hint.
§ Percentage is significantly lower (0.05) before than after hint.

The major change in the types of solutions attempted by the *Ss* who received the Clamp-hint (Columns 2 and 4) was the reduction in the proportion who continued to work on Base solutions (from 32.2% to 9.7%). Most solutions of the Base type involved using the clamp handle on the floor to provide a third point of support at the base of the construction. The instruction that the coat should be hung on the clamp handle made this approach obviously the wrong one. The *Ss* who abandoned the Base solution did not find the correct solution immediately, however, but were more likely to attempt one of the other incorrect solutions. Table I also shows the greater immediate effectiveness of the Ceiling-hint than the Clamp-hint in producing correct solutions (22.8% vs. 3.2%).

The ways in which the two hints affected *S*s' problem-solving behavior can be seen even more clearly by examining how each influenced the problem-solving activity in progress at the time the hint was received. The attempts at solution preceding the hint and those following it were classified according to whether the *S*s used the floor or the ceiling as the main focus of construction. The *S*s who received the Ceiling-hint and the Clamp-hint were then compared for changes in behavior following the hint. A comparison of Rows 1 and 2 in Table II shows that the *S*s who received the Clamp-hint more often retained the same approach than changed, regardless of the approach they had adopted prior to the hint. Except for the single success, most of the *S*s who had been trying to build up from the floor continued to do so (78.9%), and those *S*s working with the ceiling also generally continued along the same line (91.7%).

TABLE II

CHANGES IN APPROACH FOLLOWING CEILING- AND CLAMP-HINTS

Attempts Before Hint	Type of Hint	Type of first solution after hint						
		Floor		Ceiling		Correct solution		Total *N*
		N	%	N	%	N	%	
Floor	Clamp	15	78.9*	3	15.8†	1	5.3‡	19
Ceiling	Clamp	1	8.3*	11	91.7†	0	0.0§	12
Floor	Ceiling	0	0.0	12	70.6	5	29.4‡	17
Ceiling	Ceiling	0	0.0	15	83.5	3	16.7§	18

Note: Percentages having the same superscript are significantly different at the following levels:
* 0.01; † 0.01; ‡ 0.06; § 0.07.

The *S*s who received the Ceiling hint, on the other hand, present a different picture. All those who had been working on a floor approach prior to the hint, and who still failed to solve the problem, shifted to some use of the ceiling following the hint (70.6%). The *S*s, however, who had already adopted a ceiling approach retained that approach (83.3%).

Table II also shows that the Ceiling-hint was more effective than the Clamp-hint in producing correct solutions whether the *S*s were attempting floor (29.4% vs. 5.3%) or ceiling approaches (16.7% vs. 0.0%). In addition there was a slight tendency for the Ceiling-hint to be more effective for *S*s working off the floor than for *S*s working off the ceiling (29.4% vs. 16.7%).

(2) Effect of hints given before problem-solving activity. Let us next examine the data obtained from the *S*s who were given the hints at the out-

set, before they had an opportunity to establish mental sets or directions in their thinking. Table III reports the number and percentage of initial types of solution attempted in the three experimental conditions and in the control condition. The Ceiling- and Clamp-hints together appear to have had essentially the same effect on the distribution of first solutions as the Ceiling-hint alone (Columns 1 and 2). This effect was different, however, from that produced by the Clamp-hint alone. The Ceiling-hint reduced the number of Base- and Balance-solutions since these relied on the floor as the sole means of support for the construction. The Clamp-hint reduced the number of Base-solutions since the instruction that the coat should be hung on the clamp made this solution incompatible with the hint. Thus, the Ceiling-hint eliminated two incorrect solutions and the Clamp-hint eliminated one incorrect solution frequently attempted by the *S*s not receiving hints. In terms of success on the first attempt, the Ceiling-hint, whether given alone or together with the Clamp-hint, was more effective in increasing the proportion of correct solutions than the Clamp-hint (43.3% vs. 20.0%).

TABLE III

Types of Initial Solutions Attempted when Hints are Given at Outset

Type of solution	Both Hints (*N*=30)		Ceiling-Hint (*N*=30)		Clamp-Hint (*N*=30)		Control (*N*=42)	
	N	%	*N*	%	*N*	%	*N*	%
(*a*) Base	0	0.0†	1	3.3†	1	3.3†	11	26.2
(*b*) Balance	0	0.0†‡	0	0.0†‡	11	36.7	15	35.7
(*c*) Support	6	20.0	5	16.7	2	6.7	3	7.1
(*d*) Ceiling-Suspension	11	36.7	11	36.7	10	33.3	9	21.4
(*e*) Floor-Ceiling	13	43.3*§	13	43.3*§	6	20.0	4	9.5

* Percentage is significantly higher than comparable percentage in control condition at the 0.05 level of confidence.

† Percentage is significantly lower than comparable percentage in control condition at the 0.05 level of confidence.

‡ Percentage is significantly lower than comparable percentage in Clamp-hint condition at the 0.05 level of confidence.

§ Percentage is significantly higher than comparable percentage in Clamp-hint condition at the 0.05 level of confidence.

Table IV extends the time-span to show the number of *S*s who attempted solutions of each type during the first 20 min. It should be noted that the *S*s could enter more than one category in this tabulation. The striking finding here is the tendency for certain types of solution which were eliminated on the first attempt by hints given at the outset, to make a delayed appearance when the *S*s were unsuccessful in using the hint to solve the problem correctly. This tendency is most marked for *S*s who received the Clamp-hint. Only one of these *S*s (3.3%) attempted a Base-solution initially (Table III), whereas after 20 min. 8 *S*s (26.7%) had attempted it. Base- and Balance-solutions which were initially eliminated by the Ceiling-hint, also began to appear, although less frequently. Only once did an *S* attempt an incompatible solution after both hints had been given. This suggests that the *S*s who are unable to use a hint to achieve the correct solution may regress to incompatible types of solution.

A second point of interest was the similarity of all hints in the percentage of correct solutions produced. The initial superiority of the Ceiling-hint over the Clamp-hint tended to be reduced with the passage of time. The gradual development of the delayed positive effect of the Clamp-hint can be seen in Table V. In this table, the number and percentage of correct solutions is presented for the three experimental conditions and the control condition at six comparison-points. Only for the first solution attempt is the Ceiling-hint significantly better than the Clamp-hint. After 10 min. there is little difference among the three hint-conditions. At the end of 20-min. the three hint-conditions are very similar and significantly better than the control condition.

TABLE IV

SOLUTIONS ATTEMPTED DURING THE FIRST 20 MIN. WHEN HINTS ARE GIVEN AT THE ONSET

Type of Solution	Both Hints ($N=30$)		Ceiling-Hint ($N=30$)		Clamp-Hint ($N=30$)		Control ($N=42$)	
	N	%	*N*	%	*N*	%	*N*	%
(*a*) Base	1	3.3†‡	2	6.7†	8	26.7†	23	54.8
(*b*) Balance	0	0.0†‡	2	6.7†‡	14	46.7	25	59.5
(*c*) Ceiling-Suspension	12	40.0	12	40.0	12	40.0	14	33.3
(*d*) Support	10	33.3	8	26.7	8	26.7	14	33.3
(*e*) Floor-Ceiling	22	73.3*	23	76.7*	21	70.0*	20	47.6

* Percentage is significantly higher than comparable percentage in control condition at the 0.05 level of confidence.

† Percentage is significantly lower than comparable percentage in control condition at the 0.05 level of confidence.

‡ Percentage is significantly lower than comparable percentage in Clamp-hint condition at the 0.05 level of confidence.

The data from the two investigations allow a preliminary exploration into the effect of timing on the usefulness of hints. This comparison must be made with some caution because the populations in the two investigations are not strictly comparable, since the first investigation eliminated the

Ss who solved the problem without hints prior to 30 min. Table VI presents the data for the two hints when given at the outset and when given after 30 min. of problem-solving activity.

It appears that the hints given *before* and *after* problem-solving began had essentially the same immediate effect on the distribution of incorrect types of solution. In both cases, the Ceiling-hint eliminated Base- and Balance-solutions (*a* and *b*) but the Support-solution (*c*) seems to have been favored slightly more when hints were given later (31.4%) than when given at the outset (16.7%). The Clamp-hint had a very similar effect, regardless of whether it was given at the outset or after a period of problem solving.

TABLE V

STAGES AT WHICH CORRECT SOLUTIONS APPEARED FOR FOUR HINT-CONDITIONS

	Both Hints		Ceiling-Hint		Clamp-Hint		Control	
Time Ending	($N=30$)		($N=30$)		($N=30$)		($N=42$)	
	N	%	N	%	N	%	N	%
First Solution	13	43.3*†	13	43.3*†	6	20.0	4	9.5
5 Min.	18	60.0*	15	50.0*	12	40.0	11	26.2
8 Min.	20	66.7*	16	53.3	13	43.3	16	38.1
10 Min.	20	66.7*	17	56.7	18	60.0	18	42.9
15 Min.	22	73.3*	22	73.3*	19	63.3	19	45.2
20 Min.	22	73.3*	23	76.7*	21	70.0*	20	47.6

* Percentage is significantly higher than comparable percentage in control condition at the 0.05 level of confidence.

† Percentage is significantly higher than comparable percentage in Clamp Hint condition at the 0.05 level of confidence.

TABLE VI

COMPARISON OF SOLUTIONS IMMEDIATELY FOLLOWING CEILING- AND CLAMP-HINTS WHEN GIVEN BEFORE AND AFTER PROBLEM-SOLVING ACTIVITY

	Ceiling-Hint				Clamp-Hint			
	before		after		before		after	
Type of Solution	($N=30$)		($N=35$)		($N=30$)		($N=31$)	
	N	%	N	%	N	%	N	%
(*a*) Base	1	3.3	0	0.0	1	3.3	3	9.7
(*b*) Balance	0	0.0	0	0.0	11	36.7	13	41.9
(*c*) Support	5	16.7	11	31.4	2	6.7	1	3.2
(*d*) Ceiling-Suspension	11	36.7	16	45.7	10	33.3	13	41.9
(*e*) Floor-Ceiling	13	43.3	8	22.8	6	20.0	1	3.2

The fact that the correct solution immediately follows a hint more often when given at the outset than when given later raises an interesting question. In the case of the Ceiling-hint, the comparison between *before* and *after* is 43.3% and 22.8% respectively, and for the Clamp-hint it is 20.0% and 3.2% respectively. Some *S*s in the *before* condition, however, would have solved the problem without the help of hints. Since 9.5% of the control *S*s find the correct solution at the outset, we must subtract this figure from the *before* condition. The comparisons now become 33.8% and 22.8% for the Ceiling-hint and 10.5% and 3.2% for the Clamp-hint, a trend still favoring the *before* condition for both hints but no longer significant.

Discussion. The solutions attempted have been classified into five categories: (*a*) Base, (*b*) Balance, (*c*) Support, (*d*) Ceiling-Suspension, and (*e*) Floor-Ceiling. The first four categories represent approaches that fail because the material supplied is inadequate for making them successful, while the fifth is the correct and only workable solution. Classifying solutions is not difficult because it is clear from his behavior what *S* is attempting to accomplish. Thus, an *S* who constructs an inverted "T" by clamping the sticks in this manner, is attempting to make an old-fashioned three-legged hatrack.

Maier has used the term "direction" in thinking because the various manipulations within a type of solution reveal a patterned trial and error activity.[7] Krechevsky used the term "hypothesis behavior" to indicate a similar function.[8] A mediating term is needed because the varied behaviors (*e.g.* different ways of getting legs for the hatrack) are selectively controlled. The process of direction selects objects from the environment and memories from the past in building a solution. Maier distinguished between habitual and new directions because some approaches to solutions are derived from past learnings, while other solutions indicate the appearance of new aproaches. The first two solution-categories listed above most clearly illustrate habitual directions, whereas the correct solution requires a new and different conceptualization: how to use both the floor and the ceiling to hold the upright stick in place.

The term direction also has explanatory value because, like behavioral hypotheses, selective patterns of activities persist. Something must happen to cause a change in direction. Often failures are repeated because an alternative direction cannot emerge while one is dominant. For the purposes of our discussion we will use the term 'directional' to refer to the line of thinking behind each classification. Thus, hints may stop ongoing directions without suggesting new ones or they may suggest other directions.

A comparison of the Ceiling- and Clamp-hints has shown that the first of these hints will terminate two directions (those leading to Base- and Balance-solutions), while the second terminates only one direction (that leading to the Base-solution). This occurs regardless of whether these hints are given after 30 min. of problem-solving or at the outset (Tables I, III, and VI). This superior ability to block false directions should make the Ceiling-hint superior to the Clamp-hint.

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[7] Maier, *op. cit., J. comp. Psychol.,* 10, 1930, 115-143.

[8] Isadore Krechevsky, "Hypotheses" vs. chance in the presolution period in sensory discrimination, *Univer. Calif. Publ. Psychol.,* 6, 1932, 27-44.

Do the hints differ in suggesting the correct direction? The fact that new solutions immediately follow the Ceiling-hint more often than the Clamp-hint indicates the superiority of the Ceiling-hint in this regard also (Tables I and III).

Do the hints differ in suggesting false directions? In this regard the Ceiling-hint seems to be the greater distractor. Solutions classified as Ceiling-Suspension and Support are high after the Ceiling-hint, whereas only Ceiling-Suspension types of solutions are high after the Clamp-hint (Tables I and III).

The results shown in Table II lend further support to this differential effect. The Clamp-hint was less dramatic in its ability to influence direction, whereas the Ceiling-hint eliminated activity on the floor, but reinforced ongoing, but false, activity in the area of the ceiling. The fact that the Ceiling-hint encourages incorrect directions (as well as the correct one), while the Clamp-hint does not, permits some of the *S*s who received the latter, to discover the correct direction by themselves, while some of the *S*s receiving the Ceiling-hint became involved with false directions suggested by it. This explanation would seem to account, in part, for the greater delayed value of the Clamp-hint so that its value eventually approached that of the Ceiling-hint (Table V).

To account further for the realtively different delayed effects of the two hints we must consider the fact that failure eventually leads to the giving up of a direction despite its persistence. Once an ongoing direction loses its force, other directions can more readily occur. When the problem-solver is without a direction, a hint would have the best opportunity to positively initiate a new one. This point receives some support from the finding that hints given at the outset have greater value than those given after a period of problem-solving activity, during which time several directions have been explored.

When the hints were given to *S*s who failed to solve the problem after 30 min., they differed considerably in value. Thus, during the 8 min. following the Ceiling-hint 45.7% solved, whereas the Clamp-hint produced only 16.1% solutions. When, however, the hints were given at the outset, the corresponding values after 8 min. were 53.3% vs. 43.3% respectively, thus showing them to have similar values. If two hints function differently when given at the outset than when given after a period of problem solving the timing of hints becomes an important factor.

The experimental findings, in general, support the notion that directions tend to persist despite failure, and, if possible, they incorporate the hint to fit the direction. Thus, when hints are given, they can have the following effects: (1) stop the ongoing direction; (2) serve as a stimulant for the correct direction or the solution; (3) become absorbed or modified by the ongoing direction; or (4) remain in the background as a point of orientation whenever unsuccessful directions are abandoned. Then each time the individual stops a given line of thinking and attempts to generate a new approach, the hint can serve again as a stimulant.

SUMMARY

The present experiment was designed to explore the dynamics of problem-solving by studying the way hints influence it. The effects of two different hints on *S*'s behavior in solving Maier's Hatrack Problem were studied in detail.[9]

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[9] Maier, *op. cit.*, *J. exp. Psychol.*, 35, 1945, 349-360.

A total of 234 *S*s individually solved the Hatrack Problem in two separate investigations. In the first, the *S*s were given 30 min. to work on the problem before receiving one of the hints. In the second, the *S*s were given one of the hints, both hints, or neither hint after the problem was introduced, but before the *S*s attempted any solutions. It was, therefore, possible to compare the properties of the two hints (one called the Ceiling-hint, the other the Clamp-hint) and their presentation at two different stages of problem solving.

The following effect of hints were evidenced:

(1) Hints can stop the ongoing direction. In this respect the Ceiling-hint was superior to the Clamp-hint.

(2) Hints can serve as a stimulant for the correct direction or the correct solution. In this respect the Ceiling-hint was also superior to the Clamp-hint.

(3) Hints can become absorbed or modified by the ongoing direction. The Clamp-hint lent itself to this effect more readily than did the Ceiling-hint.

(4) Hints can set up false directions. The negative effect was most pronounced for the Ceiling-hint.

(5) Hints can remain in the background as a point of orientation as unsuccessful directions are abandoned. This assumption seems necessary to account for the delayed effect of hints, which was greater for the Clamp-hint than the Ceiling-hint.

It was also found that presentation of the hints before an *S* began work on the problem, rather than after, resulted in a somewhat more effective use of the hint. It thus appears that a hint has the best opportunity to initiate a new direction when a problem solver does not already have one in progress.

A STUDY OF INFLECTION-POINTS IN THE LOCUS OF ADAPTATION-LEVELS AS A FUNCTION OF ANCHOR-STIMULI

By HARRY HELSON and HENRY G. MASTERS, Kansas State University

According to the weighted log mean definition of the adaptation-level (*AL*), we might expect *AL* to increase monotonically with increase in intensity of anchors above series stimuli and to decrease monotonically with decrease in intensity of anchors below series stimuli in accordance with Equation [1]:

$$A = k\ (S^pB^qR^r) \quad [1]$$

where A is the adaptation-level, k is a constant, S is the log mean of the series stimuli, B is the anchor-stimulus, R refers to residual stimuli, and p, q, and r are weighting factors, the sum of which is taken to be 1.0.

Within limits this deduction is true as shown by the fact that even subliminal anchors have been found to raise judgments of intensity (lower *AL*) of electric shock by Black and Bevan,[1] of loudness by Bevan and Pritchard,[2] and of visual size by Boardman and Goldstone.[3] Similarly, at the upper end of the intensive scale it has been found that anchors, two to four times the upper stimulus of the series, lower judgments of lifted-weights, hence, they raise the *AL*, as reported by Helson[4] and Bevan and Darby.[5] From Equation [1], however, it is clear that if there is no background or anchor-stimulus, the term B^q reduces to 1.0 because a zero stimulus is weighted zero and $0^0 = 1.0$. In this case, *AL* is determined wholly by the series-stimuli if we assume no residual effects. Thus, *AL* is higher with zero (no) anchor than with anchor below the series-stimuli. Anchors below series-stimuli depress *AL* up to a certain point after which *AL* must reverse and approach series *AL* as the anchor approaches zero. This deduction was actually anticipated in an experiment by Pratt who found that *AL* was lower when a 15-gm. stimulus was interpolated between a standard of 100 gm. and series weights of 92, 100, and 108 gm. than

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[1] R. W. Black and William Bevan, The effect of subliminal shock upon judged intensity of weak shock, this JOURNAL, 73, 1960, 262-267.

[2] William Bevan and Joan F. Pritchard, The effect of subliminal tones upon the judgment of loudness, *J. exp. Psychol.*, 66, 1963, 23-29.

[3] W. K. Boardman and Sanford Goldstone, Effects of subliminal anchors upon judgments of size, *Percept. mot. Skills*, 14, 1962, 475-482.

[4] Harry Helson, *Adaptation-Level Theory: An Experimental and Systematic Approach to Behavior*, 1964, 135.

[5] William Bevan and C. L. Darby, Patterns of experience and the constancy of an indifference point of perceived weight, this JOURNAL, 68, 1955, 575-584.

when no interpolated stimulus was used.[6] Similarly, Helson found that the *AL* was lower with 5-gm. anchor than with 0.5-gm. anchor and series-weights of 200, 250, 300, 350, and 400 gm.[7] Inflection points in the locus of *ALs* below series-stimuli should, therefore, be found experimentally.

Turning to the upper end of the stimulus continuum, we find from the logarithmic form of Equation [1]:

$$\log A = \log k + p \log S + q \log B + r \log R \qquad [2]$$

that the influence of anchors (B) should level off with increasing intensity, but there is no *a priori* reason why there would be an inflection-point in the locus of anchors above series stimuli.

Without looking explicitly for a drop in *AL* with extreme anchors, Bevan and Darby found some loss in effectiveness of an anchor of 1950 gm. as compared with one of 1231 gm. with series stimuli ranging from 220 to 340 gm.[8] The value of *AL* with an anchor of 1950 gm. was 329 gm. as compared with *AL* of 332 gm. with anchor of 1231 gm. which, if confirmed in further experimentation, points to an inflection-point at the high end of the stimulus continuum.

The presence of inflection-points in the locus of *ALs* at both the lower and higher ends of the stimulus-continuum raises several basic questions which we shall seek to answer in this study. First, if effectiveness of anchors does not increase monotonically as anchors increase in heaviness, loudness, etc., is the weighted log mean definition of *AL* invalidated? Secondly, what is the significance of inflection-points at the high end of the stimulus-continuum? Third, can we expect inflection-points in both the low and high ends of stimulus-continua in all sense-modalities? And fourth, are there any implications from the results of these data bearing on the problem of *relevance* of anchors or the extent to which stimuli (including anchor-stimuli) pool to form level? We shall attempt to answer these questions after presenting the data of this experiment which are based on anchors varying over a range of 1200:1 using lifted-weights as the stimuli.

Subjects. Twelve college students from the Kansas State University summer session served as *Ss*. Their class-rank ranged from sophomore to graduate student. One *S* was dropped after the first session for failure to follow instructions.

Apparatus. Five medicine bottles measuring 1½ × 1½ × 3¼ in. served as the stimulus-objects. Each bottle was filled with lead shot to make a series of 100, 150, 200, 250, and 300 gm. In addition to these stimuli, there were 12 anchor-weights. While the anchor-stimuli varied in size, weight, and height, all had *tops* of equal size where they were grasped for lifting. A cardboard collar, which fitted just below the top of each container, prevented *S*s from getting differential cues from differences in size of the anchors. The differences in height were controlled by placing each stimulus on a piece of sponge rubber of appropriate height so all weights were lifted from the same starting height. A black cardboard screen was in front of *S* at all times so that the stimuli were never visible. *S* lifted the weights by putting his hand under the screen. A 5 × 8-in. card was mounted on the black screen at eye-level on which the judgment-categories were displayed throughout the testing session. The anchor weights were: 1.56, 3.12, 6.25, 12.5, 25, 50, 100, 200, 400, 800, 1600, and 2879 gm., forming a geometric series except for the last, which had to be less than twice 1600 gm. because of the physical limitations in lifting in this experimental situation.

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[6] C. C. Pratt, The time-error in psychophysical judgments, this JOURNAL, 45, 1933, 292-297.

[7] Helson, *op. cit.*, 142.

[8] Bevan and Darby, *op. cit.*, 580.

Procedure. Each *S* was tested on all anchors except the 100-gm. anchor. (The specific procedure for this anchor will be presented later.) Three test sessions were required, with four anchors presented in each session. The order of presentation for the different anchor-weights was randomly determined. Within each test-session a 5-min. recess separated the anchor series. The time between stimulus-presentations was held approximately constant by a fixed rhythm of presentation. The specific test-procedure for each *S* was as follows: with *S* standing in front of the screen *E* read these instructions:

> *Instructions.* This is an experiment in the judgment of lifted-weights. You will be given a series of weights to judge in terms of the following scale: very very heavy; very heavy; heavy; medium heavy; medium; medium light; light; very light; very very light. If you wish to use more extreme categories such as extremely light, you may do so. There is no right or wrong. We want you to report how the weights feel to you. Later you will be given other series of stimuli to judge that will be heavier or lighter than the present series. Are there any questions?

E then presented *S* two practice runs with only the five stimulus-series. Then *E* presented five test-runs, without anchor to obtain the series *AL*. The presentation of each stimulus in each run was randomized. After the five test-runs, *S* rested for 5 min. and then *E* read these instructions:

> In the following observations the stimulus you are to report on will follow another stimulus. Remember you are not judging the first stimulus; you are judging the second stimulus. Nor are you judging the second stimulus relative to the first. Please report how the second stimulus is perceived in just the same way as you did in the first series when you were given only one stimulus at a time. Use the same categories as you did before.

Then *E* presented *S* with one practice run. The anchor always preceded the series stimuli and the same procedure with respect to practice test-runs and rest-periods was followed which was used in the determination of the series *AL*. The instructions were repeated at the beginning of each test session and before the presentation of the first anchor-series. The second and third test sessions were conducted in exactly the same manner as the first test-session. None of the *S*s were informed of the details of the experiment after testing, and they were asked not to discuss the experiment with anyone else.

Due to an oversight on the part of *E*, the 100-gm. anchor was omitted in the first three test-sessions. Therefore, *E* requested the original 11 *S*s to return for one more test-session to obtain data wih the 100-gm. anchor. Of the original 11 *S*s, 6 returned for this additional test-session. It was conducted exactly like the three test-sessions before it. The data for this anchor are thus based on 6 instead of 11 *S*s.

Results. Data from 11 *S*s judging the 5 stimulus-series 5 times, each in random order with each of 11 anchors, and from 6 of the 11 *S*s judging with the 100-gm. anchor, as noted above, were averaged, and curves were fitted to each. The *AL*s were computed for each of the anchor-conditions, as well as the 'no anchor'-condition, according to Helson's formula[9] as illustrated in worked examples by Helson and Himelstein.[10] The locus of *AL*s for anchors ranging from zero (no anchor) to 2879 gm. is shown in Fig. 1. from which it appears that there are two inflection points: the first occurs with anchor of 25 gm. where *AL* is lowest, and the second at 1600 gm. were *AL* is highest. In the former case, judgments of the stimulus-series were, on the average, highest, and in the latter case, they were lowest, since there is an inverse relationship between value of *AL* and judgments of weight or any other dimension of stimuli. As values of *AL* rise with decreasing anchors below 25 gm., judgments of the heaviness of series weights decrease; similarly, there is a reversal in judgments of series weights at the high end of the continuum, since *AL* with 2879 gm. anchor is lower than with 1600-gm. anchor.

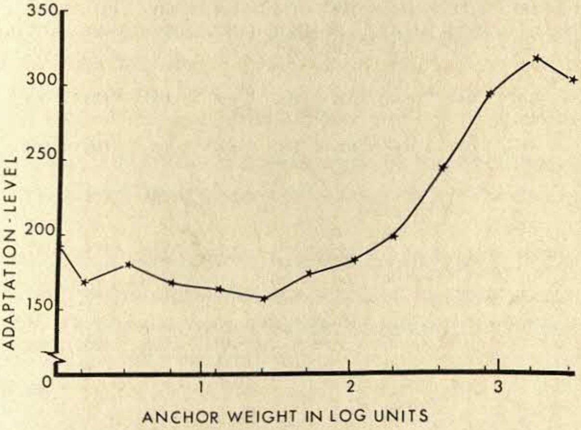

FIG. 1. ADAPTATION-LEVEL AS A FUNCTION OF ANCHOR-STIMULI

Decreasing effectiveness of visual anchors with increase in size of anchor was reported by Bevan and Pritchard, who found that when rectangles were very much larger than series stimuli they no longer functioned as anchors in judgments of shape, although their linear dimensions were in the same ratio as those of smaller rectangles that functioned as anchors.[11]

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[9] Helson, Adaptation level as a basis for a quantitative theory of frames of reference, *Psychol. Rev.*, 55, 1948, 297-313.

[10] Harry Helson and Philip Himelstein, A short method for calculating the adaptation-level for absolute and comparative rating judgments, this JOURNAL, 68, 1955, 631-637; also in Helson, *op. cit.*, 1964, 661-662.

Test of the significance of differences between *AL*s were made by means of Sandler's A-test, a modified t-test.[12] As appears from Table I, all anchors except 200-gm. anchor are significantly different from the no-anchor condition. Since the 200-gm. anchor is very close to the series *AL* of 194 gm., it would not be expected to exert significant effect on *AL*. Nor are anchors of 1.56, 3.12, 6.25, 12.5, 25.0, and 50.0 gm. at the low end and 1600 and 2879 gm. at the high end significantly different from each other. Anchors 200, 400, and 800 gm. differ significantly from each

TABLE I

SIGNIFICANCE-LEVELS OF DIFFERENCES BETWEEN *AL*S BASED ON SANDLER'S A-TEST

(Values in parentheses are the anchors)

AL (anchor)	*AL*s and Anchors (in gm.)											
	171 (1.56)	181 (3.12)	171 (6.25)	165 (12.5)	159 (25.0)	176 (50.0)	184 (100)	203 (200)	247 (400)	299 (800)	323 (1600)	309 (2879)
194 (none)	.025	.05	.01	.025	.005	.01	n.s.	n.s.	.005	.005	.005	.005
171 (1.56)		n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	.05	.005	.005	.005	.005
181 (3.12)			n.s.	n.s.	n.s.	n.s.	n.s.	.05	.005	.005	.005	.005
171 (6.25)				n.s.	n.s.	n.s.	n.s.	.005	.005	.005	.005	.005
165 (12.5)					n.s.	n.s.	n.s.	.01	.005	.005	.005	.005
159 (25.0)						n.s.	.05	.005	.005	.005	.005	.005
176 (50.0)							.025	.005	.005	.005	.005	.005
184 (100)								n.s.	.01	.005	.005	.01
203 (200)									.005	.005	.005	.005
247 (400)										.01	.005	.005
299 (800)											n.s.	n.s.
323 (1600)												n.s.

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[11] William Bevan and Joan Pritchard, The anchor effect and the problem of relevance in the judgment of shape, *J. gen. Psychol.*, 69, 1963, 147-161.

[12] Joseph Sandler, A test of the significance of the difference between the means of correlated measures, based on a simplification of Student's *t*, *Brit. J. Psychol.*, 46, 1955, 225-227.

other. Anchors from 1.56 through 50.0 gm. also differ significantly from anchors 200 through 2879 gm.

We thus see that stimulus-weights within the series which serve as anchors do not displace *AL* to a significant degree from the series *AL* or no anchor condition. On the other hand, the fact that *AL*s are higher with anchors on either side of *AL* with 25-gm. anchor is evidence for an inflection-point at this value even though there is no statistically significant difference between this anchor and the lower anchors with the exception of the zero anchor (series *AL*). Similarly, at the high end of the continuum, the 2879-gm. anchor is not significantly different from the next two highest anchors, 800 and 1600 gm., which argues for at least a levelling off, if not an inflection-point, of anchor effects at the high end of the intensive continuum.

DISCUSSION

We are now in a position to answer some of the questions raised at the beginning of this report and to venture some hypotheses regarding the others.

The first question asked above is easiest to answer. Inflection-points in the locus of *AL*s do not mean that the weighted log mean definition of *AL* is inadequate. They merely show that the weighting coefficients, p, q, and r, for series, anchor, and residual stimuli are not constant over the entire range of stimulus-intensities. This result is not surprising in view of the fact that no psychophysical law has been found or is likely to be found with fixed parameters for the entire range of stimulus-intensities. The fact that the weighting factors, p, q, and r, may vary within a given dimension, especially at the extremes of the stimulus-continuum, does not invalidate the generality or power of the weighted log mean definition. On the contrary, this definition provides the simplest and most general way of determining the contributions of the various classes of stimuli entering into *AL*. Just as variation in size of the j.n.d. over the stimulus-continuum does not invalidate the concept of the j.n.d., so variation in the weighting coefficients for series, background, and residual stimuli does not invalidate either the concept of *AL* or the concept of differential pooling in the formation of *AL*s. Where the locus of *AL*s is fairly linear, we may expect values of p, q, and r, to be fairly stable; where there are inflection-points we would expect the values of these constants to change. Determination of inflection-points can therefore furnish important information regarding the contributions of various stimuli to the formation of *AL*s and the nature of pooling processes.

The significance of inflection-points in the locus of *AL*s must be sought in the nature of the sensory and response-systems involved in judgment and in the specific conditions of experimentation. In the case of lifted-weights, we have seen that if series *AL* with no anchor stimulus is higher than with very low anchors, there must be a loss of effectiveness in anchors below a certain point. The situation is different at the high end of the scale where there is certainly no further upward shift of *AL* with heavier anchors after 1600 gm. and where the drop in *AL* may indicate an inflection-point, even though the difference between 1600 and 2879 gm. is not statistically different. It appears probable that as anchors become heavier and heavier vis-a-vis series stimuli different muscles and a different stance are employed in lifting the anchor-stimuli with resultant changes in judgment. We know that perceived weight is influenced by the shape and volume (density) of the stimulus, by the way in which the stimulus is grasped and lifted, and by other subtle factors too numerous to mention. By the time the anchor weighs over 6 lbs. (2879 gm.) as compared with an average weight of less than ½ lb. (200 gm.) for the series stimuli it is not surprising that the lifting situation is considerably different for the anchor and series stimuli. Here the inflection-point in the locus of *AL*s tells us to look for changes in stimulus and response-conditions. This argument applies also to the low end of the stimulus continuum.

The third question can be definitely answered in the negative: inflection-points are not expected, for example, at the low end of the reflectance or luminance continuum in vision. Absence of light in background does not function as a zero stimulus because black is a positive quality psychologically and, as background is made darker and darker, stimuli increase in lightness. Thus, if black velvet having about 1/10 the reflectance of black cardboard is used as a background, a white stimulus of 80% reflectance appears to glow, so great is the contrast effect under high illuminances. We would therefore not expect any decrease in effectiveness of background or anchor stimuli at the low end of the brightness-continuum. Due to glare and blinding effects of very high luminances, there may well be inflection-points in *AL*s for visual acuity and other visual dimensions at the high end of the brightness continuum. It is therefore necessary, as stated above to investigate locus of *AL*s in each sense-modality to determine effects of anchor and other stimuli.

The last question can be easily answered in the affirmative, for changes in the weighting constants, p, q, and r, give information regarding changes in the effectiveness of stimuli in pooling to form-levels. We may define relevance in terms of the degree to which stimuli affect *AL*s. If stimuli do not affect *AL*, they do not change responses. We thus have a quantitative measure of relevance in the values of the weighting coefficients for series, anchor, and residual stimuli. Wherever there are inflection-points in the locus of *ALs*, either as a function of anchors, series stimuli, or residual factors, there we can say changes occur in the relevance of the various classes of stimuli pooling to form *ALs*.

In this discussion we have considered inflection-points in the locus of *ALs* only as a function of variation in magnitude of anchor-stimuli because the series-stimuli were kept constant. It is obvious that inflection-points depend also upon series as well as anchor-stimuli and indeed are a function of both. Series and anchor-stimuli interact, and with a heavier set of series-stimuli we would expect the inflection-point at the low end of the continuum to appear earlier, *i.e.* with heavier anchor, than in this study due to the apparent lightening of anchors with a heavier set of series stimuli. It is more difficult to predict what would happen at the high end of the stimulus-continuum with a heavier set of series stimuli, owing to the fact that several factors having different effects interact. Thus stimuli are less effective with increasing magnitude in accordance with the Weber-Fechner law, yet anchors above a heavier series would feel less heavy than with a lighter series and would therefore function as lighter anchors. Still other factors may be operative at the high end of the continuum to further complicate the situation so far as predicting what will happen. This and other questions raised by the present study can only be answered by additional experiments. Good theories answer some questions and raise others. The problem of inflection-points in the locus of *ALs* is important because stimulus-continua are divided into at least three regions: the region of neutrality in the vicinity of *AL* and also into bipolar opposites, such as heavy and light, bright and dim, warm and cold, above and below *AL*. Exact determination of these regions has practical as well as theoretical importance, the possibilities of which should be fully explored in studies of inflection points in anchor and series effects.

SUMMARY

Inflection-points in the locus of *ALs* were studied as a function of anchors varying from 1.56 to 2879 gm. or 1200:1 with series-stimuli of 100, 150, 200, 250, and 300 gm. Due to the fact that series *AL* is higher with zero anchor than with anchors below the series, including subliminal anchors, it was predicted from the weighted log mean definition that an inflection-point would be found with anchors below the series stimuli, and this deduction was verified experimentally. With anchors above the series

stimuli, we expect a levelling off in their effectiveness from the definition of *AL*, but a slight drop in *AL* with the heaviest anchor suggests that there may also be an inflection-point at the high end of the stimulus-continuum as well as at the low end. Since larger muscle groups and a new mode of lifting are required with extremely heavy weights, it is not surprising that extremely heavy stimuli are less effective as anchors. The presence of inflection-points in the locus of *AL*s does not invalidate the weighted log mean definition; they merely point to the fact that the weighting coefficients for series, background (anchor), and residual stimuli are not constant over the whole stimulus-range. In view of the lability and complexity of receptor, central, and motor systems, it can hardly be expected that any psychophysical parameters will remain fixed over the entire range of stimulus-magnitudes.

Inflection-points at the low end of the stimulus-continuum are expected only in the case of sense-modalities having a true zero. Such is not the case, *e.g.* in vision, where absence of stimulation in a part of the field gives rise to positive qualities like black or complementary colors. Changes in the weighting coefficients of series and background stimuli make possible quantitative determination of degrees of relevance of stimuli in the pooling process under various conditions. A number of questions raised by the present study were answered and still others remain to be investigated within the framework of *AL* theory.

CROSS-MODAL JUDGMENTS OF LENGTH

By ROBERT S. DAVIDON and JAMES H. MATHER, Bryn Mawr College

To account for the gross correspondence of spatial judgments in vision and active touch, it has been proposed that the inputs of the spatial senses are ordered in relation to a common schema.[1] If it would have all points in the perceptual field bear the same relation to each other regardless of modality, the theory of a common schema can be refuted. Relative distance is distinctly different in vision and in active touch without vision; apparent extent varies with position in the field for tactile-kinesthetic stimuli as it does for visual stimuli, but the two functions are not the same.[2] If, however, the common schema refers to a set of subjective scales of judgment, each with values approximately the same when corresponding observations are made in two or more modalities, further investigation is required to define the conditions and degree of correspondence. With a common response-scale, a point of subjective equality (*PSE*) can be determined with the standard and the comparison-stimuli in different modalities. If the cross-modal *PSE* varies consistently with the physical magnitude of the standard, an interpretation in terms of a coördinate scale of reference is appropriate.

As evidence for a common schema, Kelvin first reported that the *PSE* was the same when standard and comparison-lengths both were in one modality as it was when they were in different modalities.[3] In a later study, with the range of the comparison-series varied and the values of the standard away from the center of the series, Kelvin and Mulik found the *PSE*

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[1] R. C. Oldfield and O. L. Zangwill, Head's concept of the schema and its application in contemporary British psychology. Part I. Head's concept of the schema, *Brit. J. Psychol.*, 32, 1942, 267-286; Part II. Critical analysis of Head's theory, *ibid*, 33, 1942, 58-64; M. D. Vernon, The functions of schemata in perceiving, *Psychol. Rev.*, 62, 1955, 180-192; M. Critchely, The body image in neurology, *Lancet*, 1, 1950, 335-340.

[2] R. S. Davidon and Mei-Fang Hsieh Cheng, Apparent distance in a horizontal plane with tactile-kinesthetic stimuli, *Quart. J. exp. Psychol.*, 16, 1964, 277-281; Mei-Fang Hsieh Cheng, Haptic perception of extent, unpublished doctoral dissertation, Bryn Mawr College, 1965; T. M. Künnapas, The vertical-horizontal illusion and the visual field, *J. exp. Psychol.*, 53, 1957, 405-407; A. A. Blank, Curvature of binocular visual space: An experiment, *J. opt. Soc. Amer.*, 51, 1961, 335-339.

[3] R. P. Kelvin, Discrimination of size by sight and touch, *Quart. J. exp. Psychol.*, 6, 1954, 23-34.

for cross-modal judgments closer to the middle of the comparison-series (farther from the standard) than the *PSE* for ipse-modal judgments.[4] They were led by the results to reject the hypothesis of a common schema. They concluded that the *PSE* in cross-modal matching is an experimental artifact —merely the neutral value of the comparison-series—and that the cross-modal standard has no significant effect on adaptation-level (*AL*). The experiments reported here were designed to evaluate these conclusions.

EXPERIMENT I

From the hypothesis of a common subjective scale, it follows that a discriminable change in the length of the standard should produce a change in *PSE* of the same order of magnitude for cross- as for ipse-modal judgments. The changes in standard (anchor) should shift the *AL* in the same direction but much less than the shift in *PSE,* for the *AL* would continue to be influenced by the constant stimuli of the series, background, and residual factors. Furthermore, if the second standard in the same modality affects the categories applied to the series, the corresponding standard in the other modality should have a similar effect. That intermodal anchor-effects can occur has been demonstrated with judgments of the duration of visual and auditory stimuli.[5] Were it true, as Kelvin and Mulik asserted, that the *PSE* in cross-modal matching is the neutral point of the comparison-series, then in the present experiment, in which changes in the length of the standard were introduced with the same variable-series, there should be no consistent difference between *PSE* and neutral point (*AL*), either after the change in the cross-modal standard or before.

Method. To test the correspondence of *AL* and *PSE,* a method is required which permits the measures to vary independently. Helson's method of comparative category-judgments equates comparative adaptation-level (*CAL*) and *PSE* by definition.[6] Each is an index of the neutral point of the same distribution of judgments of 'greater' and 'less.' Furth computed *CAL* and *PSE* for each *O* to provide a numerical illustration of their close correspondence.[7] To test for the effect of a subsequent standard upon the subjective scale applied to the same series-stimuli, however, a floating neutral point is necessary, one which is not 'pegged' by instructions to each standard. For this reason, an unusual method of judgment was introduced in the present experiment. Category-judgments of the absolute magnitude of each member of the series permitted the computation of a series–*AL*, while separate judgments were made of the standard. In principle, the series–*AL* can be independent of the *PSE*, although in fact may covary with it.

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[4] R. P. Kelvin and Aktu Mulik, Discrimination of length by sight and touch, *Quart. J. exp. Psychol.,* 10, 1958, 187-192.

[5] Isaac Behar and William Bevan, The perceived duration of auditory and visual intervals: Cross-modal comparison and interaction, this JOURNAL, 74, 1961, 17-26.

[6] Harry Helson, W. C. Michels, and Artie Sturgeon, The use of comparative rating scales for the evaluation of psychophysical data, this JOURNAL, 67, 1954, 321-326.

[7] H. G. Furth, The effect of the size-weight illusion on adaptation level, *J. exp. Psychol.,* 60, 1960, 150-154.

Observers. The *O*s were 32 undergraduate women.[8]

Apparatus. The series consisted of eight steel bars varying from 52-66 mm. in length in steps of 2 mm. The two standards were 58 and 62 mm. long. All bars were of the same 9.5 × 9.5-mm. stock with a flat black finish. For each judgment, the standard and comparison-bars were positioned vertically, one above the other and 5.5 cm. apart, on a uniformly illuminated white background. Two vertical shutters, each 10.2 × 23 cm., one for the upper and one for the lower half of the field, were set 12.7 cm. in front of *O* and 28 cm. from the background. A programmer produced a ready-signal that flashed 2 sec. before each trial and timed both the 3-sec. observation-period for each bar and the 12-sec. intertrial-interval. A telegraph-relay produced pairs of clicks to mark intervals for tactual judgments.

Experimental design. The independent variables were the modality of the standard (visual or tactual), the modality of the series (visual or tactual), the order of the standard and variable, and the length of the standard. All the *O*s had the same standard near the center of the series for the first block of trials and a longer standard for the second. A 2 × 2 × 2 factorial design was used for the first three variables, with four *O*s randomly assigned to each of the eight conditions.

The stimuli were presented successively in pairs: a standard bar which was a member of every pair in a block of trials, and a bar of the series which varied randomly in length from trial to trial. *O*'s task was to judge the length of *each* bar of the pair on the same scale of 1-9—the longer the bar, the larger the number to be assigned. Two bars of different length never were to be given the same number. *O* was not told that one bar was the same from trial to trial, nor was he told how many different lengths would be presented.

The specific procedure for one experimental condition—that with a tactual standard presented before each member of the visual series—provides a detailed illustration. With head in headrest, *O* rested his right elbow on the table below the shutter-panel. *O*'s fingers, not visible behind the panel, touched the background in a position to grasp the ends of the standard bar. *O* was told that after the flash of a warning light a click would sound, at which time *O* was to touch the bottom of the lower bar with the thumb and the top with the index finger repeatedly until the next click. Immediately after the second click, the upper shutter would open, presenting the second bar of the pair for visual inspection. Judged values for the two bars were not to be reported until the shutter had closed. Each *O* had 16 practice-trials in which tentative judgments were to be made but did not have to be reported. In the first eight of these trials, a 62-mm. standard was presented before each member of the series; then, without further instruction or delay, a 58-mm. standard was presented with each bar of the series, with series-bars in a different random order. In the first block of 40 recorded trials, the 58-mm. standard was used in each pair, while the series was presented in 5 different random orders. After a 2-min. rest, the second block of 40 trials was presented, consisting of a

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[8] Five *O*s whose responses were not discriminative were replaced: four gave the same response to extreme members of the series, and one responded with the same number to every presentation of the standard.

62-mm. standard with each member of the series in 5 different orders. *O* was not told that there would be a change in stimulus-conditions in the second block.

The timing and the sequence of stimuli were the same for all eight conditions. For the *O*s in the group for which all judgments were tactual, the shutter-panel was removed and a blindfold was used.

Measures. The *PSE* for each block of trials was computed by finding the mean of the numerical judgments for a given standard and determining from a regression-line of best fit the length within the series which had been given the same judged value. Before the computation of *AL*s, linear transformations were employed to equate *O*'s judgmental scales for mid-category and for category-range, based on the first block of trials. The short method for absolute judgments was used to compute *AL*-series for the same eight bars of the series for each block of trials under all conditions.[9]

Results. For the same series, an increase in the length of the standard from 58 to 62 mm. was associated with a mean increase in *PSE* of 4.9 mm.

TABLE I

PSE (IN MM.) FOR IPSE- AND CROSS-MODAL PAIRED JUDGMENTS

		Modality of series			
Modality of standard	Length of standard	Visual		Tactual	
		Mean	σ	Mean	σ
Visual	58	57.6	1.03	56.4	5.03
	62	61.2	2.01	61.6	8.58
Tactual	58	58.9	5.81	58.2	2.57
	62	62.6	8.04	65.3	4.71

In the analysis of variance, this difference in mean *PSE* as a function of length of standard was significant ($F = 41.3$, $df = 1/24$, $p < 0.001$). As indicated in Table I, the mean change in *PSE* was approximately the same for intermodal judgments as it was when the standard and series were in the same modality. The cross-modal *PSE* was not merely the neutral value of the series, but varied predictably with changes in the length of the standard. The mean differences in *PSE* attributable to modality of the standard, to modality of the series, and to order of the standard and series, were not significant at the 5% level, nor was any of the interactions. These results support the hypothesis of a common schema. Absolute judgments of length with active touch were quantitatively comparable to those with vision.

The variability among the *O*s in the intermodal groups was significantly greater than in the intramodal groups. The finding of significant heterogeneity of variance among experimental groups ($F_{\max} = 286$, $df = 8/3$, $p < 0.01$) restricts the interpretation of between-group differences. The finding of a difference in variability of *PSE*s, does, however, provide an alternative explanation of Kelvin and Mulik's results. If intermodal judgments are less reliable, and if *O*s cannot readily discriminate the difference between the length of the standard and that of the extreme member of the series — a difference of only 2 mm. in their study — then response-distributions should be more skewed for the cross- than for the ipse-modal conditions. The mean of the cross-modal *PSE*s would be nearer to the center of the series, as Kelvin and Mulik reported, but each *O* would not be equating the standard to the midpoint or neutral point of the series.

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[9] Harry Helson and Philip Himelstein, A short method for calculating the adaptation-level for absolute and comparative rating judgments, this JOURNAL, 68, 1955, 631-637.

With respect to *PSE*s, it should be noted, finally, that the variability of judgments of the standard was such as to indicate little awareness on the part of the *O*s that one bar of each pair was constant from trial to trial. The median number of different numerical categories used to designate the standard was 4, both in the first and in the second block of trials.

TABLE II

SERIES-*AL*s WITH PAIRED JUDGMENTS

Modality of standard	Length of standard	Modality of series: Visual Mean	Visual σ	Tactual Mean	Tactual σ
Visual	58	58.2	0.65	58.1	1.40
	62	58.7	1.13	58.3	3.27
Tactual	58	58.4	1.61	58.4	0.92
	62	58.0	1.43	60.4	3.04

The mean *AL* with each standard for each group of *Os* is given in Table II. Unlike the *PSE,* the *AL*-series did not shift significantly with an increase in the length of the standard. For the second block of trials (with the 62 mm. standard), the mean *AL* and *PSE* differed significantly ($p < 0.01$). In the analysis of variance with *AL* as the measure, none of the treatment-effects or interactions was significant. The relative magnitude of cross-modal anchor-effects, if any, could not be assessed, for the change under these conditions was not adequate to affect *AL* significantly, even when standard and series were observed in the same modality.

Discussion. The marked increase in *PSE* without a significant shift in *AL* not only refutes the supposition that the cross-modal *PSE* corresponds to the neutral value or *AL,* but also indicates the importance of distinguishing these measures in comparative judgment — a distinction which has been ignored or denied. The lack of a detectible change in *AL*, particularly for the groups with the visual series, may be attributed to the relatively small change in standard, to the heavy weighting of the background-factor, and to the residual effect of the initial block of judgments. For the visual judgments of length, the rectangular aperture provided a constant frame or background. Künnapas has demonstrated that the visual frame influences judgments of length.[10]

EXPERIMENT II

The *PSE* normally is obtained by direct comparative judgments. A general conclusion about the similarity of cross- and ipse-modal *PSE*s requires evidence that the findings of Experiment I are not specific to the novel method of paired absolute judgments, nor to the particular index of *PSE*. Furthermore, since the *PSE* may change with practice, evidence is required of the effect of a *decrease* in the length of the standard.

Observers. The *O*s were 24 college students who had not participated in the previous experiment. They were divided into an experimental group of 16 *O*s and a control group of 8 *O*s.

Apparatus. Two new standard bars, one 60 mm. long and the other 56 mm. long, were used. In all other respects, the apparatus was the same as in Experiment I.

Method. For the method of constant stimuli with dichotomous comparison, the 16 *O*s were divided into four equal groups with the modality of the standard and of the series varied in a 2 × 2 factorial design. For the method of paired judgments, as used in Experiment I, the 8 *O*s were divided into two subgroups with only modality of the standard varied; the series was visual. All *O*s had the same standard near the center of the series for the first block of trials and a *shorter* standard for the second.

The stimuli were presented successively in pairs, as before. The task of the *O*s in the comparative-judgment group was to judge the length of the first bar of a pair (one of the series) on a scale of 1-9; then to judge whether the second bar (the standard) was longer or shorter than the first. The two judgments were announced only after both bars had been presented. The procedure for paired absolute judgment was the same as in Experiment I, as were also the timing, the number of trials, and other details of presentation.

Measures. For the comparative-judgment group, the *PSE* for each block of trials was computed by the normal-interpolation process.[11] *PSE*s for the other *O*s and all *AL*s-series were computed as in Experiment I.

Results. For the purpose of comparing methods of judgment, the initial analysis of *PSE*s was based on the data of two control and two experimental groups — those with a visual series and the modality of the standard varied. Means and variabilities for the four groups are given in the column for visual series of Table III. With both methods — dichotomous comparison and paired absolute judgment — there were changes in *PSE* corresponding to changes in the standard ($F = 101.6$, $df = 1/12$, $p < 0.001$), and the difference between methods was not significant ($\alpha = 0.05$). Nor was there a significant difference in *PSE* between the cross- and ipse-modal conditions, results which add to the generality of a conclusion of Experiment I. In contradiction of the previous results, however, there were significant interactions here. An interaction between change in length and modality of the standard ($F = 21.8$, $df = 1/12$, $p < 0.001$) can be attributed to the difficulty experienced by the *O*s in relating a tactual standard to a visual series. The *O*s in this group had a greater mean *PSE* for the 56-mm. standard than for the 60-mm. standard, which was the only instance in the two experiments of mean *PSE*s that did not correspond to the stimuli in relative length. This finding of asymmetry — that a visual standard can be more reliably and accurately related to a tactual series than can a tactual standard to a visual series — is evidence against a single scale or schema for the judgment of length.

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[10] Künnapas, Influence of frame size on apparent length of a line, *J. exp. Psychol.*, 50, 1955, 168-170.

[11] J. P. Guilford, *Psychometric Methods*, 2nd ed., 1954, 123-124.

TABLE III

*PSE*s (IN MM.) AS A FUNCTION OF METHOD OF JUDGEMENT AND MODALITY

			Modality of series			
			Visual		Tactual	
Method	Modality of standard	Length of standard	Mean	σ	Mean	σ
Dichotomous comparison	Visual	60	60.2	0.96	58.5	3.22
		56	57.9	0.55	56.2	3.92
	Tactual	60	57.8	3.45	60.4	0.92
		56	58.3	3.55	57.6	0.82
Paired absolute judgments	Visual	60	59.2	0.62	—	—
		56	55.9	1.05	—	—
	Tactual	60	63.0	3.95	—	—
		56	60.4	3.22	—	—

The mean *AL*s for the same four groups are given in Table IV. With but one exception, the results of the analysis of variance of the *AL*s were similar to those of Experiment I. Again there was no significant change in *AL* associated with change in the length of the standard, and *AL* was not directly related to the modality of the standard or of the series, or their interaction. There was, however, a significant interaction between change in length and the modality of the standard ($F = 6.56$, $df = 1/12$, $p <$

0.025), with an appreciable shift downward in *AL* for the visual (ipse-modal) standard and a slight increase with the tactual (cross-modal) one.

A separate statistical analysis of *PSE*s for all four experimental subgroups (dichotomous-comparison method) provided a test of intermodal effects comparable to those of Experiment I, without confounding the modality of the standard and the ipse- *vs.* cross-modality conditions, as in the previous analysis with some of the same *O*s. With *O*s instructed to make direct comparisons, there was a significant change in *PSE* with a change in standard ($F = 89.58$, $df = 1/12$, $p < 0.001$); again there were no significant effects attributable to the modality of the standard, to the modality of the series, or to the interaction between modality of the standard and of the series (ipse- *vs.* cross-modal). There was a significant interaction between length of the standard and modality of the standard ($F = 12.27$, $df = 1/12$, $p < 0.01$). A greater change in *PSE* was associated with a change in length for the visual than for the tactual standard. This interaction is consistent with the previous finding of greater accuracy for a visual standard and a tactual series than for a tactual standard and a visual series. The interaction between change in length of the standard and the modality of the series was significant ($F = 20.65$, $df = 1/12$, $p < 0.001$), with greater changes for the tactual series.

TABLE IV

SERIES-*AL*s FOR TWO METHODS OF JUDGMENT

		Modality of standard			
	Length of	Visual		Tactual	
Method	standard	Mean	σ	Mean	σ
Dichotomous comparison	60	58.1	0.33	57.5	1.67
	56	57.6	0.88	57.5	1.27
Paired absolute judgment	60	58.8	0.84	58.1	1.04
	56	57.0	0.88	58.5	2.09

To check more thoroughly for shifts in the subjective category-scale than was possible with a single index (*AL*) under each experimental condition, the graphs of Fig. 1 were prepared to show the mean judgment for each series-stimulus for each block of trials. With tactual standard and tactual series, the judged magnitude *increased* for each member of the series after the shorter standard was introduced. The effects were similar, but not as consistent, with visual standard and series. For cross-modal judgments, no effect upon the subjective scale of judgment was indicated. One may infer that if there is a cross-modal anchor-effect in the judgment of length, it will be less than the effect of an anchor in the same modality. As indicated in Fig. 1, the influence of a visual standard upon categories applied to a tactual series was less than that of the tactual standard of the same length presented with this series.

Discussion. The findings with respect to the *PSE* generally confirm those of Experiment I. For either method of judgment, the cross-modal *PSE* varies consistently with the standard, and the cross-modal *PSE*s have approximately the same values as the ipse-modal ones. The evidence of a difference in anchor-effects and of asymmetry associated with the modality of the standard does, however, preclude an interpretation in terms of a single scale of reference with equal values in each modality. A more complex model of intermodal judgment appears necessary to account for the fact, now demonstrated and verified, that category-judgments of length with

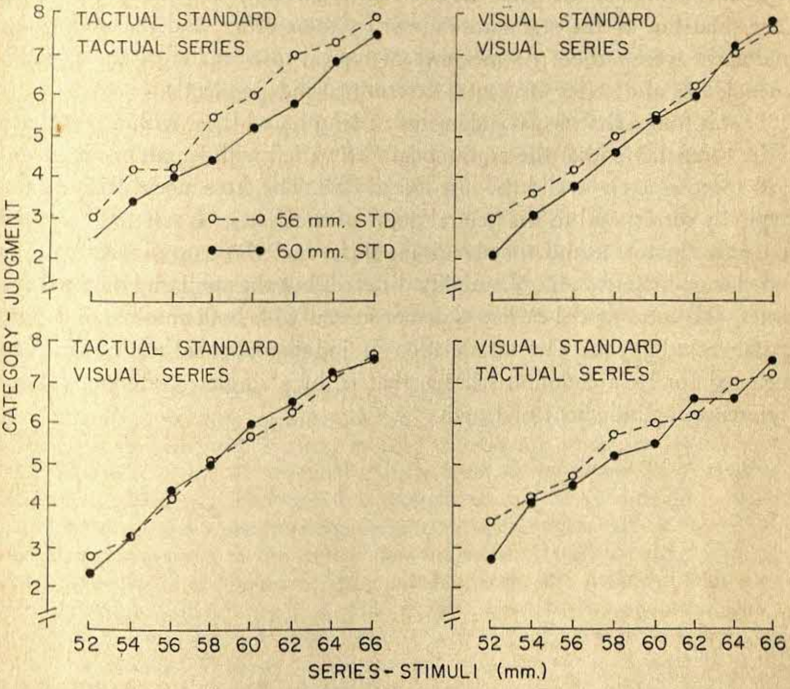

FIG. 1. MEAN JUDGMENT OF LENGTH FOR EACH SERIES-BAR WITH TWO STANDARDS
With modality of standard and series varied, *O*s in four subgroups judged each series-bar, first with a 60-mm. and then with a 56-mm. standard. Category-values were transformed as in Experiment I.

active touch are closely associated with visual judgments of similar objects. The model should provide for differences between modalities in the 'weight' of particular stimuli in determining the respective *ALs*, and for differences in the variability and lability of the category-judgments.

SUMMARY

Judgments of relative apparent length were obtained with different standards in relation to the same series of bars, tactually or visually perceived. The modality of the standard and series, their order, and the method of judgment were varied. As measures of typical ipse- and cross-modal judgment, *PSEs* and series *ALs* were determined independently.

It was found that visual judgments of length and those with active touch were comparable, that the cross-modal *PSE* varied with length of the standard approximately as did the ipse-modal *PSE*. The cross-modal *PSE* did not typically correspond to the neutral point of the series (as asserted by previous investigators), and the significant shifts in *PSE* occurred with little or no change in series *AL*. Variability differed, but the similarity of the mean inter- and intra-modal *PSE* was demonstrated with both absolute and comparative judgments. The interrelation of judgments could not be fully accounted for by a common schema, that is, by a single coördinate scale of reference for judgment of length.

SOME PROPERTIES OF THE REACTION-TIME READY-SIGNAL

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In 1911, Ladd and Woodworth wrote that "the preparation for reaction is clearly an essential part of the process; without some preparation the reaction would not occur at all, and the more perfect the preparation, the more prompt the reaction."[1] Among the factors influencing preparation in the reaction-time (*RT*) experiment is the ready-signal; yet, the properties of the ready-signal and their effects on *RT* have been largely ignored. Since at least the time of Wundt (1887), it has been known that if the ready-signal was omitted, the time of reaction was increased.[2] Furthermore, Geblewiczowa has demonstrated that the use of two ready-signals, closely spaced, produced shorter *RT*s than did a single ready-signal, or two that were more widely separated.[3]

In the present experiments, intensity and mode of the ready-signals were manipulated. The choice of these variables, derived from a model of the *RT* experiment based upon analogies with classical aversive conditioning paradigms. We believe that components of the *RT* experiment share many of the properties of those in conditioning. Thus, the ready-signal is homologous to the conditioned stimulus (*CS*), the fore-period to the conditioned-unconditioned stimulus (*CS-UCS*) interval, and the response-signal to the unconditioned stimulus (*UCS*). Starting with the latter, a number of studies have demonstrated that the *RT* is shorter with more intense response-signals than it is with weaker signals.[4] Similarly, conditioning occurs faster and is generally more effective with stronger than with weaker unconditioned stimuli.[5] Variation of the fore-period in *RT* studies frequently yields U-shaped functions with the optimal fore-period occurring in the vicinity of 0.5 to 2 sec.,[6] which corresponds to the range of optimal *CS-UCS* intervals in classical

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[1] G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology,* 1911, 482.

[2] Wilhelm Wundt, *Physiologische Psychologie,* 3d edition, II, 1887, 287.

[3] Maria Geblewiczowa, Influence of the number of warning signals and of the intervals between them on simple reaction-time. *Acta Psychol.,* 21, 1963, 40-48.

[4] W. J. McGill, Stochastic latency mechanisms, in R. D. Luce, R. R. Bush, and Eugene Galanter (eds.), *Handbook of Mathematical Psychology,* 1963, 327-332; David Raab and Elizabeth Fehrer, Supplementary report: The effect of stimulus-duration and luminance on visual reaction time, *J. exp. Psychol.,* 64, 1962, 326-327.

[5] K. W. Spence, D. F. Haggard, and L. E. Ross, Intrasubject conditioning as a function of the intensity of the unconditioned stimulus, *Science*, 128, 1958, 474-475.

[6] Lawrence Karlin, Reaction-time as a function of foreperiod duration and variability, *J. exp. Psychol.* 58, 1959, 185-191; Risto Näätänen, On the interrelations

aversive conditioning.[7] If the response-signal in the *RT* task is response-terminated, *RT* is shorter than when the response-signal persists even briefly after the response.[8] In conditioning studies, evidence is ample that the *UCS* is more effective if it is response-terminated.[9] With respect to the *RT* ready-signal, very little can be said, except that its presence is clearly important for rapid *RT*s.

In the framework of the present discussion, the reason that the ready-signal plays a critical role is that this signal becomes a true conditioning stimulus for eliciting the various *preparatory responses,* the presence of which determine the 'goodness' or speed of response. These responses may include: postural adjustments, orienting or attending responses to the expected response-signal, alpha blocking, and a motor set to respond of the responding digit.

If, indeed, these preparatory responses become conditioned to the ready-signal, then the effective properties of the signal to elicit these conditioned responses may play an influential role in the *RT*. In the literature on human conditioning there is, however, only equivocal evidence that the *CS* are themselves important, since according to Kimbe, mediational processes may be the "true" *CS*.[10] Grice and Hunter, however, have demonstrated a reliable effect of intensity of the *CS* in the conditioning of the eyelid when the separate intensities were presented in a within-*S*s design instead of the more traditional between-*S*s design.[11]

Experiment I: Ready-Signal Intensity

In the first study, three levels of white noise (60-db. range) were used as ready-signals. As the type of experimental design influences the effect of intensity of the *CS* in classical conditioning, both within- and between-*S*s designs were employed in the present study to determine if parallel effects occur in the *RT*. In addition, the fore-period was varied in order to determine whether the effect of the intensity of the ready-signal, if obtained, varied with different fore-periods.

Subjects. Forty-two enlisted technicians, between the ages of 17 and 24 yr., served as *S*s. None had previous training in psychology or prior experience in *RT* experiments.

Apparatus. The auditory ready-signal consisted of the output of a General Radio Noise Generator, fed through three Daven variable attenuators set at 30 db., into Willson earphones. The duration of the ready- and the response-signals as well as the foreperiod was determined by recycling Hunter timers. The intensity of the ready-

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between foreperiod and the reaction time in simple reaction experiments, *Reports from the Psychological Institute, University of Helsinki,* 1963, No. 1, 1-22.

[7] G. A. Kimble, E. R. Hilgard, and D. G. Marquis, *Conditioning and Learning,* 1961, 155-160.

[8] Rube Chernikoff and W. J. Brogden, The effect of response termination of the stimulus upon reaction time, *J. comp. physiol. Psychol.,* 42, 1949, 357-364.

[9] R. C. Bolles and J. A. Warren, Jr., Effects of delayed UCS termination on classical avoidance learning of the bar-press response, *Psychol. Rep.,* 17, 1965, 687-690.

[10] Kimble, Hilgard, and Marquis, *op. cit.,* 120.

[11] G. R. Grice and J. J. Hunter, Stimulus intensity effects depend upon the type of experimental design, *Psychol., Rev.,* 71, 1964, 247-256.

signal was selected by an 8-bank, 25-position stepping switch which shorted out the appropriate number of attenuators for the intensity programmed for each trial. The white noise was below audible threshold during the intertrial interval (-2 db.) and was either approximately 28 db. (Intensity 1), 58 db. (Intensity 2), or 88 db. (Intensity 3) sound-pressure level (*SPL*) for subjectively soft, moderate, and very loud ready-signals. The visual response-signal was an increase in luminance to 30-ft. candles from an ambient level of 10-ft. candles, produced by adding resistance in the circuit of a fluorescent lamp. The output of the lamp was so diffused and masked by a circular aperture that it subtended a visual angle of 1°. A black cross centered in the circle served as a fixation-reference. *S* rested his hand on a spherical platform upon which were fixed three metal studs. When *S* placed his index finger on the center stud, he completed the controlling circuit of a Hunter photo-contact relay. A ground electrode was attached to the forearm of the non-responding hand. The response-time was measured to 1 m.sec. by a Hunter Klockounter which was controlled by the Hunter contact relay, starting from the onset of the response-signal.

Procedure. The *S*s were seated in a sound-treated room adjacent to that occupied by *E* and the programming equipment. They were instructed to rest the index finger of the preferred hand on the appropriate outside, brass stud. When they heard a signal from the earphones, they were to move the index finger onto the center stud and "get ready." As soon as the intensity of the visual response signal increased, they were to lift the finger straight up, off the center stud as fast as they could and rest it on the appropriate side stud. They were cautioned not to make anticipatory responses.

Both a within-*S*s and a between-*S*s design were used. The 12 *S*s in the former condition received 48 trials on each of four consecutive days. On the first day they received a random ordering of 16 trials at each noise-level, divided approximately equally over three fore-periods (1, 3, and 8 sec.). This day constituted practice. On subsequent days, they received a constrained (runs limited to three) random ordering of the three intensities; all trials with one of the three fore-periods. Fore-periods were counterbalanced among the *S*s during the three test-days. Fore-period variability on each day was obtained by using the scheduled fore-period plus 10 and 20% randomized over trials. The ready-signal persisted throughout the fore-period and overlapped with the response-signal, the duration of which was fixed at 0.3 sec. The intertrial interval was 20 sec. (*i.e.* from the onset of the ready-signal to the onset of the next ready-signal).

The 30 *S*s in the between-*S*s conditions were assigned at random to one of the same three levels of intensity. All were tested only with the 3.0 to 3.6 fore-period range. They were given one session of 54 trials of which the first six were practice.

The data used for analysis for the within-*S*s scores were the arithmetic means of the 14 *RT*s in each condition remaining after the shortest and longest *RT* were eliminated. For the between-*S*s data, the arithmetic mean was calculated for the 42 *RT*s remaining after the three shortest and longest *RT*s were eliminated.

Results. The mean *RT*s for each intensity and fore-period for the within-*S*s conditions are portrayed graphically in Fig. 1. It can be seen that *RT*s decrease with increased intensity of the ready-signal, and that this decrease is proportional at each fore-period. The decrease in *RT* with intensity is significant at the 0.001 level ($F\ 2/22 = 9.80$), but the intensity-by-fore-period interaction is not significant (F 4/44 < 1). *RT*s increase markedly between fore-periods of 1- and 3-sec., and gradually between 3 and 8 sec. The effect of fore-period is also highly reliable (F 2/22 $=$ 45.2, $p < 0.001$).

The mean *RT*s for the soft, moderate, and loud ready-signals for the *S*s in the between-*S*s conditions were 230, 226, and 225 m.sec., respectively. This difference is not reliable (F 2/27 < 1).

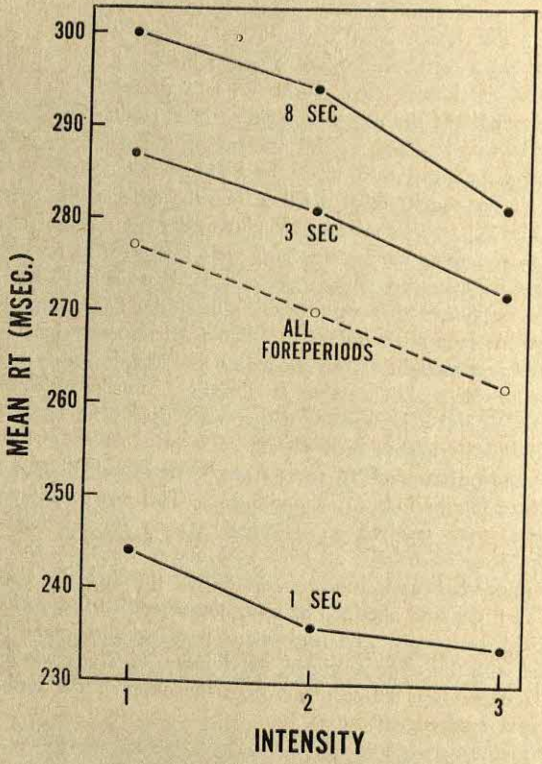

FIG. 1. MEAN *RT* AS A FUNCTION OF READY SIGNAL INTENSITY
The parameter is fore-period.

Discussion. The fore-period was varied in the present study over only a relatively small range, and only to determine whether this variable interacts with the intensity of the ready-signal. No attempt was made to compare the effects of the variations in the fore-period with those of the *CS-UCS*. From the data of the present study it would be fruitless to speculate about the value of the optimal fore-period; a more extensive study would be needed. The shape of the fore-period function is in general agreement with those previously obtained. Since this function, however, appears to depend upon a number of factors, no particular significance should be attached to any given fore-period function.[12] For our purpose, we may conclude that over the range of fore-periods explored, the effect of the intensities of the within-*S*s ready-signals are essentially linear.

The magnitude of the within-*S*s intensity was about the same as that obtained by Grice and Hunter, when they manipulated the intensity of the response-signals.[13] They had also a significant effect between *S*s in contrast to the present findings. The reason for the difference in results is, as we believe, that manipulating the intensity of the response-signal in *RT* is analogous to manipulating the intensity of the *UCS*. The effects of the intensity of the *UCS* and response-signal are well documented and usually are obtained when either of the designs are used. The intensive effects of the *CS* and ready-signal, on the other hand, appear to depend upon the type of experimental design, as shown by Beck, and by Grice and Hunter, for the intensive effects of the *CS* and in the present study for the intensity of the ready-signal.[14]

The monotonic decrease of *RT* with increasing intensity of the ready-signal, obtained with the within-*S*s design, suggests a dynamistic interpretation of the results in terms of stimulus-intensity.[15] The lack of an effect of intensity with the design between-*S*s indicates, however, that either some form of contrast was operating, or that differential habituation occurred in the two situations.[16] Whatever the mechanism, it appears to be independent of the fore-period.

The finding that *RT*s are, over-all, considerably longer with the within-*S*s design than with the between-*S*s design suggests an interpretation of informational uncertainty. It is well known that stimulus-ucerainty and response-uncertainty profoundly influence *RT*.[17] The present data indicate that uncertainty regarding the ready-signal may play a similar role.

EXPERIMENT II: MODE OF THE READY-SIGNAL

In Experiment II, the mode of presenting the ready-signal provided a further test of our conditioning model for *RT*. The ready-signal either was very brief, followed by a silent period and then the response-signal (trace), or the ready-signal persisted from its onset throughout the entire fore-period and overlapped with the response-signal, both signals terminating simultaneously (delayed). As the results of conditioning studies indicate that the superiority of delayed presentation of the *CS* over that of trace increases with increasing *CS-UCS* interval, several fore-periods were used.

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[12] E. T. Klemmer, Time uncertainty in simple reaction time. *J. exp. Psychol.*, 51, 1956, 179-184.

[13] Grice and Hunter, *op. cit.*, 247-256.

[14] Sally B. Beck, Eyelid conditioning as a function of CS intensity, UCS intensity, and Manifest Anxiety scale score. *J. exp. Psychol.*, 66, 1963, 429-438; Grice and Hunter, *op. cit.*, 247-256.

[15] C. L. Hull, Stimulus intensity dynamism (V) and stimulus generalization, *Psychol. Rev.*, 56, 1949, 67-76.

[16] S. S. Fox, Evoked potential habituation rate and sensory pattern preference as determined by stimulus information, *J. comp. physiol. Psychol.*, 58, 1964, 225-232.

[17] P. D. Bricker, The identification of redundant stimulus patterns, *J. exp. Psychol.*, 49, 1955, 73-81; Isaac Behar, On the relation between response uncertainty and reaction time in category judgments, *Percept. mot. Skills*, 16, 1963, 595-596; I. M. Schlesinger, Response uncertainty as a determinant of reaction time. *Acta Psychol.*, 22, 1964, 52-59.

Subjects. Twelve enlisted technicians served as *S*s. Several of them had previously participated in Experiment I.

Apparatus. The ready-signal consisted of the output of a General Radio Noise Generator fed into Willson earphones. Its intensity was approximately 73 db. *SPL.* The duration of the ready-signal with trace-presentation was 0.1 sec. The response-signal consisted of a 0.3-sec. increase in luminance to 27 ft.-c. from an ambient level of 7 ft.-c. of the 1° circular aperture. The response-time was measured as in Experiment I.

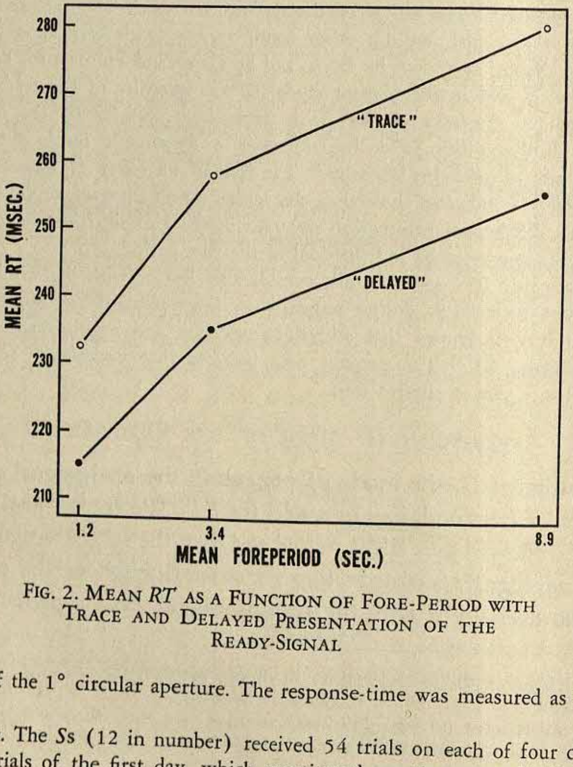

FIG. 2. MEAN *RT* AS A FUNCTION OF FORE-PERIOD WITH TRACE AND DELAYED PRESENTATION OF THE READY-SIGNAL

Procedure. The *S*s (12 in number) received 54 trials on each of four consecutive days. The trials of the first day, which constituted practice, were divided approximately equally among the various modes of presentation of the ready-signals (trace and delayed) and fore-periods (1.1, 3.1, and 8.1 sec.). On each of the three test-days, every *S* received six 'warm-up' trials followed by 48 trials containing a constrained random ordering with both modes of the ready-signals. All trials on any day were given with one of three fore-period ranges: 1.1 to 1.3, 3.1 to 3.7, and 8.1 to 9.7 sec. The fore-period ranges were counterbalanced among the *S*s for the three test-days. The intertrial interval was 25 sec. The experimental procedure and instructions to the *S*s were the same as in Experiment I.

Results. The data used for analysis consisted of the arithmetic means of the 20 response-times remaining after the two longest and shortest responses were eliminated in each mode of ready-signal and fore-period combination for each *S*. An analysis of variance of the results, graphically depicted in Fig. 2, indicates that the superiority of delayed presentation of of the ready-signal is statistically significant, (F, $1/11 = 32.2$, $p < 0.001$). The fore-period effect is also significant, (F, $2/22 = 14.9$, $p < 0.001$). The interaction between mode of ready-signal and fore-period is not, however, significant, (F, $2/22 = 1.31$, $p > 0.05$).

Discussion. The shorter reaction-times obtained with delayed presentation of the ready-signal indicate that the ready-signal serves more than a mere cuing function. If our argument that preparatory responses may become conditioned to the ready-signal in *RT* tasks is correct, then it appears that a delayed ready-signal is more effective than a trace ready-signal in conditioning these preparatory responses or in eliciting the already conditioned responses.

In the previous study, we interpreted the effect of the intensity of the ready-signal as an example of Hull's "stimulus-intensity dynamism." This interpretation may apply here as well. If the effectiveness of the ready-signal is determined by the intensity of the stimulus at, perhaps, the time of onset of the response-signal, or if the stimulus is terminated before the onset of the response-signal, the effectiveness is determined by the magnitude of the waning trace of the stimulus, then it may be expected that the delayed presentation of the ready-signal would be more effective than the presentation of the trace. If, furthermore, the stimulus-trace may be assumed to wane monotonically with time, then the difference in *RT* for the two modes of presentation of the ready-signal would be expected to increase with increasing fore-period. In fact, a small, though nonsignificant, trend, as predicted, was found. For mean fore-periods of 1.2, 3.4, and 8.9 sec., the difference trace and delayed presentation of the ready-signals were 17.2, 22.9, and 24.8 m.sec. A larger, and perhaps significant, effect would be expected if the range of the fore-period was increased. For example, Ellison, working with dogs, found that conditioning was equally effective with 8-sec. trace and delayed *CS-UCS* intervals, but that large differences emerged with 16-sec. intervals.[18]

Another possible interpretation of the present results relates to the accuracy of estimation of filled and unfilled intervals. If the durations of filled intervals are estimated more accurately than those of unfilled intervals, then it might be argued that this advantage reduced the temporal uncertainty for the delayed ready-signals. In attempting to explain the difference between trace and delayed conditioning, Fraisse noted, "'Filled' time has more reality than 'empty' time and is judged far more accurately, as if the physical duration of the stimulus added another cue to those provided by 'internal' processes."[19] Variability of the fore-period would be expected, however, to reduce the difference in the accuracy of judgment.

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[18] G. D. Ellison, Differential salivary conditioning to traces, *J. comp. physiol. Psychol.*, 57, 1964, 373-380.

[19] Paul Fraisse, *The Psychology of Time*, 1963, footnote p. 51.

CONCLUSIONS

The results of these studies, taken together, provide evidence for the value of the view that components of the *RT* and conditioning experiment are similar. At least the results of these experiments suggest that a number of variables, not traditionally considered to be relevant, may, in fact, be potent determinants of response-time. The investigation of *CS*-like properties of the ready-signal became immediately obvious when the model was made explicit. That the expected variations in *RT* were obtained, may have been fortuitous, but we think not. It would be inappropriate, however, to assume that these data confirm in any way, our assumption that preparatory responses become conditioned in the *RT* task. Direct measurement of these processes is possible and is needed. The results however, do underscore the homologies between the two tasks.

SUMMARY

Two experiments, derived from a conditioning model of the reaction-time task, examined *CS*-like properties of the *RT* ready-signal. In the first study, the intensity of the ready-signal was varied over a range of 60 db. with three different fore-periods. Both a within-*S*s and a between *S*s design was used. In the former, the reaction-times decreased significantly with an increase of the intensity of the ready-signal. The decrease was proportionate at each fore-period. The between-*S*s conditions were not significant.

In the second study, trace and delayed presentations of the ready-signals were compared, using a within-*S*s design. Delayed ready-signals yielded significantly shorter reaction-times than did trace-signals at all fore-periods. Taken together, the results of the two studies clearly indicate that the ready-signal in the reaction-time task serves more than a mere function of cuing.

CUE-CONSISTENCY AND CUE-UTILIZATION IN JUDGMENT

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Although the process of inference is acknowledged to be among the most important of cognitive activities, little is known about the manner in which human subjects (*S*s) combine information from multiple cues, each with a probabilistic relationship to a criterion, into a unitary judgment about that criterion. Numerous writers have emphasized the need for an analysis of the manner in which patterns or configurations of cues are utilized.[1] There is, however, little evidence to date indicating that the interpretation a person gives to a particular cue depends upon the values taken by accompanying cues.

In the area of impression-formation, the work that led Asch to reject an additive model of cue-combination in favor of a configural model has recently been criticized on both logical and empirical grounds.[2] In addition, the results of more than a dozen studies in the context of clinical or quasi-clinical diagnosis seem to indicate that people typically combine cues in a simple linear manner.[3] Nevertheless, some judgments deviate from the predictions of the linear model and, as Anderson has pointed out, investigators have not given sufficient attention to the meaning and significance of these deviations.[4]

The purpose of the present investigation is to examine the influence of one aspect of a pattern of cues, namely *cue-consistency,* upon the manner in which these cues are used by the judge. The situation to be studied is one in which a person is provided with several items of information pertaining to the personality and behavior of another individual and asked to

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* Received for publication July 21, 1965. This study was supported by Research Grant MH-04439 from the National Institutes of Mental Health, Public Health Service.

[1] See S. E. Asch, Forming impressions of personality, *J. abnorm. soc. Psychol.,* 41, 1946, 258-290; P. E. Meehl, The cognitive activity of the clinician, *Amer. Psychologist.,* 15, 1960, 19-27; P. J. Hoffman, The paramorphic representation of clinical judgment, *Psychol. Bull.,* 57, 1960, 116-131; K. R. Hammond, Carolyn Hursch, and F. J. Todd, Analyzing the components of clinical inference. *Psychol. Rev.,* 71, 1965, 438-456.

[2] Asch, *op. cit.,* 258-290; N. H. Anderson, Application of an additive model to impression formation, *Science,* 138, 1962, 817-818; D. N. Jackson, The measurement of perceived personality trait relationships, in Dorothy Willner (ed.), *Decisions, Values and Groups,* 2, 1962, 177-188.

[3] For a list of these studies see K. R. Hammond and D. A. Summers, Cognitive dependence on linear and non-linear cues, *Psychol. Rev.,* 72, 1965, 215-224.

[4] Anderson, *op. cit.,* 817.

make an inferential judgment of the individual with respect to some other attribute.

As Bruner, Shapiro, and Tagiuri have noted, one device by means of which partial information gets coded and organized in such a way as to enable the prediction of other information is a theory, in this case a *lay personality theory.*[5] These writers further state,

> What we infer about others is based upon a set of expectations or a 'coding system' about the nature of persons, derived in part from experience with behavior. Indeed, support for such expectations about others resides in the very consistency of behavior. It is a truism that the acts performed by people are not independent one from the other—a man who steals is likely to lie as well.[6]

The notion of internal consistency among a set of cues underlies the hypotheses of the present investigation. A set of cues can be considered *consistent* if the judge feels that the cues all agree in their implications for the attributes being judged. A set of cues is said to be *inconsistent* if the inferences that would be made from a subset of these cues would be contradicted by the implications of another subset of cues. Though consistency will be conceptualized as an all-or-none matter in the present paper, it is obvious that it is really a matter of degree.

It is hypothesized that, when a set of cues is consistent, each cue will be combined additively to determine a resultant judgment of which the judge is quite confident. An inconsistent set of cues, however, presents a problem for the judge. To reconcile apparently contradictory information, he must doubt either the reliability of the cues or the validity of his theory or both. It is possible that the judge will continue to employ the same additive weighting function to inconsistent cues that he uses for consistent cues. If so, his resultant inference would lie somewhere between the contradictory poles of the two inconsistent subsets. Alternatively, he might resolve the inconsistency by focusing his reliance upon some consistent subset of the inconsistent cues or by looking to previously ignored cues for confirmation or clarification.

Any change in the importance of a particular cue for a judge that occurs as a function of the amount of consistency among relevant cues is an example of patterned cue-utilization. A recent study by Anderson and Jacobson has demonstrated just such an effect.[7] These investigators found that judgments in an impression formation task were well represented by an additive model but that the weight given a particular cue was less for sets of cues in which that cue was inconsistent with the implications of other cues than for sets where it was consistent. The present study attempts to explore further the relationship between cue consistency and cue utilization.

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[5] J. S. Bruner, David Shapiro, and Renato Tagiuri, The meaning of traits in isolation and in combination, in Renato Tagiuri and Luigi Petrullo (eds.), *Person Perception and Interpersonal Behavior,* 1958, 277-288.

[6] Bruner, Shapiro, and Tagiuri, *op. cit.,* 277-278.

[7] N. H. Anderson and Ann Jacobson, Effect of stimulus inconsistency and discounting instructions in personality impression formation. *J. pers. soc. Psychol.,* 2, 1965, 531-539.

METHOD

The task. Our *S*s were asked to judge the intelligence of a person on the basis of quantified information from nine cues. These cues were: (1) High-school grade rating; (2) Status; (3) Percentage self support; (4) English effectiveness; (5) Responsibility; (6) Mother's eductation; (7) Study habits; (8) Emotional anxiety; and (9) Credit hours attempted. The cues were displayed in profile form in the order listed above with the levels of each cue presented as percentiles. The profiles did not represent real persons. In order, however, to enhance involvement in the task, the *S*s were told that the profiles were constructed from information decribing real University of Oregon freshmen and that they should try to adopt a judgmental attitude appropriate to a member of an admissions or scholarship committee. The *S*s were paid $1.25 per hour for their work. In addition, they were told that cash rewards would be given to the most 'accurate' judges. Judgments were made on a stanine scale with no restrictions placed on their distribution over categories. They also indicated their confidence in each judgment by assigning to it a number representing its probability of being correct.

The profiles were arranged in a loose leaf notebook. The *S*s were allowed as much time as they wished. On the average, they took about one and a half hours to judge the entire set of profiles.

Definition of consistency. Results from a previous study that used the same judgmental task indicated that a linear combination of the nine cues accounted for about 75% of the total variance in the average *S*s intelligence judgments for a sample of profiles in which the cues were uncorrelated.[8] Furthermore, the *S*s were found to rely primarily upon just two of the nine cues: (1) High-school grade rating (*HSR*), and (4) English effectiveness (*EE*). On the average, the linear use of these two cues accounted for about 80% of the total linearly predictable variation and about 60% of the total variation in a person's judgments. Though some persons relied on *HSR* more than on *EE* and vice versa, the judgments of more than 85% of the *S*s were correlated significantly and positively with both of these cues. This information led to the consistency of any given profile being defined, for the purposes of the present study, as a function of the absolute percentile difference between *HSR* and *EE*. This definition of consistency, dictated by considerations of convenience, is admittedly incomplete. For example, a large percentile-difference should appear more inconsistent to a person who weighs *HSR* and *EE* substantially and equally than it does to a person who relies more on one cue than on the other or uses other cues as well. Furthermore, possible inconsistency due to other cues is ignored. Nevertheless, should systematic effects of consistency emerge despite these inadequacies, future studies can pursue them using more sophisticated conceptualizations.

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[8] F. W. Clark, Cue-extremeness and cue-utilization in clinical judgment, Unpublished Master's thesis, University of Oregon, 1965.

Profile sets. Every *S* judged 75 profiles. Afterwards, certain subsets of these judgments were selected by *E* for separate analysis. Fifteen profiles, all exhibiting a percentile-difference between *HSR* and *EE* that exceeded 40, were singled out as inconsistent profiles. Another 15 profiles with percentile-differences between 10 and 20 inclusive were selected to represent a relatively consistent group of profiles. Profiles in which *HSR* and *EE* were identical were not included in the consistent set because this would have precluded the possibility of determining whether *S* was weighting these two cues differentially when judging these profiles. The correlation between *HSR* and *EE* over the 15 inconsistent profiles was made less than —1.00 for this same reason. The remaining 45 profiles of the total set of 75 provided the context within which the consistent and inconsistent profiles were judged. In general, *HSR* and *EE* were consistent for the context profiles. Judgments for these profiles were not analyzed. The first 15 profiles seen by the judge were from the context set.

TABLE I

CHARACTERISITICS OF CONSISTENT, INCONSISTENT, AND CONTEXT PROFILES

Profile Set	Number of profiles	AD*	$r_{HSR,EE}$	*HSR*		*EE*	
				Mean	SD	Mean	SD
Consistent	15	16.0	+.80	4.9	2.6	5.0	2.8
Inconsistent	15	45.7	−.50	4.7	2.6	4.6	2.8
Context	45	21.8	+.53	5.0	2.9	5.0	3.0

* Average absolute percentile difference between *HSR* and *EE*.

Consistent, inconsistent, and context profiles were randomly interspersed within the remaining 60 profiles.

The profiles were so constructed that the means and variances of each of the nine cues were approximately equal for the consistent and inconsistent subsets. The intercorrelations among all cues were low with the exception of those between *HSR* and *EE*. Pertinent information about the profile sets is presented in Table I.

Subjects. The *Ss* were 92 undergraduates from the University of Oregon.

RESULTS

The linear model. The data were analyzed within the framework of the linear model.[9] This model depicts each judgment as a simple weighted sum of the values of the cues in the profile. The major aim of the analysis is to determine the weighting coefficient for each cue such that the multiple correlation (R_J) between the weighted composites and the judge's responses over a set of profiles is maximized. If R_J is high, a transformation of the weighting coefficients can be viewed as descriptive of the degree to which the various cues are used by the judge. This model assumes that the manner in which a judge weights a cue does not depend upon the values of the other cues in the profile. In the present study, it was assumed that this model would accurately predict judgments that were made from profile sets which were homogeneous with respect to the consistency between *HSR* and *EE*.

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[9] Hoffman, *op. cit.*, 119; Carolyn Hursch, K. R. Hammond, and J. L. Hursch, Some methodological considerations in multiple-cue probability studies, *Psychol. Rev.*, 71, 1964, 42-60.

Two separate multiple regression analyses were performed on the data from each judge; one for the set of consistent profiles and one for the inconsistent profiles. Hoffman's index of relative weight ($W_i = \beta_i \cdot r_i / R^2_J$) was calculated to assess the extent to which the judge was using the i^{th} cue over the set of consistent or inconsistent profiles.[10] Since $R^2_J = \Sigma_i \beta_i \cdot r_i$, Hoffman contends that W provides a method for describing the relative contributions of each of the cues such that a simple sum of them accounts entirely for the linearly predictable variance in the judgments. Hoffman's relative weights sum to 1.00 over the set of cues. The index of relative weight used in the present investigation (RW_i) was normalized to sum to R^2_J. Thus, in this study, RW_i represents the proportion of the total variance in the judgments that can be accounted for by the linear use of the i^{th} cue.

TABLE II

VALUES OF JUDGMENT-INDICES AVERAGED OVER *S*s FOR CONSISTENT AND INCONSISTENT PROFILES

Index	Profiles: Consistent (+)	Profiles: Inconsistent (−)	% of *S*s for whom (+>−)*	*P*
R_J^2	.94	.88	77	<.001
RW_{HSR}	.37	.24	73	<.001
RW_{EE}	.29	.23	63	<.02
SUM	.66	.47	83	<.001
DIFF	.29	.41	30	<.001
LARG	.46	.41	59	>.05
SMAL	.19	.06	87	<.001
VAR_J	4.08	2.65	79	<.001
CONF	.58	.54	78	<.001

* Percentage of *S*s for whom the value of the judgment index calculated from consistent profiles exceeded the value calculated from inconsistent profiles.

Dependent variables. The discussion of the results which follows will center around the effects of profile-consistency on the following variables, most of which are derived from the multiple regression analyses.

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[10] Hoffman, *op. cit.*, 121.

(a) *VAR_J;* the variance of *S*'s judgments over a set of 15 profiles.
(b) R^2_J; the proportion of VAR_J that can be accounted for by the linear model.
(c) RW_{HSR} and RW_{EE}*;* the proportion of VAR_J that can be accounted for by the linear use of cues *HSR* and *EE* respectively.
(d) *SUM;* the sum of RW_{HSR} and RW_{EE}. This is the proportion of VAR_J accounted for by the linear use of both critical cues.
(e) *LARG;* the larger of the values for RW_{HSR} and RW_{EE}.
(f) *SMAL;* the smaller of the values for RW_{HSR} and RW_{EE}.
(g) $DIFF = R^2_J - SUM$; this index gives the sum of the relative weights for the seven other cues.
(h) *CONF;* the mean of confidence ratings over the set of 15 profiles.

Group means. Two values of every dependent variable described above were calculated for each *S*; one for the consistent and one for the inconsistent profile sets. These indices were then averaged over the *S*s. The results are presented in Table II. The sign-test was used to evaluate the statistical significance of differences between consistent and inconsistent profile sets.[11] The principal findings are described below for each dependent variable.

R^2_J. The linear model predicted the *Ss*' judgments quite well within both consistent and inconsistent profile sets. Judgments, however, were slightly more linear for the consistent profiles.

RW_{HSR}, RW_{EE} and *SUM*. Both *HSR* and *EE* were used to a greater extent when they agreed with one another than when they disagreed.

DIFF. The sum of the relative weights for the seven additional cues was greater for inconsistent than for consistent profiles. The value of this index is sensitive to small and rather inconsequential relative weights as well as large ones. When one looks only at the number of relatively large relative weights given to these seven cues, the effect of consistency remains substantial. For example, 46% of the *S*s gave one or more of these seven cues a relative weight greater than 0.20 over the set of inconsistent profiles while only 22% did so for the sets of consistent profiles. A test of significance for correlated proportions showed this difference to be significant at $P < 0.01$.[12]

LARG. Consistency had little effect upon the average relative weight given to the more important of the two critical cues.

SMAL. The mean relative weight for the lesser of the two critical cues was lower for inconsistent profiles than for consistent ones.

The results for the indices labeled *SUM*, *DIFF*, *LARG*, and *SMAL*, indicate that there was a tendency for the *S*s to rely upon both *HSR* and *EE* when these cues agreed with one another. When *HSR* and *EE* were contradictory, *S* tended to use only one of these two cues. The lesser cue was excluded. Also, there was greater use of other cues when *HSR* and *EE* were inconsistent.

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[11] S. Siegel, *Nonparametric Statistics for the Behavioral Sciences*, 1956, 68-75.
[12] Quinn, McNemar, *Psychological Statistics*, 1962, 228.

VAR_J. Judgments were considerably less variable for inconsistent profiles than for consistent ones.

CONF. The *S*s were less sure of judgments made for inconsistent profiles. Although the magnitude of the confidence decrease seems rather small, it should be evaluated with the knowledge that a person's confidence varied little within a set of 15 consistent or inconsistent profiles. The average standard deviation about the mean confidence rating was only 0.1.

DISCUSSION

It is apparent that cue-consistency does influence the manner in which a person utilizes fallible information when making a judgment. Since consistency is a feature of all cue-configurations, the type of pattern-sensitivity exhibited by the *S*s in the present study should generalize to other judgment tasks as well. Patterned cue-utilization is essentially incompatible with the linear model. It is interesting to note, however, that the degree of linearity within judgmental sets that were relatively homogeneous with respect to cue-consistency was extremely high; greater, in fact, than that reported in any of the previous studies that used similar tasks but did not take consistency into account.[13]

Perhaps the most surprising result was the fact that, although a substantial number of persons relied upon both *HSR* and *EE* when these cues agreed with one another, only a very few people continued to use both when they were contradictory. Instead, the relative magnitude of their judgments for inconsistent profiles was primarily determined by just one of these two cues. This disregard for one of the inconsistent cues is congruent with the findings of Anderson and Jacobson.[14] In the Anderson-Jacobson study, however, cues were discounted because they disagreed with the majority of the other cues. In the present study, the cue that was consistently discounted could not have been generally in less accord with the other cues because of the manner in which the profiles were constructed. Thus, in this study, reliance on one cue and discounting of the other must have been due to the judge's implicit feelings about the relative validity of the two cues for predicting the criterion.

Other data also point to an interaction between a cue's intrinsic validity and the manner in which consistency affects the use of that cue. For example, the value of *HSR* is derived from a great deal of behavior during the course of several years, while that of *EE* is the product of just a few hours' work. If the *S*s thought inconsistency was caused by the unreliability of one of these cues, it seems likely that they would have distrusted the *EE* score more. If so, one might expect that inconsistency would have reduced the use of *EE* more than it reduced the use of *HSR*. The fact that this did not happen suggests that considerations of a cue's intrinsic validity take precedence over considerations of reliability in determining whether that cue will be discounted or used in the event of inconsistency.

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[13] For example, see R. E. Knox and P. J. Hoffman, Effects of variation of profile format on intelligence and sociability judgments, *J. appl. Psychol.*, 46, 1962, 14-20; Stuart Oskamp, How clinicians make judgments from the MMPI: An empirical study, *Amer. Psychologist*, 17, 1962, 316; Hammond, Hursch, and Todd, *op. cit.*, 441; J. Newton, Judgment and feedback in a quasi-clinical situation, *J. pers. soc. Psychol.*, 1, 1965, 336-342.

[14] Anderson and Jacobson, *op. cit.*, 517.

While it is evident that other cues were called upon to help determine the judgment when *HSR* and *EE* were contradictory, the nature of this process remains unclear. One hypothesis for future study is that a cue regarded by *S* as having relatively low validity will be used in conjunction with a more valid cue only when it is consistent with that cue.

In conclusion, it appears that the simple linear model should be amended to include a term that is sensitive to the level of incompatibility among cues pertaining to a particular stimulus. Further theoretical and empirical work is necessary, however, before the precise nature of this complication can be adequately formulated.

SUMMARY

*S*s were asked to judge the intelligence of a person on the basis of nine cues presented in profile form with the levels of each cue displayed as percentiles. Previous research with this task had determined that judgments of intelligence were based primarily on a linear combination of only two of these cues—High-school grade rating (*HSR*) and English effectiveness (*EE*). The consistency of a profile was defined in terms of the degree of agreement between these two cues. Each *S* judged 15 'consistent' profiles in which the percentile-difference between *HSR* and *EE* was rather small and 15 'inconsistent' profiles in which the percentile-difference was quite large. Consistent and inconsistent sets of profiles were analyzed separately by means of a multiple correlation model. The results indicated that an *S*'s judgments were dependent upon both *HSR* and *EE* when these cues agreed with one another. When these cues, however, had contradictory implications, *S* relied upon only one of them. The other was consistently excluded from consideration. Cues other than *HSR* and *EE* were used to a greater extent when *HSR* and *EE* were in disagreement. These data call for a modification of the commonly accepted 'linear model' of information combination.

RETINAL ANOXIA AND THE LOCUS OF THE AFTER-EFFECT OF MOTION

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Lately there has been renewed interest in the physiological origin of the after-effect of motion.[1] This interest receives its impetus from two sources: the possible application of such knowledge as a diagnostic tool;[2] and the existence of numerous mutually contradictory interpretations in the literature.[3] The most popular technique for eliciting the after-effect of motion in these studies has been the familiar spiral after-effect. Whether this illusion owes its appearance to a primarily retinal process, a central process, or some combination of the two is not known. Superficially, the solution to the problem of retinal or central primacy appears simple enough, but unfortunately no one of the many ingenious studies designed to answer the question has produced unequivocal results. Indeed, the various attempts to solve this physiological jig-saw puzzle make a most curious chapter in the history of perception.

One approach toward a solution to the problem has been the interocular transfer study. Transfer is said to have been demonstrated when one eye is stimulated and the after-effect is observed with the unstimulated eye. The demonstration of "transferred" after-effect has been taken by some authors as support for a central process.[4] Day, however, has shown that this demonstration does not necessarily rule out the possibility of a retinal contribution.[5] As Day points out, the complexity of the

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* Received for publication, July 23, 1965.

[1] S. L. Freud, The physiological locus of the spiral after-effect, this JOURNAL, 77, 1964, 422-428; M. J. Pickersgill, After-effect of movement in the stimulated and opposite eyes during and after pressure blinding, *Nature*, 202, 1964, 833-834; M. J. Pickersgill and M. A. Jeeves, The origin of the after-effect of movement, *Quart. J. exp. Psychol.*, 16, 1964, 90-103.

[2] A. C. Price and H. L. Deabler, Diagnosis of organicity by means of the spiral after-effect, *J. consult. Psychol.*, 19, 1955, 299-302; T. R. Scott, R. A. Bragg, and R. G. Smarr, Brain damage diagnosis with the MMG, *ibid.*, 27, 1963, 45-53.

[3] W. S. Hunter, Retinal factors in visual after-movement, *Psychol. Rev.*, 22, 1915, 479-489; Ragnar Granit, On inhibition of the after-effect of seen movement, *Brit. J. Psychol.*, 19, 1928, 147-157; H. C. Holland, Some determinants of seen after-movement in the Archimedes spiral, *Acta Psychol.*, 14, 1958, 215-222; Wolfgang Köhler and Hans Wallach, Figural after-effect: An investigation of visual processes, *Proc. Amer. Phil. Soc.*, 88, 1944, 269-357.

[4] Köhler and Wallach, *op. cit.*, 271, 340.

[5] R. H. Day, On interocular transfer and the central origin of visual after-effects, this JOURNAL, 71, 1958, 784-790.

problem has its basis in the fact that one hemiretina from each eye projects onto the same hemisphere of the visual cortex. The fact of coincidence of the two monocular visual fields results in the possibility that continuing retinal processes in one eye will affect what is seen by the other.

Freud has studied the effect of stimulating the hemiretinas of the two eyes independently.[6] The results of this procedure are equally ambiguous. He found that stimulation of the hemiretina of one eye could produce an after-effect observed by the homonymous hemiretina of the opposite eye. No such 'transfer,' however, could be demonstrated when the test-stimulus was viewed by the heteronymous hemiretina. These results can be perfectly well accounted for by either a central or a retinal theory.

Young in a very ingenious study introduced different levels of illumination to the previously stimulated eye while measuring the duration of the after-effect in the opposite eye.[7] He found a significant difference and concluded that this difference would be found *only* if the after-effect has a retinal component. The processes, however, which Young thinks of as being in the retina and as being affected by varying the illumination might equally well be in the brain.

Terwilliger has recently suggested that the problem may be insoluble unless a way can be found to block temporarily the transmission of neural impulses between the retina and the cortex.[8] For as long as the retina sends information to the brain, the roles played by the cortex and the retina remain difficult if not impossible to separate.

Perhaps a solution can come from the fact that pressure on the eye results in a diminution of the blood supply and subsequent retinal anoxia.[9] While ophthalmoscopic examination of the pressure-blind eye does not reveal complete vascular occlusion, sufficient anoxia is obtained by this method to produce total blindness, indicating that retinal neural processes are suppressed considerably if not totally.

Pickersgill and Jeeves have recently made use of pressure blindness in a study of spiral after-effect and have presented two results which, they feel, are suggestive of a retinal process.[10] In one experiment, they stimulated one eye with a rotating spiral and applied pressure to the eye until total blindness resulted. Blindness was maintained for periods up to 35 sec. Upon release of pressure, the after-effect was observed to persist for a long enough time to suggest that some form of retinal 'storage' may have taken place. In another experiment, after presentation of the spiral, the after-effect was observed in the unstimulated eye. In seven out of 12 *Ss*, blinding the stimulated eye was then observed to interrupt the after-effect which subsequently continued upon release of pressure. Pickersgill summarized this result by saying that the after-effect seen with the unstimulated eye appeared to be dependent upon the state of the retina of the stimulated eye. These results would seem to suggest a retinal component.

The measure of the after-effect used by Pickersgill and Jeeves was the reported duration of apparent movement. There are some reasons to doubt the suitability of this measure. After stimulation, the after-effect slowly decreases. An *S* instructed to tell the *E* when there is no longer any after-effect is in a position similar to that of an *S* in an absolute threshold experiment in which the stimulus is never increased from below the threshold, but is always decreased from above threshold values. Since the after-effect decreases exponentially, there really is no precisely identifiable time when it disappears altogether. This creates a problem for *S* who is thereby made peculiarly susceptible to anchoring effects. In a study of the duration of the after-effect of motion in this laboratory, it was found that the duration of *S*'s first after-effect in the experiment had a marked influence on his report of subsequent conditions. If the experiment began with a condition producing prolonged after-effect, *S* tended to give prolonged reports of duration in all subsequent conditions. The Ss whose first experimental condition produced only slight after-effect, on the other hand, tended to report shorter after-effects even under conditions which are known to produce much more extended after-effects in unbiased *S*s. There is still another reason to doubt the validity of findings based on the measure of duration. Pressure blinding is an uncomfortable experimental procedure for *S*. Under any condition in which pressure can be released as soon as the *S* reports cessation of after-effect, *S* is likely to be motivated to find that his after-effect has ceased. In addition, whenever after-effects are slight, *S*'s beliefs as to what he should see are very likely to affect what he sees.[11]

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[6] Freud, *op. cit.*, 422-428.

[7] Young, *op. cit.*, 74.

[8] R. F. Terwilliger, A reply to Scott, *Percept. mot. Skills*, 18, 1964, 640.

[9] K. J. W. Craik and M. D. Vernon, The nature of dark-adaptation, *Brit. J. Psychol.*, 32, 1941, 62-81.

[10] Pickersgill, *op. cit.*, 834.

The present study was designed in an attempt to circumvent the objections presented above. Specifically the purpose was to make use of the facts of pressure blinding in deciding between retinal and cortical contributions in such a way that motivational factors, beliefs of the *S*, and anchoring effects could not operate.

Apparatus. The apparatus used in this study to measure the rate of spiral after-effect is similar to that described in a previous publication.[12] A two-field mirror tachistoscope and electronic timer were used. The eliciting stimulus was a black single throw spiral, drawn counterclockwise outward on a white disk 19 cm. in radius. The black and white bands were of equal widths, and the formula for the spiral was $\rho = 0.353\ \Theta$, with ρ in centimeters and Θ in radians. The spiral was rotated counterclockwise at a stroboscopically controlled speed of 160 r.p.m. A central fixation-point was provided by a 6.3-v. bulb set in the center of the spiral behind an aperture which was 3 mm. in diameter. The light was powered inductively so that flicker would not occur as the spiral rotated. The spiral was placed in one arm of the tachistoscope and was illuminated by 8 DC-operated fluorescent lights and two 150-w. incandescent bulbs. This unusually bright illumination was made necessary by the method used to equate the brightness of the visual fields. A TV picture tube (ALP4-A) and deflection yoke were mounted in the other arm of the tachistoscope. The test-stimulus was a circular trace about 2-mm. wide. The circle appeared with an initial radius of 5 cm. and then expanded, contracted, or remained stationary. The rates at which the circle expanded or contracted were controlled by means of an electronic circuit which could be adjusted to produce any desired rate of change of circle size. *E* could select any one of several precalibrated rates of change of circle size by adjusting a step switch. The particular rates of change of radius were calibrated in 1 mm./sec. steps.

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[11] P. London and J. H. Bryan, The influence of instructions on spiral after-effect, *Amer. Psychologist,* 13, 1958, 335.

[12] A 3-page description and one circuit diagram have been deposited with University Microfilms, Inc., Ann Arbor, Michigan. Order Special Film S-294, remitting $2.75 for microfilm or positive copies.

Electrically controlled shutters were placed in front of *S*'s eyes. To avoid differential dark-adaptation of the two eyes during certain conditions, two layers of translucent bond paper were placed over one aperture of each shutter and an adjustable polaroid lens was placed over the other aperture of each shutter. The polaroid lenses were so adjusted by use of a Gossen Lunasix light meter that the amount of light passing through the translucent paper was equal to the amount passing through the lenses. The two shutters were controlled in such a way that wherever one eye was covered by translucent paper, the opposite eye looked through one of the polaroid lenses. The light scattering produced by the translucent paper was sufficient to obliterate completely any perception of the stimuli as optical figures. The necessity for compensating the two eyes in this way accounts for the unusually bright levels of illumination used. Under certain control conditions, it was necessary to present a stationary stimulus in place of the rotating spiral. A stationary spiral was rejected because the use of such a figure could induce a clear after-image. Therefore, a gray paper shield matched in reflectivity to the spiral disk was inserted in front of the spiral, completely covering it.

A manipulandum box containing a potentiometer with a large control knob was connected to the electronic circle generator in such a way that the size of the circle could be linearly controlled by turning the knob. The electronic circle generator was so constructed that it was also possible to superimpose controlled rates of expansion or contraction on the effect of the *S*'s adjustments of the knob. A second potentiometer was ganged to the other just described and connected through a bridge circuit to a DC amplifier in such a way that *S*'s adjustments of the knob were recorded on an oscillograph.

A dermographia stimulator was adapted for use as an eye-pressor. The point of the stimulator was removed and a hard rubber hemispherical tip of 0.5 cm. radius of curvature was attached in its place. A standard pressure of 200 gm. was used by all *Ss*.

Subjects. Eleven *Ss* ranging in age from 15 to 36 yr. were used in this study.

Procedure. Prior to the experiment, each *S* was trained to apply 200 gm. of pressure to each upper eyelid in order to obtain pressure-blindness. Also each *S* was given a trial period in which he learned to adjust the knob which regulated the movement of the test-circle. Following the practice session, each *S* was given a series of 30 random trials in which the test-circle changed size at various rates. His objective was to hold the circle stationary for each trial. A trial consisted of three presentations. The first presentation of each trial was given to familiarize the S with the new rate which he then attempted to neutralize on the second and third presentations. His response to the first presentation was not scored. His responses were recorded on the oscillograph and were correlated with the actual change in the test circle. No *S* was used in this study unless this correlation was 0.89 or more. The correlation ranged from 0.89 to 0.97 with the median at 0.92. *S* was seated in a dark room at an optical distance of 90 cm. from the stimuli. The rate of after-effect was measured under seven conditions. A condition was determined by the eye stimulated, the eye tested, the presence or absence of pressure on an eye, and the duration of the pressure applied. In each condition, one eye was stimulated for 30 sec., followed by a period of darkness lasting 17 sec. This was the maximal time required by any *S* to achieve total blindness and to regain sight upon release of pressure. Under conditions in which it was desired that sight return before presentation of the test-stimulus, the *S* was instructed to release pressure after 12 sec. In this way, 5 sec. were allowed for the return of retinal circulation before the presentation of the test-stimulus (the 'release' conditions listed below). Under some conditions, pressure was maintained until the end of the test-period (the 'hold' conditions listed below). The dark period was followed by a 5-sec. presentation of the circular test-stimulus. The seven conditions used are listed below:

(A) *Simple monocular:* right eye stimulated, right eye tested, no blinding.
(B) *Simple transfer:* left eye stimulated, right eye tested, no blinding.
(C) *Blinded monocular:* right eye stimulated, right eye blinded and released, right eye tested.
(D) *Blinded transfer (release):* left eye stimulated, left eye blinded and released, right eye tested.
(E) *Blinded transfer (hold):* left eye stimulated, left eye blinded and held throughout the test period, right eye tested.
(F) *'Placebo' (hold):* right eye stimulated, left eye blinded and held throughout the test period, right eye tested.
(G) *'Placebo' (release:)* right eye stimulated, left eye blinded and released, right eye tested.

The last two conditions listed above are 'placebo' conditions in the sense that although blinding is involved, the blinded eye is neither stimulated nor tested and presumably plays no role in the perceptual process under study. This would appear to be an essential control for any study of this type. The initial order of presentation of the conditions was selected from a table of random numbers. This order was DACGBEF. The conditions were presented in reverse order to alternate *S*s. Each of the seven conditions was presented under six sub-conditions. For three of the six sub-conditions, *S* viewed the rotating spiral; for the other three, he viewed the gray shield, thus controlling for any possible artifact of the measurement technique. To make sure that the *S* could not know whether he was responding to an after-effect or to real motion, the eliciting stimulus was followed by one of three objective motions of the circle: 2, 0, and —2 mm./sec. change in radius. These three objective motions were presented in a random order. As in the training procedure, each trial consisted of three identical presentations. The first presentation was not scored.

Results. Table I summarizes the results of the experiment. The entries in the table are expressed in minutes of arc per sec. A preliminary analysis of the effects of order yielded a non-significant result, and orders were pooled for the subsequent analysis of variance. The effect associated with the seven conditions was highly significant ($p < 0.0005$), but interpretation depends on subsequent parts of the analysis. The circles-effect was significant ($p < 0.0005$), thus showing that the *S*s' responses were a function of the movement of the test-stimulus. The spirals effect was also highly significant ($p < 0.0005$), demonstrating that the *S*s did observe an after-effect. The interaction of conditions by circles was not significant, indicating that the *S*s' ability to respond differentially to the circles was not ap-

TABLE I

Average Rate in Minutes of Arc per Second of Test-Stimulus Adjustment for All Conditions

Condition	Turning spiral				Gray shield				Difference between stationary and turning conditions (After-effect rate)
	Contract	Circle			Contract	Circle			
		Not Moving	Expanding	Mean		Not Moving	Expanding	Mean	
Simple monocular	5.8	15.5	22.2	14.5	−7.1	1.3	1.2	1.2	13.3
Simple transfer	0.0	7.1	15.5	7.5	−9.8	0.4	8.9	−0.2	7.7
Blinded monocular	2.2	12.9	22.6	12.6	−6.2	0.9	11.1	1.9	10.7
Blinded transfer (release)	1.3	11.1	17.8	10.1	−8.9	1.3	9.8	0.7	9.4
Blinded transfer (hold)	1.3	8.4	14.2	8.0	−5.3	0.0	4.9	−0.1	8.1
Placebo (release)	1.3	9.3	19.1	9.9	−4.4	0.4	7.5	1.2	8.7
Placebo (hold)	1.8	8.0	15.5	8.4	−9.8	0.0	8.0	−0.6	9.0
Means	2.0	10.3	18.1		−7.4	0.6	8.5		
Differences: A		8.3	7.8			8.0	7.9		
B	9.4	9.7	9.6						

preciably affected by the different conditions. This demonstrates that the discomfort and inconvenience involved in applying pressure to the eye under some conditions of the experiment did *not* affect the *S*s' ability to perform the required discriminations. The conditions by spirals interaction was significant ($p < 0.025$), suggesting that the amount of after-effect was different in different conditions. This interpretation is based, however, on the assumption that the control conditions are equivalent. A separate analysis of the control conditions failed to cast significant doubt on their equivalence, ($F = 1.4$, 6 and 60 *df*, $0.10 < p < 0.25$) so it must be concluded that transfer or blinding or both had some effect on the after-effect. The interaction of circles by spirals was insignificant, ($F < 1$). Thus there is no reason to doubt the assumption of algebraic additivity between the after-effect and objective motion of the test-stimulus within the range of test-motions and after-effects involved.[13] Indeed, inspection of Table I reveals a striking consistency among the differences shown at the bottom of the table. These differences show that varying the objective motion has approximately the same effect whether or not after-effect is present (see differences in Row A). Also, the same amount of after-effect is observed whether the test-stimulus is objectively expanding, stationary, or contracting (see differences in Row B). A separate analysis was performed using only data from the following four conditions: simple monocular, simple transfer, blinded monocular, and blinded transfer. This analysis was performed in an attempt to separate the effects of transfer from the effects of blinding. The four conditions were arranged in a 2 × 2 contingency table with the variables *transfer* and *blinding* as the two dimensions of the table. The transfer-effect was highly significant ($p < 0.0005$) indicating that transferred after-effect is less than monocular after-effect. Blinding appeared to have no significant effect in this analysis ($F < 1$). Another analysis of variance comparing simple monocular with the two 'placebo'-conditions showed a significant difference among these conditions ($F = 5.97$, 2 and 20 *df*, $p < 0.01$). It thus appears that blinding a non-participating eye significantly lessens the magnitude of after-effect in the opposite eye. A comparison of blinded monocular and placebo release conditions, however, results in an insignificant F ($F = 2.49$, 1 and 10 *df*, $0.10 < p < 0.25$). It would appear that it makes no difference whether the eye blinded is or is not involved in the eliciting or testing situation.

Discussion. Pickersgill reports that for 6 of her *S*s, retinal anoxia induced after monocular stimulation and released prior to testing for after-effect in the same eye, resulted in complete absence of after-effect.[14] Under the same conditions, 5 *Ss* reported an increase in after-effect. In the present study neither an absence of after-effect nor an increase in it was observed in any of the 11 *Ss* tested. Rather, blinding the stimulated eye under monocular conditions resulted in a reduction of the after-effect, although this effect was not statistically significant. It would appear that the discrepancy between the two studies can be accounted for on the basis of the methods of measurement chosen. It would also appear likely that the present study more accurately reflects the effects of retinal anoxia.

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[13] See also T. R. Scott, A. E. Jordan, and D. A. Powell, Does visual after-effect of motion add algebraically to objective motion of the test-stimulus?, *J. exp. Psychol.*, 66, 1963, 500-505.

While results from the 'placebo' leave little doubt that pressure has an effect, no retinal theory would have predicted the diminution of the after-effect resulting from applying pressure to the non-participating eye. Under the monocular conditions of this study, blinding did decrease the after-effect but it did not appear to matter whether the blinded eye participated either in the eliciting or the testing situation. There appear to be only two consistent explanations for the obtained effects of blinding. The first is that a blinded eye sends impulses to the brain which interfere with processes there. This could take place during the onset of blinding or during its release. The second is that the lack of normal background impulses from the blinded eye disrupts brain processes concerned with the after-effect. In either case, the results of the present study indicate that if there is a retinal component in the after-effect of motion, it can account for only a very small fraction of the after-effect.

SUMMARY

The purpose of this study was to test whether a retinal component exists in visual after-effects of motion. An attempt was made to remove any influence of the stimulated retina by applying pressure to the eye, thereby temporarily interrupting retinal blood supply. The rate of the after-effect was measured by having *S* adjust the size of the test-stimulus after viewing a rotating spiral. The results showed that 'transferred' after-effect obtained by stimulating one eye and testing the opposite was significantly less than the monocular after-effect. While pressure-blinding had some significant effects on the after-effect rate under some of the conditions of the experiment, the pattern of these effects was not consistent with the hypothesis of a retinal locus for the processes involved in the after-effect of motion. It was concluded that if such retinal processes exist, they can account for only a very small fraction of the after-effect.

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[14] Pickersgill, *op. cit.*, 834.

GAZE-CONTINGENT ADAPTATION TO PRISMATIC SPECTACLES

By HERBERT L. PICK, JR., University of Minnesota, and
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Ivo Kohler reported remarkable after-effects resulting from the prolonged wearing of prismatic spectacles; when the spectacles were removed the apparent size and shape of objects varied with direction of view.[1] How does a constant retinal image come to evoke different impressions of size and shape? The result obviously is connected with the distortion produced by prisms. A prism displaces light as a function of the angle of incidence and the thickness of glass traversed. Thus, a person wearing wedge-prisms mounted in spectacle-frames (with bases oriented in the same direction) notes a variety of deformations, both when the head is moved while fixating a target, and when the eyes are moved with the head stationary. In general, the after-effects reported by Kohler are the same distortions as those produced by prisms, but in the opposite direction.

Kohler's observations of these gaze-contingent after-effects were based upon two *S*s, himself and Dr. T. Erisman. Pilot studies by the present authors indicate that *S*s exposed to prismatic distortion for as short as 20 days experience such after-effects but that their measurement is difficult. The purpose of the present study was to develop appropriate techniques for verifying the existence of no after-effects and for quantifying them.[2]

Problem. The present report focuses on the measurement of adaptation to two deformations produced by prisms. The first distortion is contingent upon change in vertical position of the eyes in the head. Specifically, horizontal lines above and below eye-level appear to converge towards the apex of the prism. This distortion is alternatively manifest in the apparent 'shearing' or tilting of a fixated horizontal target as the head is nodded up and down. The second distortion is contingent upon change in lateral position of eyes in the head. Specifically, identical targets viewed through the base and apex of the prism have different apparent widths (the base-target being compressed and the apex expanded). This distortion is alternatively manifest in 'stretching' and 'compression' of a fixated target as the head is turned right and left. Adaptation to these two types of distortion was measured separately.

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[1] Ivo Kohler, The formation and transformation of the perceptual world. *Psychological Issues,* 3, 1964, 1-173.

[2] Some of these data were summarized in H. L. Pick, Jr. and J. C. Hay, Adaptation to prismatic distortion, *Psychonom. Sci.,* 1, 1964, 199-200.

Method: (1) Measurement of adaptation. To reduce the possibility that positive results simply would reflect a change in *S*'s response-tendencies or expectations, adaptation was measured by a variety of procedures. They all shared the feature that *S* was required to report when an adjustable viewing situation appeared *not* to deform with change in gaze, that is, to find a visual null. The procedures differed in three principal ways: *(a)* *S* was tested with or without his own spectacles; *(b)* the viewing situation was adjusted by manipulation of the target or by manipulation of the optical input by means of a compensating prism; and *(c)* the target was familiar or unfamiliar. The logic of these tests is outlined below.

(a) Prism- and target-adjustment. An *S* may be asked to find a visual null by manipulating the optical channel with a compensating prism. Thus he might so adjust a variable prism that no distortion occurs as he moves his head from left to right while fixating a target. If *S* is asked to do this before prism-exposure, he should set a variable prism close to zero-strength. After exposure, if he has adapted completely to a distortion and is asked to find a visual null, he should set the variable prism close to the strength of his own prisms. Values intermediate between zero and his own prism-strength signify intermediate degrees of adaptation.

Alternatively, *S* may be presented with an adjustable target, viewed with gaze in one direction, and asked to match it with a standard target viewed in a different direction. The two techniques are equivalent optically, although the adjustment of the target influences only a part of the visual field, while the adjustment of the prism applies to all of it. Both tests should give the same quantitative results if adaptation is a matter of direction of the gaze controlling the apparent form of the target.

(b) S tested with and without his own spectacles. When *S* is tested while wearing his own spectacles and is asked to find a visual null, he must introduce an opposite distortion to counteract that produced by his own spectacles. If the test involves prism-adjustment, he must introduce prism-power opposite in direction to that of his own prism. For example, if he is tested while wearing his own prisms, but before any appreciable exposure, he should introduce prism-power of approximately the same strength, but in the opposite or compensatory direction. If he has adapted completely to his own prisms, he should set an adjustable prism close to zero strength during the test of the visual-null. Again, intermediate values should represent intermediate degrees of adaptation. A similar argument applies to the adjustment of the target. If *S* is wearing his own spectacles while making null judgments, the more he has adapted the less counteracting target-distortion will have to be introduced.

When *S* is tested without his own prisms and before appreciable exposure, little or no distortion of the target should be necessary to obtain a visual null. If, however, he has adapted completely to his own prisms, he should require, to obtain a visual null, approximately the same amount of target-distortion as produced by his own prism-spectacles. Intermediate degrees of adaptation should require intermediate amounts of distortion.

(c) Tests with familiar and unfamiliar targets. Whichever of the above procedures is used, *S* may be tested with familiar or unfamiliar targets. If the target is a familiar one, it is possible that his judgment may be somehow specific to it—he may have learned a new response for just this class of stimuli. If adaptation is a general change in the visual system, rather than one which is restricted to a particular class of visual stimuli, the same measurements should be obtained both with familiar and with unfamiliar targets.

(2) Prism-exposure. Eight *Ss*, age 19-35 yr., wore 20-diopter prisms mounted in spectacle-frames for a planned period of 42 days (one *S* dropped out after 30 days).[3] As in Kohler's experiments, the two prisms in each pair of spectacles were identically oriented, bases-right for half the *Ss* and bases-left for the others. The prisms had flat faces, were made of crown spectacle-glass, and were mounted in 40-mm. round, metal frames. Plastic side-pieces prevented any non-prismatic view; the binocular field was approximately 100° wide and 60° high. The *Ss* wore the spectacles continuously during their waking hours, but carried out their normal activities as college students.

Results : (1) Shearing distortion. Adaptation to the distortion contingent upon vertical eye-movements was measured by (*a*) test with variable prisms; (*b*) a spectacle-test; and (*c*) a square-adjustment-test.

(a) Variable test with prism. This test was carried out both with and without *S*'s own prisms. It was administered during the preëxposure period and every six days during the prism-exposure period with *S* wearing his spectacles, and during the preëxposure period, on the 21st day, and the 41st day of the prism-exposure period with *S* not wearing his spectacles. For the test, a variable prism was mounted monocularly over the right eye in a face-mask giving a field of view of approximately 40°.[4] The variable prism was covered with a monochromatic filter to prevent *S* from basing his null judgments on the prismatic color-fringes. Because of the filter and low ambient illumination in the test-room, only the brightly illuminated target was visible to *S*. Two types of target were used: a single horizontal line and random dot-patterns. The latter consisted of slides of randomly placed dots projected on a screen 95 cm. from *S*. The average angular size subtended by the dot-patterns was 14°, and each dot subtended an angle of 30′.

S was instructed so to adjust the variable prism that the target did not distort in any way, *i.e.* didn't shear, as he nodded his head up and down. Control measures were made by asking *S* so to adjust the prism that the target was undistorted while holding his head stationary. No consistent results were obtained in this control condition, suggesting that the test was in fact specific to the distortion contingent upon head-eye movements and not affected by extraneous cues.

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[3] We are indebted to Dr. Edwin Hart of Ithaca, New York, and to Dr. Bruce Kantar of Minneapolis, Minnesota, for performing ophthalmological examinations on our *Ss* before and after the experiment. The examinations included the following visual parameters: depth-perception, muscle-balance, convergence, accommodation, acuity, cycloplegic refraction, and condition of retina. Although there was a slight suggestion of improvement in some of the tests of eye-muscle coördination and balance, there were no clinically significant or statistically reliable changes in any of the functions tested.

[4] For a discussion of the optics of a variable prism, see K. N. Ogle, *Optics: An Introduction for Ophthalmologists,* 1961, 80-81.

The adaptation to the shearing as measured by the variable prism-test is shown by the solid lines in the section of Fig. 1 entitled "Tilt-Distortion." The solid circles indicate tests with spectacles and the unfilled circles the tests without spectacles. These are the results for the horizontal-line target. The data for *S*s with base-left and base-right prisms are plotted separately with base-left prisms arbitrarily being designated positive and base-right negative. In this terminology, the base-right *S*s wore −20 diopter prisms and the base-left +20 diopter prisms.

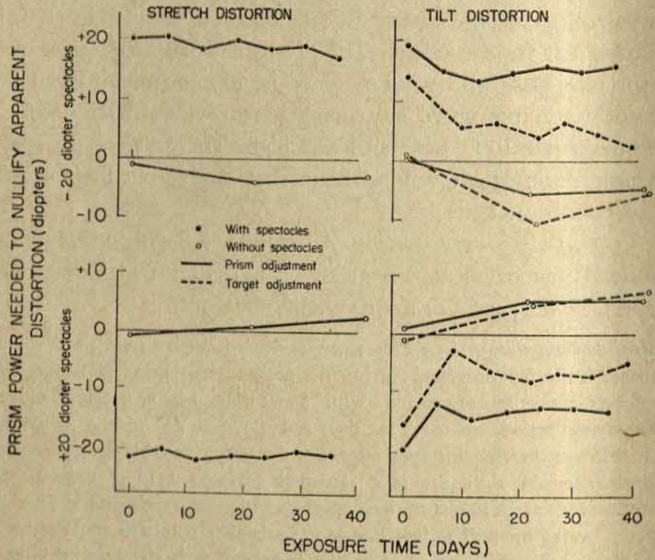

FIG. 1. PRISM-ADAPTATION TO GAZE-CONTINGENT DISTORTIONS

The adaptive change to gaze-contingent shearing (tilting) shown on the graph for the variable prism-test with spectacles (solid lines, filled circles) is confirmed by statistical analysis. The null setting for this test six days after putting prisms on is significantly lower than the pretest level. The mean shift in the adaptive direction for the results of all the *S*s pooled is 6.3 (± 4.7) prism diopters. (Figures in parentheses represent 95% confidence-limits.) The null settings remain consistently lower than the pretest level throughout the 42 days of the exposure.

The data for the completely unfamiliar dot-targets follow a similar course, but they do not become statistically significantly different from the pretest level until the 30th day the prisms were worn. Although the mean value of the adaptive shift remains the same, the variability increases on subsequent tests resulted in insignificant shifts.

Without spectacles (solid lines, unfilled circles) there is found approximately the same change in visual null, in this case away from the zero-compensation expected before exposure to the prisms. This change is statistically confirmed on the first testing occasion, after 21 days of prism-exposure. The mean shift in the adaptive direction here is 5.4 (± 1.6) prism diopters.

(b) Spectacle-test. In principle, this test was the same as the variable prism-test. It was modeled on the traditional procedure for fitting corrective eyeglasses: the patient tries out lenses of various strengths until he finds one which makes a standard target look undistorted.[5] In the present study, *S* removed his own spectacles and was given in random order a series of prisms mounted monocularly in spectacle-frames. Their strength varied in one-diopter steps from nine diopters with prism base in the same direction as *S*'s own prisms to nine diopters in the opposite direction. In this test, the right and left eyes were tested separately with unfamiliar targets (dot-patterns). As in the variable prisms test, *S* was asked to fixate the target, nod his head up and down, and select a prism which yielded no distortion. This test was administered on the 42nd day of prism-exposure.

The results of this test also indicate a significant adaptation for random-dots. The mean prism-strength selected was 3.41 (± 1:52) diopters, which is significantly different from the zero-setting which would be expected if no adaptation occurred.

(c) Square-adjustment test. This test in contrast to the two previous tests involved adjustment of the targets. The *S*, with stationary head, adjusted four points on a vertical screen until they formed a square. The ratio of the lengths of the two vertical sides after *S* put on his prisms served as an index of degree of distortion. Changes in this index toward unity as a function of prism-exposure measured adaptation.

S was seated 71 cm. from a uniformly white vertical board, 107 cm. wide and 73 cm. high. The board was draped with black cloth, giving it an irregular border. Small disk-magnets, which could be manipulated by *E* from behind the board, served as the points to be adjusted. The lower right-hand corner of the square was fixed. The width of the square also was fixed at 28 cm. by restricting the lower left-hand corner-point to move in a vertical track only. The upper left-hand corner-point was likewise restricted to horizontal movement. Except for these restrictions *E* moved the points at *S*'s command until *S* indicated a square had been formed. The position of *S*'s head was fixed by a biteboard approximately 8 cm. to the left of the right-hand corner-point and 10 cm. above it. The *S*s were tested with spectacles during the preëxposure period as well as periodically during the exposure-period. They were tested without spectacles during the preëxposure period and on the 21st and 42nd day of the exposure-period.

The results of this test are shown by the dotted lines in the section of Fig. 1 entitled "Tilt Distortion." The data obtained with spectacles (filled circles) and those obtained without spectacles (unfilled circles) show a similar change. (The data for this test were, of course, originally in target-measurements; they were converted by photographic techniques to equivalent prism-strength.) Again the adaptation is statistically significant, the mean shift in adaptive direction without spectacles is 11.4 ± 1.5 diopters. One curious feature of these data is that the test with spectacles shows consistently less deformation here than is shown on the variable prism-test of shearing. It seems reasonable to suppose that the visible context of the adjustable target, also deformed by the prism-spectacles, somehow reduced the apparent distortion. Without spectacles, prism-adjustment and target-adjustment show similar results for the distortion.

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[5] We are indebted to Professor Ivo Kohler for suggesting this procedure.

(2) Stretching distortion. Adaptation to the distortion that is contingent upon change of lateral eye-position with respect to the head was measured by the variable prism test and the spectacle-test.

The variable-prism and spectacle-tests were similar to the tests described above for the distortion of the vertical position of the eyes. The only difference was that *S* was asked to find a visual null while turning his head right to left rather than nodding up and down. All the other specifications of the test were identical.

(a) Variable prism test. The results of this test using random targets are shown in the section of Fig. 1 entitled "Stretch Adaptation." While there is no evidence of adaptation to this distortion with the spectacles on (filled circles on graph), there is a significant change without spectacles. The variable test without spectacles showed a mean shift of 2.3 ± 1.6 diopters on the 21st day of prism-exposure. *(b) Spectacle-test.* In this test with unfamiliar targets *Ss* selected a mean prism strength of 1.04 (± .73) diopters, showing a slight but statistically significant adaptation. Three *Ss* were given a supplementary target-adjustment test to measure adaptation to the stretch-deformation. In this test, *S* with head stationary viewed a luminous horizontal line in a dark room in the direction of the apex of his prism, and equated its length with a comparison-line viewed in the direction of the base of his prism. These three *Ss* showed consistent evidence of adaptation when measured without spectacles.

The most reasonable interpretation of these data on adaptation to stretch—distortion seems to be that there was a small visual adaptation, which was reliably detected by one test but not by another; certainly it is true that the amounts of stretch adaptation as measured by different tests do not differ from each other reliably.

(3) Effects of target-familiarity. The most direct comparison of target-familiarity is provided by the spectacle-series test. This test was given on the 42nd day of prism-exposure with familiar and unfamiliar targets. The former were ordinary objects in a large room; the latter were the random dot-patterns mentioned previously. The *Ss* showed 3.41 (± 1.52) diopters adaptation for shearing on unfamiliar targets, 3.04 (± 1.35) diopters adaptation for familiar targets. On stretching the amount of adaptation was 1.04 (± .73) diopters for the unfamiliar targets and 1.13 (± 1.03) diopters for the familiar targets. There was obviously no real difference in adaptation measured for the familiar and unfamiliar targets.

DISCUSSION

The results strongly indicate that a change occurred in the visual system during prism-exposure with the result that stationary horizontal lines appeared to tilt when the head was nodded up and down. A similar, but smaller, adaptation was suggested for stretching during lateral head-movements.

These after-effect deformations are similar to Gibson's after-effect of prism-curvature, as well as to Köhler and Wallach's figural after-effects, in that they are opposite to the actual optical deformations experienced during the exposure-period.[6]

The more familiar after-effects are tied, however, to a fixed locus on the retina like color after-images. If a fixed locus is in any sense to be attributed to the gaze-contingent after-effects, it must be defined relative to the field determined by the brows and cheeks which the eyes explore by turning in their sockets. Such a concept, similar to Helmholtz's *sehfeld* ("scan-field") implies that the response-characteristics of the moving eye are modulated continuously by the eye's position in the orbit.

These results are for the most part in direct agreement with Kohler's observations, the only major disagreement concerning the magnitude of the adaptations. Kohler suggests that almost complete adaptation took place, but none of our *S*s approached complete adaptation, and the developmental curves of Fig. 1 do not suggest that further exposure would result in further adaptation. The adaptation for shearing was only 25-50%, and it was essentially complete after six days of prism-exposure.

While our results strongly indicate that the adaptation is in the visual system and is reflected in a variety of response-measures, the detailed nature of the mechanism is not indicated in this study. A clue to the mechanism may be contained in the greater adaptation found to tilting or shearing than to stretching. Tilting was accompanied by vertical head- and eye-movements; stretching by horizontal movements. It may be that the vertical movements can affect the visual system more readily since they are associated with a distinctive category of reflex.[7] It may be, however, that the optical distortion of shearing is itself more susceptible to adaptation.

The prisms used in this study induce other distortions which have not been discussed: curvature of vertical lines, chromatic fringes along vertical brightness-contours, and lateral displacement of visual-targets. Strictly speaking, these distortions also are gaze-contingent. Their magnitude varies slightly with direction of gaze through the prism. This variation is, however, very small in comparison with the basic distortion itself. Nevertheless, in the present study supplementary tests indicated that adaptation contingent upon fixation occurred for the curvature-distortion, but not for the chromatic fringes and the lateral displacement. It is possible, of course, that such differential adaptation occurred for the chromatic fringes and the lateral displacement as well, but that the techniques of measurement were not sensitive enough to detect it.

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[6] J. J. Gibson, Adaptation, after-effect, and contrast in the perception of curved lines, *J. exper. Psychol.*, 16, 1933, 1-31; Wolfgang Köhler, and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. Phil. Soc.*, 88, 1944, 269-357.

[7] Mathew Alpern, Movements of the eyes, in Hugh Davson (ed.) *The Eye*, vol. 3, 78 ff.

SUMMARY

Adaptation to the gaze-contingent distortions produced by wedge-prisms was measured. Attention was focused on the tilting or shearing associated with vertical head-eye movements and the stretching and compression associated with lateral head-eye movements. In the course of 42 days of prism-exposure, small but statistically significant amounts of adaptation were shown for both these distortions by a variety of testing procedures.

THE EFFECTS OF GOAL-SETTING, RULE-LEARNING, AND KNOWLEDGE OF SCORE ON PERFORMANCE

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Both Ammons, and Bilodeau and Bilodeau, have pointed out that, while knowledge of results has well substantiated effects in improving performance, there are few theoretical principles that would predict or explain the results obtained in any systematic fashion.[1] Knowledge of one's over-all performance may affect motivation, but the specific mechanism is by no means clear. For example, a recent experiment by Chapanis demonstrated that by removing the "demand characteristics" inherent in most experimental settings (by convincing the *Ss* that they were performing a regular job rather than participating in an experiment) knowledge of performance was ineffective as an incentive.[2]

An hypothesis of the present investigation was that one possible mechanism might be the setting of performance goals that could influence level of effort. In other words, without changing the knowledge of the correctness or incorrectness of the answers to each problem, the knowledge of total score on a trial or series of trials would influence the performance goals the individual set for himself.

Previous studies demonstrating the relation of knowledge of total score to goal-setting activity and performance are few in number. Mace found that on a target-aiming task when knowledge of *total* score was introduced performance improved immediately, and when such knowledge was withheld performance declined.[3] All *Ss* had knowledge of the correctness of each throw from visual feedback. Mace claimed that the knowledge of results suggested appropriate performance standards to the *Ss* which resulted in a more sustained level of effort than was the case without them. Payne and Hauty gave some *Ss* knowledge of their total score in relation to a

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[1] R. B. Ammons, Effects of knowledge of performance: a survey and tentative theoretical formulation, *J. gen. Psychol.*, 54, 1956, 279-299; E. A. Bilodeau and I. McD. Bilodeau, Motor-skills learning, in *Ann. Rev. Psychol.*, 1961, 243-280.

[2] Alphonse Chapanis, Knowledge of performance as an incentive in repetitive, monotonous tasks, *J. appl. Psychol.*, 48, 1964, 263-267. On the use of the term "demand characteristics," see M. T. Orne, On the social psychology of the psychological experiment with particular reference to demand characteristics, *Amer. Psychologist*, 17, 1962, 776-783.

[3] C. A. Mace, Incentives: Some experimental studies, *Indus. Health Res. Bd. (Great Britain)*, Report No. 72, 1935.

standard.[4] These *Ss* were told that the standard represented the typical performance "of subjects like you," but actually it was one standard deviation above this point (so that *S* would be expected to beat it only 16% of the time). These *Ss* performed at a significantly higher level than *Ss* given no such knowledge of score. Although the *Ss* given knowledge in relation to a standard were not told explicitly to try to beat the standard, the implication was clear that they should.[5] Church and Camp gave some *Ss* signal lights on a reaction-time task to indicate whether or not they had beaten a score based on their own previous performance and found that these *Ss* improved their reaction-times more than *Ss* given no such knowledge.[6] Again, the information was given in relation to a standard, rather than in terms of raw scores, and this may explain its effectiveness. Finally, Fryer found that having *S* set performance-goals (levels of aspiration) before each trial led to a higher learning rate on a Morse Code task than giving knowledge of score at the end of each trial alone.[7] Locke found, upon analysis of these data, that this finding was dependent upon the level at which the goals were set. The *Ss* who set high goals performed better than the *Ss* with knowledge alone; those who set low goals did not.[8]

If it is true that knowledge of score is effective because it influences the setting of goals, then we would expect that *Ss*' goals set spontaneously on the basis of such knowledge would be systematically related to *Ss*' performance, whereas there should be no effect independent of the effect on goals. The present experiment was designed to explore this hypothesis. A second purpose of the experiment was to compare performance under whatever goals were set by *S* given varying knowledge of results with performance under conditions where a hard standard of performance was given by instructions. Previous work by Locke, and Locke and Bryan, suggested that the latter would yield a higher level of performance than other goals that *Ss* might pursue, such as "do as well as possible" or quantitatively lower goals.[9]

A third purpose of this study was to determine the effect of another cognitive component — the learning of rules relevant to the task — on performance. Haygood and Bourne have shown previously that learning of rules improves performance on a task requiring concept formation.[10]

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[4] R. B. Payne and G. T. Hauty, Effect of psychological feedback upon work decrement, *J. exp. Psychol.*, 50, 1955, 342-351.

[5] Personal communication.

[6] R. M. Church and D. S. Camp, Change in reaction time as a function of knowledge of results, this JOURNAL, 78, 1965, 102-106.

[7] F. W. Fryer, *An Evaluation of Level of Aspiration as a Training Procedure*, 1964.

[8] E. A. Locke, A closer look at level of aspiration as a training procedure: a reanalysis of Fryer's data, *J. appl. Psychol.*, 1966 (in press).

[9] E. A. Locke, The relationship of intentions to level of performance, *J. appl. Psychol.*, 50, 1966, 60-66; E. A. Locke and J. F. Bryan, Cognitive aspects of psychomotor performance: The effects of performance goals on level of performance, *J. appl. Psychol.*, 1966 (in press).

[10] R. C. Haygood and L. E. Bourne, Jr., Attribute and rule-learning aspects of conceptual behavior, *Psychol. Rev.*, 72, 1965, 176-195.

Similar studies in the area of verbal learning have shown that awareness of the rule *E* uses to reinforce *S* results in an immediate and marked performance gain.[11] The present investigators explored the generality of these findings with a different task and with a far more complex rule than has been used in previous studies.

Method: (1) Task. The task was the Complex Computation task used previously by Mace.[12] *S* is presented with a series of four-digit numbers. For each one he must perform a series of operations according to certain rules and come out with the answer, performing all the operations in his head (only the answer is written).[13] The task requires intense concentration and shows considerable learning.

The *S*s were given two 2-min. practice-trials on the task followed by 6 experimental trials of 10 min. each. There was one work sheet for each experimental trial containing 144 different problems.

(2) Subjects. The *S*s were 70 University of Maryland students, 43 men and 27 women, who responded to a college newspaper advertisement offering money for participation.

(3) Conditions. There were three experimental conditions to which the *S*s were assigned at random. The first group of 26 *S*s were given specific instructions that imposed a hard performance standard to aim for on each experimental trial. This we shall call the *STD* Group. The standard was set by adding 15 to the highest number correct attained on any previous trial. The second group, of 25 *S*s, had knowledge of their total performance. They computed the total number of problems they each got correct at the end of each trial and wrote that number at the bottom of the page. This is designated the *KR* Group. Finally, 19 *S*s did not compute their total score and were explicitly told to refrain from trying to count the number correct. This is labeled the *No KR* Group. Both of the latter groups were told to "do their best" on each trial.

All *S*s were given knowledge of the correctness of each answer, since at the end of each trial *E* read off the correct answers to each problem. In addition, the *STD* and *KR* Groups kept track of their scores by circling each correct answer and totaling them up. In the *No KR* Group, the *S*s did not circle their correct answers or total them; they simply followed *E* as he read the answers by looking at their own answers. Of course, these *S*s did obtain knowledge of their total performance implicitly, but such knowledge could not be very precise.

(4) Procedure. Each *S* was given a 3 × 5-in. index card with the formula for the computation on it to which he could refer at will. The formula was explained and three sample problems were done by *E*, step by step. Then, all the *S*s worked on two 2-min. practice-trials doing "as many as they could." The correct answers were given after each trial.

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[11] C. D. Spielberger, Theoretical and epistemological issues in verbal conditioning, in S. Rosenberg (ed.), *Directions in psycholinguistics,* 1965, 149-200.

[12] *Loc. cit.*

[13] "Call the four digits A, B, C, and D. If A is greater than B, multiply A by B; if A is less than B, add A to B. If C is odd, multiply C by D; if C is even, add C to D. If both of the previous operations were the same (i.e., both add or both multiply), subtract the first result from the second; if the two previous operations were different (*i.e.* multiply and add or vice versa) subtract the second result from the first. Write the answer."

At this point the *STD* Group computed the total number of problems correct on the second practice-trial, set the standard for the experimental Trial 1, and wrote that standard at the bottom of the work sheet for Trial 1. The *KR* Group simply computed the total correct on the practice trial and wrote this number at the bottom of the page. The *No-KR* Group did nothing. This same procedure was followed for each of the subsequent 6 experimental trials. The time between trials ranged from 4 to 7 min. and was the same for all the *S*s.

At the end of Trial 6 all the *S*s were asked to describe their performance goals in detail. Sample goals, such as "tried to do my best," "tried to improve each time," "had no goals," were given in each case to make the meaning of the question clear. In addition, the *S*s of the *No KR* Group were asked to what degree they had knowledge of their number correct on each trial during the experiment. All the *S*s were asked if they had been able to completely memorize the computation formula and, if so, by what trial.

Results: (1) Analysis by experimental conditions. The first analysis was done using the experimental groups as units. The experimenters re-scored all protocols for the *STD* and *KR* Groups in addition to scoring the *No KR* protocols. The measure used was the total number of problems solved correctly on the six experimental trials. There were no significant differences among the three experimental groups on this measure.

The *No-KR* Group was broken down on the basis of their responses to the post-experimental question asking them how much knowledge they had about their scores. The *S*s were divided into two groups: those who had a "fair or good" idea of how well they were doing and those who had a "very rough idea" or "no idea" of how well they were doing. There was no significant difference between the mean number correct of these two groups. Thus, without taking account of *S*'s goals, there appeared to be no effect of knowledge of score on performance.

(2) Analysis by goals. The post-experimental description of goals was used to classify *S*s into homogeneous groups. The 5 *S*s in the *STD* condition who indicated they were trying to reach the goals set by *E* were classified as *Standard*. The remaining *S*s fell into three groups: (*a*) *Improved:* the *S*s in this group indicated that their main goal during the experiment was to improve over their best previous score or to improve as they went along. A few mentioned trying to improve by a specific amount (*e.g.* 5 points); most did not. None indicated he was trying to improve by as much as the 15 points required of the *Standard* *S*s. (*b*) *Do Best:* the *S*s in this group who indicated that they were predominantly trying to "do their best" or "do as many as possible" and did not mention trying for any specific standard; several said explicitly that they paid no attention to their previous scores. (*c*) *Other* (low motivation): all the remaining *S*s were put into this residual group. They were trying less hard on the whole than those

in other groups. Some tried for awhile to do their best and then relapsed. Others were trying for accuracy only, and still others listed three or four different goals on different trials.

The present investigators first developed the categories together, wrote descriptions of them, and reclassified all the *S*s, using the descriptions (with only two or three disagreements). To check on the reliability of this classification, a naïve 'rater' was given the descriptions of the four categories and told to classify the *S*s from what each said was his goal. This rater agreed initially with the experimenters' classifications of 57 of the 70 protocols. Eight of the 13 disagreements were due to his inability to understand *S*'s statements, not being familiar enough with the experimental design and task to interpret the protocols. When the design and task were explained, he classified them as the *E*s did. This left only 5 of the 70 that the 'rater' could not classify. The two *E*s reëxamined these and classified them. It appeared at this point that further reliability checks would not prove fruitful as most of the goal descriptions were easy to classify.

TABLE I

Performance as a Function of Experimental Treatment and of Stated Goals

Each Score Is Mean Total Problems Correctly Completed in 6 Experimental Trials.

	Experimental treatment							
	STD		*KR*		*No KR*		Total	
Stated Goal	*N*	score	*N*	score	*N*	score	*N*	score
Standard	5	352.2					5	352.2
Improved	11	190.5	9	251.1	4	154.5	24	207.2
Do Best	6	249.8	5	276.2	6	256.8	17	260.1
Other	4	211.8	11	204.8	9	174.6	24	194.6
Total	26	238.5	25	235.8	19	196.3	70	226.1

	Improvement	*Do Best*	*Other*
Standard	3.41	1.99	4.23
Improvement		1.82	<1
Do Best			2.43

(3) Relationship of goals to performance. Table I shows the number of *S*s in each experimental condition who were placed in each goal classification and the mean number of problems solved correctly for each subgroup. The *Standard S*s showed the highest performance level; the *Do Best S*s, the next highest, were followed by the *Improvement* and the *Other S*s. This means each of these sub-groups is fairly consistent across the different experimental conditions.

For purposes of analysis, the sub-groups with similar goals were combined across experimental conditions and the totals shown in the last column. To test for differences in initial ability, an *F*-test was done on the scores for the two practice trials of these groups. The *F* was not significant, indicating there were no over-all differences in initial ability. The total scores during the experimental trials, however, tell a different story. The over-all *F*-ratio on the total performance scores of the four groups was 5.99 ($p < 0.01$). Excluding the *Standard* group and computing the *F*-value only for the *Improvement, Do Best,* and *Other* groups gives a value of 3.07 ($p < 0.06$). When *t*-tests were performed to test the differences between the group-means, the values shown at the bottom of Table I were obtained.

The difference between the *Standard* and the remaining three groups is substantial, suggesting that trying for a hard standard will lead to a higher level of performance than will the type of goal ordinarily set if *S* is just given knowledge of his results. As an indication of the relative difficulty of the goals that the Standard and Improved groups set for themselves, the *Standard Ss* were able to beat their goal only 13% of the time, whereas the *Improvement Ss* were able to beat their standards (*i.e.* their best previous score) 70% of the time. Since previous experiments have shown difficulty of the goal to be positively related to level of performance by the *Ss* who accept the goals, the reason for the difference between the two groups in this case seems clear. Unfortunately, similar estimates of difficulty could not be made for the *Do Best* or *Other* groups.

(3) Effect of memorizing formula. At the end of the experiment, the *Ss* were also asked: (1) whether they had memorized the computation formula before the end of the experiment; and (2) if so, by what trial they had memorized it. It was thought that memorization of the rule would facilitate performance since the *Ss* would not have to keep referring to the card all the time and thus take their eyes off the problem at hand and loss concentration. Upon the basis of their reports, the *Ss* were reclassified into four groups: Group 1, memorized by Trial 1-2, ($N = 17$); Group 2, memorized by Trial 3-4, ($N = 28$); Group 3, memorized by Trial 5-6, ($N = 12$); Group 4, did not memorize, ($N = 12$). One *S* was dropped from the analysis because he failed to report upon this point.

Since there were significant differences among the means of these groups on the practice trials, an analysis of covariance was used in determining the effect of memorization on later performance. The over-all *F*, corrected for the level of initial ability, was 5.47, which is significant at the 0.01 level. The trend of the result was as expected; the earlier *Ss* memorized the formula, the better their total performance.

In addition, linear slope-scores were computed for each *S* for each experimental segment, and a significant interaction ($p < 0.01$) was found between trial on which memorization occurred and the slope-score attained on each segment. The most rapid rate of improvement occurred during that segment in which *S* first memorized the formula.

Finally, Table II shows the Pearson product-moment correlations among the measures of initial ability, goal, memory, and total performance. The 'goals' were coded: *Standard* = 3; *Do Best* = 2; *Improvement* and *Other* = 1; and the 'memorization'-groups were coded: Trials 1 and 2 = 3; Trial 3 and 4 = 2; Trials 5 and 6 = 1; and Not at all = 0. The multiple correlation of all three predictors with total performance was 0.74 ($p < 0.01$). The correlation of goals with performance with the effects of initial skill and memorization group partialled out is 0.27 ($p < 0.05$); the correlation of memorization group with performance controlling for initial skill and goal is 0.33 ($p < 0.01$), suggesting both measures contributed independently to the overall prediction.

TABLE II

CORRELATION AMONG INITIAL ABILITY, GOAL MEMORIZATION GROUP, AND TOTAL PERFORMANCE MEASURES
($N = 69$)

	Goal	Memorization Group	Total Performance
Practice score	.26*	.37†	.64†
Goal	—	.36†	.45†
Memorization group	—	—	.53†

* $p<0.05$. † $p<0.01$.

DISCUSSION AND SUMMARY

It appears that giving *Ss* knowledge of total score under the conditions of our experiment does not result in any automatic gain in performance Rather, our results suggest that it is what *S* does with that knowledge, *i.e.* what kinds of performance goals he sets with it that is important.

Of considerable interest in the present experiment was the finding that setting a determinate and hard performance standard resulted in considerably higher performance than did other types of goals. This replicates a similar finding by Mace using a simpler computational task than the one used here and with 12-yr. old boys as *Ss*, as well as an earlier study by the present authors using a psychomotor task. The *Ss* perform at a higher level if given very hard goals and if they accept those goals than if they try merely to "do their best."

The effect of learning the rule in the performance of our complex task was shown to be considerable. The earlier *Ss* were able to memorize the rule, the better their performance. In addition, their performance improved most rapidly during the segment on which they first memorized the rule.

Finally, it was shown that initial skill, learning of the rule, and motivation, as measured by type of goal, made independent contributions to the prediction of over-all performance. In the present case, nearly 55% of the variation in performance was accounted for by these three measures.

ANCHOR-EFFECTS IN PITCH-LOCALIZATION

By EUGENE D. RUBIN, MARK E. WARE, and HARRY HELSON,
Kansas State University

Evidence that high pitches are localized higher in space than low pitches was first presented by Pratt, who stated that "prior to any associative addition there exists in every tone an intrinsic spatial character which leads directly to the recognition of differences in height . . . along the pitch continuum."[1] Since his pitches were an octave apart, and there were no inversions in the ordering of the tones, the evidence was very strong, especially since the octave in which a tone lies is often confused with other octaves.

It was not until nearly thirty years later that Pedley and Harper attempted to settle whether pitches are associated with a particular height in space or are subject to series-effects,[2] *i.e.* are localized absolutely or relative to other pitches. To test if position in a pitch-series would affect perceived height, these workers presented *O*s with three series of seven pitches in each of which two were the same, viz. 900 and 1400. In one series, they were the top members, in the second, they were the middle members, and in the third, they were the lowest members. The results were clear: when at the top of the series, the two pitches were localized a little above 5 on a 1-7 scale; when in the middle, they were localized about 4; and at the bottom of the series, they were localized a little over 3.

In this study, we seek to determine if localization in space of pitches is subject to anchor- as well as context-effects, as predicted from adaptation-level theory.[3] We hypothesized a low anchor should raise and a high anchor should lower perceived height of series-tones as compared with their positions when no anchor is employed. Two experiments were performed and will be reported to show that apparently minor differences in conditions may affect the results in this area of investigation.

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* Received for publication May 20, 1965. This study was supported in part by the Office of Naval Research under Contract Nonr 3634(01) with Kansas State University and in part by Public Health Service Training Grant 5TIMH-8359 from the National Institute of Mental Health.

[1] C. C. Pratt, The spatial characters of high and low tones, *J. exp. Psychol.,* 13, 1930, 282.

[2] P. E. Pedley and R. S. Harper, Pitch and the vertical localization of sound, this JOURNAL, 72, 1959, 447-449.

[3] Harry Helson, *Adaptation-Level Theory: An Experimental and Systematic Approach to Behavior,* 1964, 156.

EXPERIMENT I

Observers. The *O*s were 10 undergraduate men and women selected from introductory courses in Psychology.

Apparatus. The study was conducted in a small room, 8 × 3 × 13 ft. An audio frequency oscillator connected in series with an interval-timer provided the series- and anchor-tones. The output of the oscillator was fed to two speakers connected in phase and mounted behind a black cloth screen 3 ft. wide, reaching from the floor to the ceiling 1 ft. in front of the wall. On the screen was a white vertical strip 4 in. wide, marked with the numbers 1 through 7 at 1-ft. intervals from bottom to top; number 1 was located at 1 ft. from the floor. In addition, the scale was marked at 3-in. intervals between the numbers to aid the *O*s in making more accurate estimates of the positions of the tones. The speakers were placed behind numbers 1 and 7, and wires were led to positions where speakers would have to be if there were seven rather than two speakers behind the screen.

Procedure. There were three conditions: (1) in the control *(C)* condition, only the five tones consisting of 250, 375, 562, 844, and 1265 c.p.s. were presented; (2) in the high anchor (*HA*) condition, a tone of 2000 c.p.s. was presented before each of the series-stimuli; and (3) in the low anchor (*LA*) condition, a tone of 80 c.p.s. preceded the series-stimuli. The *C*-condition was always presented first, followed by either the *HA*- or *LA*-conditions, counterbalanced across the *O*s. Each *O* observed in all conditions.

O sat in a chair 10 ft. from the screen, with head-level with number 4 on the scale. He was provided with pencil and answer-sheet on which his judgments were recorded.

All tones were equated for loudness according to equal loudness contours for pitch as given in Chapanis, Garner, and Morgan.[4] Each tone was presented for 1.5 sec. with a 5-sec. rest-period between tones and a 1-min. rest-period between conditions. In the *HA*- and *LA*-conditions, there was a 3-sec. interval between the anchor and the series-tones, with the anchor always presented first.

O was told there were seven small speakers behind the screen, one behind each of the numbers, and that several tones would be presented randomly through these speakers. He was asked to record the number posted in front of the speaker from which each tone came. If the tone seemed to be between numbers on the scale, he was to record exactly where it seemed to come (e.g. 4½, 6¼, etc.). In the anchor-conditions, *O* was told he would hear a tone immediately followed by another, but that he was to record only the position of the second tone. A ready signal was given before each tone, and before the sequence of two tones in the anchor-conditions.

Each *O* made practice-judgments until he felt confident that he could locate the position of the tones on the scale. The 5 tones were then presented randomly 5 times making 25 reports by each *O* in each of the conditions. The data, therefore, are based on a total of 750 observations (10 *O*s × 25 judgments × 3 conditions).

Results. The results were combined and averaged for each condition. In the *HA*-condition, the anchor had a clear effect upon the series-tones; all tones were localized below the control localizations when no anchor was employed. The *LA*-condition, however, appeared to have had no effect on the series-tones as the *LA*- and *C*-curves crossed four times.

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[4] Alphonse Chapanis, W. R. Garner, and C. T. Morgan, *Applied Experimental Psychology*, 1949, 199.

An analysis of variance showed that there was a significant difference between stimuli, F (df 4, 36) $= 20.31$, $P < 0.01$, and between conditions, F, (df 2, 18) $= 1.47$, $P < 0.05$. The significant difference between stimulus-series shows that the stimulus-tones were judged as being at different heights on the scale: as the tones increased in pitch, they were judged higher on the scale. The significant effects between conditions show that the anchors did affect the series-tones, but this must be ascribed to the *HA*-condition in view of its clear separation from the control condition and not to the *LA*-condition. The difference between the mean of the *C*-condition and the mean of the *HA*-condition was 0.615, F (df 1, 18) $=$ 15.846. $P < 0.01$. The high anchor pushed the judgments of the series tones down an average of about 7.4 in. on the scale. The difference between means of the *C*- and *LA*-conditions was 0.053 (not significant). The *LA*-condition pushed the series tones up an average of only a little over 0.5 in. on the scale. The significant interaction between stimuli and conditions shows that the stimuli were differentially affected by the anchors in accordance with the usual repulsion-effects of anchors.

Discussion. The analysis of variance supported the prediction that the high anchor would lower judgments of vertical location, but did not support the prediction that the low anchor would raise the judgments. The ineffectiveness of the low anchor upon the series members must be attributed to a number of factors. It appears that the low anchor, far below the series-stimuli, was perceived as being somewhat unrelated to them. Another reason why the *LA*-condition was ineffective may have been that the equal loudness curves, used in calibrating the stimuli, were not appropriate for the experimental room, thus creating inequalities in the loudness of the tones.

EXPERIMENT II

A second experiment was therefore performed in view of the clear-cut anchor effect of the *HA*-condition, not only to verify the initial hypotheses of this study, but also to gain an understanding, if possible, of why the *LA*-condition failed to yield significant effects.

Observers. Twelve undergraduate women were recruited during a summer session from the introductory classes in Psychology.

Apparatus. A larger room (17 × 13 × 13 ft.) was used for the experiment. All other apparatus was identical with that of Experiment I, except for three changes. An electronic switch was introduced into the circuit to eliminate clicks which may occur at the onset and offset of the tones. It was found that some *Os* reported that these clicks appeared to shift the location of the tones. Two oscillators were used, one for the series-tones and one for the anchor-tones along with two attenuator pads for equating loudness. The wires that led to supposed oscillators behind the seven numbers on the scale were also removed.

Procedure. Several important changes were also introduced in the procedure. The series of tones was the same as in Experiment I, except that the *LA*-condition was moved up from 80 to 158 c.p.s., making it the same distance on a geometric scale as far below the lowest tone of the series as the *HA*-condition was above the highest tone.

Each *O* still served across all conditions, but in this case, all conditions were counterbalanced across all the *O*s. In this experiment, however, each *O* observed under only one condition per day, the three conditions, *C, HA,* and *LA* being given on successive days.

The presentation order of the tones was a 5 × 5 Latin square presented two times in succession. Every tone was therefore judged 10 times under each of the 3 conditions. The data are therefore based on a total of 1800 observations (12 *O*s × 5 tones × 10 observations × 3 conditions).

A short pilot study was run in order to equalize the loudness of the tones in the new room where the second study was conducted. All tones were equated in loudness to the middle tone (562 c.p.s.) using the method of limits.

The instructions emphasized the fact that this was purely a judgmental task, and that there were no right or wrong answers. The *O*s were encouraged to use the entire scale, and, if they wished, to use numbers above or below the scale (numbers greater than 7 or less than 1). They were only told that the speakers were behind the screen to counteract any preconceived ideas as to where the tones came from. They were also instructed to make verbal reports of the locations of the tones. It was emphasized that they were to keep their heads steady and turned toward the scale while the tones were being presented.

Results. The data of this experiment are graphically illustrated in Fig. 1. In this experiment, the low as well as the high anchor had significant effects on localization of the series-tones. The conditions were found to be significantly different in the analysis of variance, F (*df* 2, 100) = 58.35, $P < 0.01$, and, in the same analysis corrected for residual effects,[5] F (*df* 2, 4) = 11.984, $P < 0.025$. Multiple comparisons indicated that both *LA* vs. *C* and *HA* vs. *C* were significantly different.

Once again, the tones were judged as being at significantly different places on the scale, F (*df* 4, 100) = 255.30, $P < 0.01$. Particular sequence, practice effect (P), and all interactions were statistically not significant. The *adjusted* residual effects were also not significant.

When the tone component (T) and the tones × conditions (T) × (C) interaction were analyzed, there was a significant linear tone component,

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[5] W. G. Cochran and G. Cox, *Experimental Designs*, 1959, 133-138. At this point we wish to acknowledge with thanks aid in the statistical analysis of the data given by Professor Sam C. Brown of Kansas State University.

F (df 1, 124) = 756.91, $P < 0.01$ and a significant $T \times C$ linear component, F (df 2, 124) = 3.32, $P < 0.05$, indicating that, while all three curves were statistically linear, the slopes of the curves were significantly different. All other components were not significant.

Discussion. The results support the original hypotheses that anchors would shift the judgments of the vertical location of pitch. Both high and low anchors shifted localization of pitches significantly. Furthermore, the hypothesis that contrast effects would occur with both the *HA*- and *LA*-conditions was borne out statistically. From Fig. 1, it can be seen that the tones which were affected most by each anchor were those closest to the anchor, as has been found with lifted-weights and in vision.

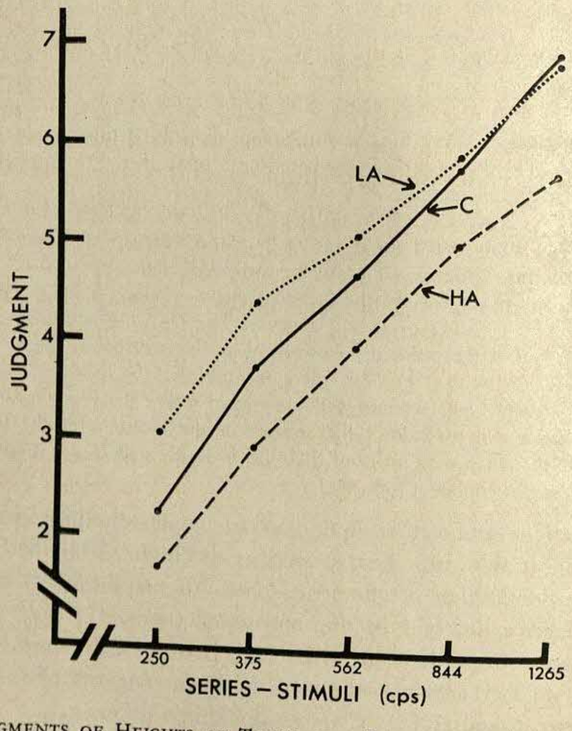

FIG. 1. JUDGMENTS OF HEIGHTS OF TONES AS A FUNCTION OF ANCHOR BELOW (LA) AND ABOVE (HA) THE SERIES TONES

In the Control (C) condition the series-tones were judged without anchor-tones.

The second study has shown that judgments of height of pitches are affected by factors which, if not controlled, can hide anchoring effects. Such factors as room size, reverberation time, absorbent qualities of the walls, background noise, which may mask certain tones, unequal loudness, insufficient number of observations, and instructional set may affect the judgmental process. For example, a pilot study was run for the second experiment in a special sound absorbent and shielded room. It was hoped that this would reduce any background noise which might influence judgments; but it turned out that the sound absorbent tile appeared to have reduced the effect of the higher tones, especially the high anchor, which was previously effective in the room used in the first study.

SUMMARY AND CONCLUSIONS

Typical anchor effects were found in pitch-localization: an anchor below the series-pitches displaced them upward, and an anchor above the series pitches displaced them downward with the maximal shifts occurring in series-stimuli nearest the anchors in accordance with the usual anchor-effects. It thus appears that pitch, supposedly a metathetic dimension, yields effects similar to loudness, a prothetic dimension, so far as localization is concerned. While it may be claimed that 'height' is not a metathetic dimension, Christman showed that satiating tones lower in pitch cause an upward displacement in perceived pitch, and higher satiating tones cause a downward displacement in perceived pitch.[6] Whether the anchor-effects found in this study result from apparent position of the anchors relative to that of the series-tones or from their effects on the *pitch* of the series-tones, or both, can only be answered by further experimentation. It thus appears that no distinction can be made between so-called metathetic and prothetic continua, so far as series- and anchor-effects in perception of pitch and apparent position in space are concerned. Furthermore, series- and anchor-effects appear to be the same with respect to pitch as they are in perception of loudness, brightness, and tactile-kinesthetic qualities.

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[6] R. J. Christman, Shifts in pitch as a function of prolonged stimulation with pure tones, this JOURNAL, 67, 1954, 484-491.

THE EFFECT OF PRACTICE ON THE SPEED AND ACCURACY OF EQUIDISTANCE-SETTINGS

By ALFRED LIT and W. M. VICARS, Southern Illinois University

Because few reports have been published on practice effects in depth discrimination, we thought it would be desirable to obtain quantitative data during the training period of a program on binocular space discrimination which could demonstrate how these discriminations are influenced by practice.[1]

The specification and control of practice have been a persistent task for classical psychophysics, and the early experiments in this area have yielded quantitative data on this problem.[2] Military interest in the effects of training on depth-discrimination also has generated a considerable number of experimental studies.[3] These studies reveal that practice typically leads to no systematic change in mean position of equidistance-settings. Response variability, however, decreases to a low value with extended practice. No experimental studies were found in which the speed of equidistance-settings was measured as a function of practice. Accordingly, the present study was designed to yield quantitative data on both the speed and accuracy of equidistance-settings obtained from two inexperienced *S*s during an extended training period.

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* Received for publication September 8, 1965. Supported in part by National Science Foundation grant G24021, Psychiatric Training and Research Fund of the Illinois Department of Mental Health, a Public Health Service Research grant MH-06621 and by Office of Research and Projects, Southern Illinois University.

[1] Cf., for example, recent reviews by E. J. Gibson, Improvement in perceptual judgments as a function of controlled practice or training, *Psychol. Bull.*, 50, 1953, 401-431; Perceptual learning, *Ann. Rev. Psychol.*, 14, 1963, 29-56.

[2] W. N. Kellogg, An experimental comparison of psychophysical methods, *Arch. Psychol.*, 17, 1929, 1-86.

[3] See, for example, A. H. Holway, D. A. Jameson, M. J. Zigler, L. M. Hurvich, A. B. Warren, and E. B. Cook, Factors influencing the magnitude of range-errors in free space and in telescopic vision, *National Defense Research Committee, Report to the Services Number 100, Division 7, Fire Control*, July 1945, 1-314; Norman Willard, Jr., C. A. Bancroft and J. G. Reddan, The training effectiveness of a stereoscopic range-finder trainer, *Human Resources Research Office, Technical Report 12*, Oct. 1954, 1-18; H. C. Olson and N. Willard Jr., A simplified method for rating the performance of stereoscopic range finder operators, *Human Resources Research Office, Technical Report 34*, Dec. 1956, 1-21; M. A. Schmitz, E. A. Stark and Norman Willard, Jr., Comparison of the stereoscopic range finder, M12 and the coincidence range finder T 43 as used in range determination at night, *Human Resources Research Office, Technical Report 53*, April, 1959, 1-26.

APPARATUS AND PROCEDURE

The apparatus used to obtain the data has been fully described and illustrated in earlier reports.[4] It is essentially similar to the two-rod test device known as the Howard-Dolman apparatus.

The standard (stationary) target is a black vertical rod, .32 cm. in diameter, which is located at a distance of 100 cm. in the upper half of *S*'s visual field and is laterally displaced 1.3 cm (= 0.75°) to the right of his median plane. The comparison (fixation) target is a similar vertical rod located in the lower half of *S*'s visual field and is movable to and from *S* in the median plane. The bottom end of the upper rod and the top end of the lower rod touch a horizontal plane through the eyes of *S*. *S* is seated in a dark room and binocularly observes the targets through a pair of circular artificial pupils 2.5 mm. in diameter. Uniform background illumination (0.71 log troland) is provided by a lightbox located 250 cm. from *S*.

Two inexperienced undergraduate students with normal vision served as paid *Ss*. In equating the apparent distance to the rods, *S* is required to fixate continuously on the top end of the lower rod and adjust this rod back and forth, using a 'bracketing' procedure, until it appears to lie in the same frontal plane as the upper standard rod.

The total training period was divided into two phases: an initial period consisting of approximately 18 experimental sessions and a terminal period consisting of about 10 experimental sessions. Five weeks separated the two training phases.

Each experimental session lasted approximately for 2 hr. Following a 30-min. period of dark-adaptation, *S* was allowed to make as many groups of 20 consecutive equidistance settings as he could achieve within the remaining 1.5 hr. On the average, about five groups of 20 equidistance-settings were obtained from each *S* during a single experimental session. *S* was not aware of the fact that the time for making each equidistance setting was being measured. The instructions which were given emphasized only the need for accuracy of setting. The response-time for each equidistance setting began when the fixation-rod was adjusted by *S* to reach a position 10 cm. either in front of or behind the standard rod, depending on the direction of initial displacement. The response-time ended when *S* signalled the completion of his equidistance setting.

The mean and average deviation of each group of 20 settings was calculated using the linear distance from *S*. These values have been converted into a difference between the angular separation of the rods in one eye and the angular separation in the other eye.[5] These were averaged further for 10 blocks of 20 settings, as will be made clear below.

A corresponding procedure was used for the response times. In this case, the medians were determined for the time of the block of 20 settings, and the average deviation was determined within each block around the median response time.

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[4] Alfred Lit and Aaron Hyman, The magnitude of the Pulfrich stereophenomenon as a function of distance of observation, *Amer. J. Optom.*, 28, 1951, 564-580; Alfred Lit, Magnitude of the Pulfrich stereophenomenon as a function of target thickness, *J. opt. Soc. Amer.*, 50, 1960, 321-327; The magnitude of the Pulfrich stereophenomenon as a function of target velocity, *J. exp. Psychol.*, 59, 1960, 165-175. See also, H. J. Howard, A test for the judgment of distance, *Amer. J. Ophthal.*, 48, 1919, 461-474.

[5] C. H. Graham, Visual perception, in S. S. Stevens, (ed.), *Handbook of Experimental Psychology*, 1951, 888.

RESULTS

(1) Effects of practice. The experimental results on the effects of practice on the accuracy of equidistance-settings are presented in Fig. 1. The

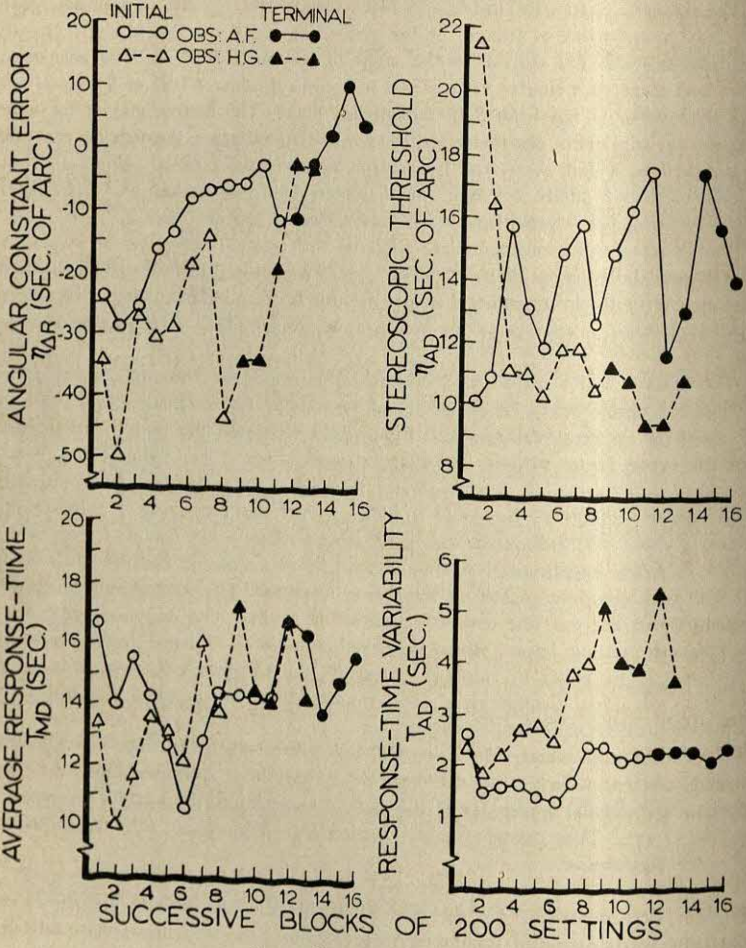

FIG. 1. EFFECTS OF PRACTICE ON THE SPEED AND ACCURACY OF EQUIDISTANCE SETTINGS.

Data for two *S*s are shown for both the initial period (open symbols) and terminal period (solid symbols) of practice for each of four response-variables.

magnitude of the constant error, in sec. of arc, and of the threshold as measured by the average deviation, are each shown as a function of practice. Each data point is based on a block of 200 settings, *i.e.* on the mean of 10 successive groups of 20 equidistance settings. For the threshold values, the mean is that of the average deviations within each group of 20 as described above.

From Fig. 1 it can be seen that each *S* initially exhibited a negative angular constant error (variable nearer to *S* than standard) which gradually reduced to zero as practice progressed through both phases of the experiment.

One *S*, (A.F.), gave threshold values that remained relatively low (about 14 sec. of arc) throughout both the initial and terminal phases of practice. For the other *S*, (H.G.), the threshold, that is, the mean average deviation, rapidly decreased from an initially high value of 22 sec. of arc to a final low value of 11 sec. of arc after only about 600 settings.

The experimental results on the effects of practice on the speed of equidistance settings are presented in lower half of Fig. 1. The median response time and that of the variability (average deviation) of the response times T are shown as a function of practice for each *S*. Each data point for median time is based on the average of the medians obtained for each of the 10 groups of settings. Similarly, the average deviations were averaged for the same 10 groups of 20 settings.

From this figure it is evident that, for both *S*s, both the median and its variability remained relatively constant throughout the experiment. The variability of H.G.'s times increased slightly from an initial value of 2 sec. to a final value of about 4.5 sec. A.F.'s time-variability was approximately constant for the entire experiment at about 2 sec.

(2) Intercorrelations among response-variables. Product-moment correlation coefficients (r) were calculated to determine the relationships existing among the four response-variables for each *S* during both the initial and terminal phases of practice. The correlation between the constant error and its corresponding response-time was essentially zero in all cases except for the initial practice-period for H.G. Here, r was $+0.17$. The grouped data similarly showed that no consistent relationship exists between the speed and accuracy of settings. The data revealed a slight but significant relationship between the constant error of the grouped settings and the corresponding variable error for A.F. ($r = +0.19$) for data obtained in the initial practice period and a slightly greater correlation ($r = +0.40$) for data in the terminal period. As expected, a moderate positive correlation was obtained between median response time and its variability ($r = +0.60$).

(3) Frequency-distributions of settings and response-times. The frequency-distribution polygons of the equidistance-settings and their response

times are presented in Fig. 2. The data for the first 1000 and the last 1000 responses are plotted separately in each case. It can be seen that each of the eight curves is essentially unimodal, fairly symmetrical, but somewhat leptokurtic.

SUMMARY

Two inexperienced *Ss* each made about 3,000 settings in a two-rod apparatus designed to test the threshold for binocular depth-perception.

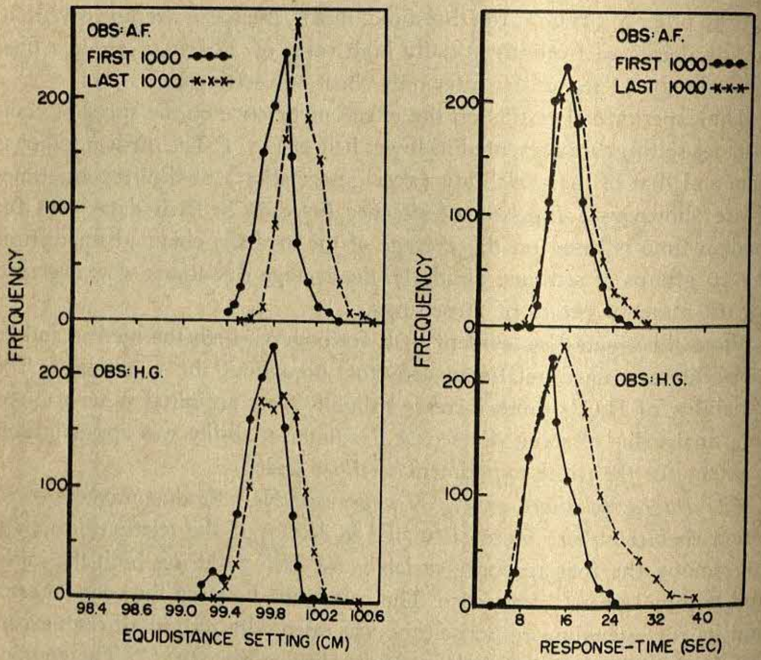

FIG. 2. FREQUENCY-DISTRIBUTIONS OBTAINED FROM TWO *SS* ON THE FIRST 1,000 AND LAST 1,000 EQUIDISTANCE SETTINGS
Standard target located at a distance of 100 cm.

The standard rod was located 100 cm. from *S*. The black targets were viewed against an illuminated background.

The results were analyzed in terms of both the constant and the variable errors of the settings. Unknown to *S*, the response-time for each setting was also measured.

The results show that the magnitude of the constant errors was initially roughly three times the threshold value and negative in direction for both

*S*s. However, the constant error gradually approached zero with practice but without benefit of knowledge of results. The variable errors were also high for one *S* and declined rapidly. They were constant for the other *S*. The response-time for each setting rapidly reached a value of 15 sec. The intercorrelations among the response variables are essentially zero for both *S*s except, of course, for the expected positive correlation between the median response-time and its variability.

SKIN-CONDUCTANCE AND REACTION-TIME IN A CONTINUOUS AUDITORY MONITORING TASK

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Activational theorists contend that level of performance rises with increases in physiological activity of an organism up to a point that is optimal for a given function and beyond this point further increases cause a drop in performance.[1] Thus, the basic proposition of this concept is that performance is best at some intermediate level of physiological activation and the relation between the two can be described by an inverted "U".

One study frequently cited in support of activation-theory is that of Freeman who studied the relation between palmar skin-conductance (*PSC*) and reaction-time (*RT*) with a single subject (*S*) under various states of alertness.[2] The results of 100 experimental sessions were recorded over a number of days. Freeman found an inverted "U" shaped relation between *PSC* and *RT*, in which *RT*s were slower at high and low *PSC* levels and fastest at the middle levels. Schlosberg reported that Freeman's result had been duplicated with another *S*, but a later study, using a greater number of *S*s (Schlosberg and Kling) failed to replicate these findings.[3] In none of these studies was a continuous measure of *PSC* obtained along with the *RT*s. The purpose of the present experiment was to explore the relationship between *PSC* and *RT* to an auditory stimulus when *PSC* was measured continuously.

Method. Sixteen students (9 women and 7 men) served as *S*s. *S*'s task was to sit quietly in a comfortable chair and to respond as quickly as possible to a 200-cps tone which was presented aperiodically over headphones, against a background of continuous white noise. The tone (70 db.) was generated by a Hewlett-Packard Wide Range Oscillator and a Scientific Prototype Audio Noise Generator was the noise source (60 db). Both tone and noise were presented to *S* via MSA Noisefoe Mark II headphones and the level of each was measured at the headphones with a General Radio Sound Level Meter. A tape programmer was used to actuate a three position relay. Holes were punched in a film strip at desired intervals and, when actuated, the relay performed three functions simultaneously: it started a Lafayette

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[*] Received for publication November 2, 1965. This study was performed while the author was a USPHS Predoctoral Research Fellow at Western Reserve University. The writer's thanks are due Dr. R. C. Wilcott, for his assistance during the conduct of this experiment.

[1] Elizabeth Duffy, *Activation and Behavior*, 1962, 139-194; R. B. Malmo, Activation: A neuropsychological dimension, *Psychol. Rev.*, 66, 1959, 367-386.

[2] G. L. Freeman, The relationship between performance level and bodily activity level, *J. exp. Psychol.*, 26, 1940, 602-608.

[3] Harold Schlosberg, Three dimensions of emotion, *Psychol. Rev.*, 61, 1954, 81-88; Harold Schlosberg and J. W. Kling, The relationship between "tension" and efficiency, *Percept. mot. Skills*, 9, 1959, 395-397.

timer, it allowed presentation of the tone, and it caused an event marker to produce a vertical line on the ink writer used to record *PSC* continuously. Thus, *E*, who was situated in an adjacent room could record *RT* to the nearest hundredth of a second and make continuous readings of *PSC*. *S* responded by pressing a switch mounted on the right arm of the chair. None was given any knowledge of results.

The *PSC* electrodes consisted of 3-cm. zinc-plates, mounted in hard rubber cups, with zinc sulfate paste as the conducting material. One electrode was placed on the palm of *S*'s left hand just below the base of the first finger, and the other electrode was placed below the base of the little finger of the same hand. Skin-resistance was recorded with a Wheatstone Bridge coupled into an Offner Type R Dynograph. The values of skin-resistance were later converted into conductance measures.

TABLE I

DIFFERENCES BETWEEN MEAN *RT* FOR *PSC* TRIALS, AND LEVELS

H = 10 highest values, *M* = 12 intermediate values, and *L* = 10 lowest values

Comparison	Mean diff.	*t*-value	*df*	*p* (two-tailed)
RT				
H versus *M*	34.00	2.31	15	< .05
H versus *L*	31.00	2.80	15	< .05
M versus *L*	3.00	.25	15	
PSC				
H versus *M*	2.50	5.13	15	< .01
H versus *L*	4.05	6.10	15	< .01
M versus *L*	1.55	5.03	15	< .01

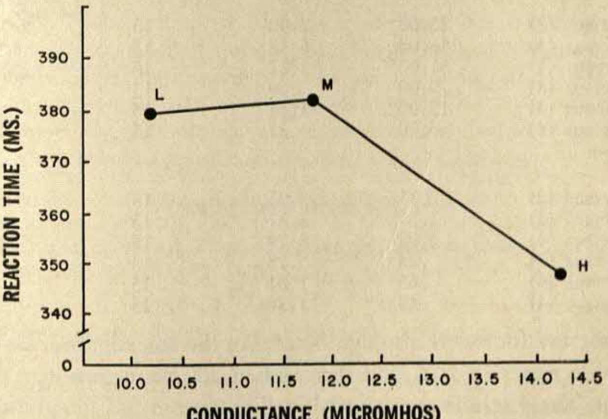

FIG. 1. REACTION-TIMES FOR MEANS OF 10 HIGHEST (*H*), 12 MIDDLE (*M*), AND 10 LOWEST (*L*) CONDUCTANCE-VALUES

Results. The difference between the mean *RT*s for those trials with the 10 highest conductance-values (*H*), 12 middle conductance-values (*M*) and 10 lowest values (*L*) were analyzed by *t*-tests for correlated data (Table I). These results indicate that on those trials for which *S*s had the highest mean *PSC* values, *RT*s were significantly faster than those trials in which the *PSC* measures were of intermediate or low magnitude. The mean *RT*s for each third, by conductance, is shown in Fig. 1.

Mean differences for the four successive segments of the experiment were examined by *t*-tests for correlated data (Table II). Trials 1-8 are represented by (1); Trials 9-16 by (2); 17-24 by (3); and 25-32 by (4).

There was a sharp decrease in conductance between the first and second 10-min. segments of the experiment, and this was accompanied by a significant increase in *RT*. The differences between the first segment and segments three and four were also significant. For the second, third, and fourth segments decreases in *PSC* and increases in *RT* were consistent but not significant. These results are depicted in Fig. 2.

TABLE II

DIFFERENCES BETWEEN MEAN *RT* AND MEAN *PSC* FOR THE DIFFERENT SEGMENTS OF THE EXPERIMENT

(1), Trials 1–8; (2), Trials 9–16; (3), 17–24; (4), 25–32

Comparison	Mean diff.	*t*-value	*df*	*p* (two-tailed)
RT				
(1) versus (2)	36.00	2.98	15	<.01
(1) versus (3)	45.00	3.53	15	<.01
(1) versus (4)	51.00	2.82	15	<.05
(2) versus (3)	9.00	.69	15	
(2) versus (4)	15.00	1.51	15	
(3) versus (4)	6.00	.37	15	
PSC				
(1) versus (2)	2.33	3.91	15	<.01
(1) versus (3)	2.55	3.37	15	<.01
(1) versus (4)	2.98	3.29	15	<.01
(2) versus (3)	.23	.78	15	
(2) versus (4)	.65	1.61	15	
(3) versus (4)	.43	1.36	15	

To determine the association between percentage-change in conductance and that in reaction-time for the means of the 10 highest and 10 lowest conductance-values, a Spearman rank correlation coefficient was computed for the 16 *S*s. The Spearman method was used since the percentage-values obtained were not normally distributed. A rank correlation coefficient of 0.47 was obtained and, for 14 degrees of freedom (N-2) this value was significant at the 0.05 level, indicating that changes in *RT* were significantly related to changes in *PSC* level for the 16 *S*s as a group. This result indicates, for example, that *S*s who ranked high in terms of percentage-decrease in *PSC* also ranked high with respect to percentage-increase in *RT*.

Discussion. The results of the present study differ from those of Freeman in that an inverted "U" shaped relation between *RT* and *PSC* was not found, *i.e.* there was no indication of an upturn in *RT* at the highest values of *PSC*.[4] Freeman used one *S* and experimental sessions occurred at all times of the day, including early morning and late at night, with *S*'s condition ranging between half asleep and extremely tense. Perhaps Freeman's method is more similar to a situation where *S*'s state is more actively manipulated, as in the case of studies of Induced Muscle Tension (*IMT*). An inverted "U" relation between level of effort and performance has been frequently reported in *IMT* studies.[5]

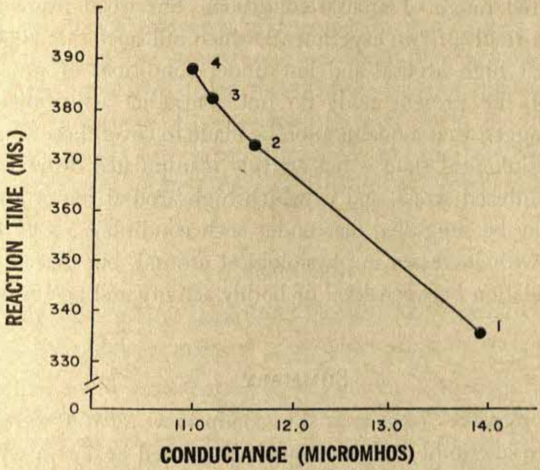

FIG. 2. MEAN REACTION-TIME AND CONDUCTANCE-VALUES FOR (1) TRIALS 1-8; (2) 8-16; (3) 17-24; and (4) 25-32

The findings of the present investigation are similar to those of Travis and Kennedy who investigated the relationship between frontalis muscular tension, reaction-time and level of performance in a continuous tracking task.[6] They reported that *RT*s became progressively slower with low levels of tension and faster when tension level was high. There was no evidence of an upturn in *RT* at the highest muscle tension levels, but there was no deliberate manipulation of tension-level by Travis and Kennedy, just as no effort was made to change *S*'s state in the present investigation. *S*'s state, however, did change gradually in the present study, as indicated by the progressive decrease in *PSC*-values during the course of the experimental session, and this change was related to a change in *RT*s as shown in Fig. 2. Most *S*s reported that the white noise presented continuously via the headphones produced drowsiness, making it difficult to stay awake. Thus, instead of showing an improvement in *RT* as the session progressed, *RT* became slower and the slowing was associated with decreased *PSC* values.

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[4] Freeman, *op. cit.*, 602-608.

[5] F. A. Courts, Relations between muscle tension and performance, *Psychol. Bull.*, 39, 1942, 347-367.

[6] J. L. Kennedy and R. C. Travis, Prediction and control of alertness: II. Continuous tracking, *J. comp. physiol. Psychol.*, 41, 1948, 203-210.

The fact that the results do not support the activational theory may be due to a limited range of arousal conditions. Sherwood made a similar suggestion as a result of two experiments which indicated that performance was high under high arousal and low under conditions of low arousal.[7] The results of the present study do not contradict activational theory. Rather, they suggest that a qualification be made to cover those situations in which *S*'s physiological state is not actively manipulated through physical work or task-induced stress, and in which high arousal conditions are not attained. It may be suggested that under such conditions *S*'s performance will improve with increases in physiological arousal, but that an inverted "U" shaped relation between level of bodily activity and performance will not be found.

SUMMARY

Continuous measures of palmar skin-conductance (*PSC*) were taken as *S* responded to aperiodic auditory signals presented against a white noise background. Thirty-two reaction-time (*RT*) trials were taken for each of 16 *S*s over a 40-min. session. The results indicated that *S*s had significantly faster *RT*s on the 10 trials in which *PSC* was highest as compared to the *RT*s for the 12 middle and 10 lowest *PSC* trials. There were decreases in *PSC* as the experiment progressed. The sharp decrease in *PSC* between the first and second 10-min. segments of the experiment was accompanied by a significant increase in *RT*.

There was no upturn in *RT* at the highest levels of *PSC* and it was suggested that in certain situations *S*'s level of arousal must be actively manipulated to achieve an inverted "U" relation between bodily activity level and performance.

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[7] J. J. Sherwood, A relation between arousal and performance, this JOURNAL, 78, 1965, 461-465.

TIME-ESTIMATION AT REDUCED BODY-TEMPERATURE

By A. D. BADDELEY, Medical Research Council, Cambridge, England

Hoagland has shown a relationship between increased body-temperature and time-estimation.[1] He tested two *S*s whose temperatures were raised by fever and one whose temperature was increased by diathermy. When asked to count up to 60 at what they considered to be a 1-sec. rate, they showed a positive relationship between oral temperature and speed of counting. A similar relationship had been found previously by François, using two *S*s who had been heated by diathermy.[2] On the basis of this evidence, Hoagland postulated a chemically regulated internal clock; the higher the temperature, the quicker the chemical reaction, and the faster the clock. Two recent attempts to replicate Hoagland's result have been made, one successful,[3] the other not.[4]

EXPERIMENT I

The following experiment explores Hoagland's chemical-clock hypothesis in terms of the effects of *reduced* body-temperature on time-estimation.

Method. The *S*s, 20 amateur scuba divers, all men, were tested during a week of diving in cold sea water (4°C) off the coast of Wales during cold March weather. Half the *S*s were tested for the first time just before diving, and for the second time just after, while the remaining *S*s were tested first after a dive. Subsequently, the *S*s were tested whenever possible, which in some cases included a control test separated from diving by several hours. It proved impracticable to test all the *S*s equally often, and, consequently, the number of observations per *S* ranged from 2–5, with a mean of 3.5. In the tests, the *S*s were seated and asked to count to themselves up to 60 at what they considered to be a 1-sec. rate. Pulse-rate was then measured, after which *S* estimated a second minute. Temperatures were measured orally by a clinical thermometer.

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* Received for publication December 2, 1965. The author is grateful to the members of the Cambridge University Underwater Exploration Group and R.E. divers who served as the *S*s, and to Captain J. Chilton of Cyprus Port Unit R.E. for his help in arranging Experiment II.

[1] Hudson Hoagland, The physiological control of judgments of duration, *J. gen. Psychol.*, 9, 1933, 267-287.

[2] Marcel François, Contribution a l'etude du sens du temps. La temperature interne, comme facteur de variation de l'appreciation subjective des durees, *Année psychol.*, 28, 1927, 186-204.

[3] R. J. Kleber, W. T. Llamon, and Sanford Goldstone, Hyperthermia, hyperthyroidism and time judgments, *J. comp. physiol. Psychol.*, 56, 1963, 362-365.

[4] C. R. Bell and K. A. Provins, Relation between physiological responses and environmental heat and time judgments, *J. exp. Psychol.*, 66, 1963, 572-579.

Results. Mean body-temperature, pulse-rate, and counting time for tests before and after diving are shown in Table I. Wilcoxon's test showed that the *S*s were significantly colder after the dive ($p < 0.001$), counted more slowly ($p < 0.02$) as Hoagland's hypothesis would predict, but also had a lower pulse-rate ($p < 0.05$). Most *S*s were tested several times, and there were considerable differences between dives in the extent to which body-temperature fell. A more detailed analysis took this into account as follows: For each *S*, a rank-correlation (rho) was computed between length of an estimated minute and (a) bodily temperature; (b) pulse-rate; and (c) order of test. In each of these cases the hypothesis, that the correlation shown by the 20 *S*s deviated significantly from zero, was tested. Since the number of observations on which they were based varied from *S* to *S*, the correlations could not be assumed to conform to an ordered metric scale, and the significance of the relationships was assessed by the sign test. A positive relationship between body-temperature and rate of counting was shown by 16 of the 20 *S*s (median rho = $+0.50$, $p < 0.01$). Twelve *S*s showed a positive relationship between pulse- and counting-rate (median rho = $+0.20$, $p > 0.1$), and 10 showed a positive relationship between counting rate and order of test (median rho = 0.20, $p > 0.1$).

TABLE I

Mean and Range of Temperature, Pulse-Rate, and Time-Judgment before and After Diving in Very Cold Water

	Oral temp. (°F)	Pulse-rate (per sec.)	Time judged as 1 min. (sec.)
Before diving	97.39	86.40	64.48
	(95.0–98.8)	(66–120)	(35–105)
After diving	95.03	76.88	70.44
	(93.5–97.5)	(54–99)	(40–119)
Diff.	2.36	9.52	−5.96

Experiment II

The results of the Experiment I support Hoagland's hypothesis of a chemical clock by showing that there is a tendency for the *S*s to count more slowly when oral temperature is reduced; however, since changes in body-temperature in this study are always associated with diving, it is possible that some other factor may be crucial. Most *S*s appeared to find the diving relatively stressful, and, since there is evidence that anxiety may influence time-estimation, it is possible that the results may be due to pre-dive stress causing faster counting *before* the dive, rather than low body-temperature's producing slower counting *after* the dive.[5] Experiment II examines this possibility by requiring the *S*s to estimate a minute before and after a warm but stressful dive.

Method. Divers were required to take a 1-oz. charge of gun cotton with fuse already lit and place it on a wreck lying in 15 ft. of water. If the charge did not explode within a few minutes, the diver was required to dive and retrieve it. Before and after diving, he was seated and asked to estimate a minute, as in Experiment I, and both oral temperature and pulse-rate wese recorded. Eight divers of the Royal Engineers and six amateur civilian divers served as the *S*s. Since the test took place in fine August weather in Famagusta Bay, Cyprus, air- and water-temperatures were relatively high.

Results. Table II shows the mean and range of oral temperature, pulse-rate, and length of estimated minute before and after diving. Only pulse-rate shows a reliable change, and even this is small and by Wilcoxon's test is only marginally significant ($T = 24$, $z = 1.79$, $p = 0.037$, 1 tail).

TABLE II

MEAN AND RANGE OF TEMPERATURE, PULSE-RATE, AND TIME-JUDGMENTS BEFORE AND AFTER A WARM BUT STRESSFUL DIVE

	Oral temp. (°F)	Pulse-rate (per sec.)	Time judged as a min. (sec.)
Before diving	98.37 (97.4–99.4)	86.8 (68–102)	55.4 (30–94)
After diving	98.20 (96.4–100.0)	81.4 (50–106)	54.8 (35–73)
Diff.	0.17	5.4	0.6

DISCUSSION

Both the studies described are essentially field experiments in which *E* had little control over the variables. In Experiment I, the crucial variable was *body*-temperature, while the variable measured was *oral* temperature. Since the face was the one part of the body directly exposed to the water, oral temperature was likely to give a reading which is somewhat lower than deep-body temperature. The absolute temperature-values shown in Table I are therefore probably underestimates, and any relatively small drop in oral temperature may possibly not reflect a change in deep-body temperature.

A further problem raised by Experiment I is whether some other factor which co-varied with oral temperature might be producing the change in time-estimation. The most plausible alternative explanation is probably in terms of stress or arousal. Since both these concepts embrace a whole complex of imperfectly correlated variables, testing this hypothesis becomes very difficult.

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[5] J. L. Falk and Dalbir Bindra, Judgment of time as a function of serial position and stress, *J. exp. Psychol.,* 47, 1954, 279-282.

In Experiment II, an attempt was made to avoid some of these difficulties by using a situation equivalent to that used in Experiment I except for the reduction in body-temperature. How successfully this was accomplished is difficult to assess. The only available index of arousal was mean pulse-rate, which was comparable before the dive, (86.4 per min. in Experiment I and 86.8 in Experiment II) but showed a slightly greater drop following the dive in Experiment I than in Experiment II — (9.5 as against 5.4). This difference does not, however, approach significance by the Mann Whitney test ($p > 0.1$), and it probably is due to bodily temperature.[6] What little evidence there is, then, suggests a roughly similar level of arousal in the two situations. Since the counting rate goes slightly in the opposite direction to that predicted by an arousal interpretation, it seems most plausible to explain the results of Experiment I in terms of body-temperature.

In general, then, these results support Hoagland's chemical clock hypothesis and suggest that it holds for reduced as well as for increased temperatures. If we assume, however, that Hoagland's generalization is justified, we are left with the problem of why Bell and Provins failed to show an effect.[7] They performed three experiments, of which two, though relevant, are not crucial to Hoagland's hypothesis, since they required *S* to judge longer intervals than those to which Hoagland applied his model.[8] They also are open to the objection that *S* was required to perform a complex task during the interval to be judged. In one of the experiments however, conditions were comparable with Hoagland's, the *S*s being required to estimate two short, unfilled intervals (45 and 100 sec.). In that experiment, 14 of the 20 observations were in the direction predicted by Hoagland, a result which is marginally significant by a Sign Test ($p = 0.058$, 1 tail).

It is possible to prove the results of the latter experiment with those of the Kleber experiment[9] and estimate the time it would have taken the *S*s in these experiments to count out a minute. Fig. 1 shows this estimate for the hot and cool conditions, in both studies, together with the equivalent data from Experiments I and II. The results appear to describe a function which could reasonably be interpreted as supporting Hoagland's hypothesis.

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[6] O. G. Edholm, J. M. Adam, and R. H. Fox, The effects of work in cool and hot conditions on pulse rate and body temperature, *Ergonomics*, 5, 1962, 546-556.

[7] Bell and Provins, *op. cit.*, 572-579.

[8] Hoagland, *op. cit.*, 283.

[9] Kleber, Llamon, and Goldstone, *op. cit.*, 362-365.

SUMMARY

In the first of two experiments 20 Scuba divers were asked to count up to 60 at a 1-sec. rate at various times during a week of diving in cold sea water. Rate of counting was found to be correlated significantly and positively with body-temperature, but not with pulse-rate or with order of test. A second experiment tested the hypothesis that this result was due to pre-dive anxiety causing *S* to count more rapidly before a dive. When other divers counted a minute before and after a warm but stressful dive involving the placement of explosives, there was no change in counting rate. It may be concluded that body-temperature affects time-estimation in the manner predicted by Hoagland's chemical-clock hypothesis.

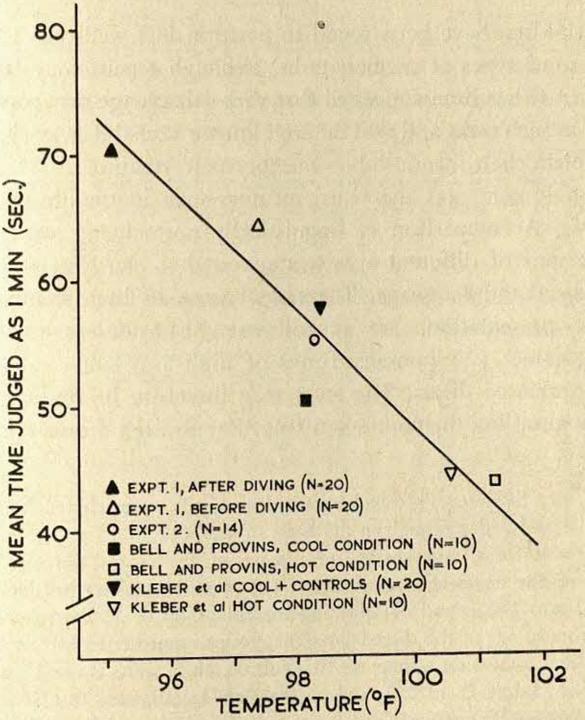

FIG. 1. LENGTH OF TIME JUDGED AS 1 MIN. AS A FUNCTION OF BODILY TEMPERATURE
(Data from three studies; function fitted by eye.)

VISUAL MEMORY-SPAN IN THE DEAF

By James E. Olsson and Hans G. Furth
The Catholic University of America

Deaf children have been found to perform less well than hearing children on some types of memory-tasks, although results were by no means consistent.[1] It has been suggested that verbal language may play an important role in such tasks and that the well known verbal deficiency of the deaf could explain their inferiority on memory-performance.

This study compares the visual memory-span in the deaf with that of hearing *Ss*. A comparison of linguistically normal and linguistically deficient persons of different ages was expected to clarify relations between memory-span and language. The tasks, given in both simultaneous and successive presentation, are as follows: (1) nonsense-forms of low asociative value; (2) nonsense-forms of high asosciation value; and (3) visually presented digits. The tasks may therefore be considered as progressively sampling the continuum from low to high accessibility of verbal labels.

Method: (1) Subjects. Thirty adolescent deaf pupils were selected from the files of a residential and a day school on the basis of age (12 to 16 yr.), *IQ* (above 70), early severe deafness, and absence of disease of the central nervous system. The mean *IQ* of the residential deaf ($N = 15$) was 103.3, of the day school deaf ($N = 15$) was 102.3, and of a hearing control group of 15 day school pupils was 103.9. The mean age of the three adolescent groups ranged from 13.7 to 14.3.

A deaf and a hearing group of 16 adult *Ss* each were equated on age (mean age = 38 yr., range 25 to 55), and on the Beta Intelligence Test (mean *IQ* 106). The deaf *Ss* came from two clubs for the deaf and were deficient in verbal skills. All five groups had about an equal number of men and boys and women and girls.

(2) Tasks. The memory-span for digits was taken from the WAIS and Wechsler Memory-Scale. The former was used for the successive presentation of digits, and

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* Received for publication July 8, 1965. This study was aided in part by Training Grant 198-T from the United States Vocational Rehabilitation Administration. The coöperation of the Maryland School for the Deaf and the Wm. S. Baer School in Baltimore is appreciated.

[1] F. Blair, A study of the visual memory of deaf and hearing children, *Amer. Ann. Deaf*, 102, 1957, 254-263; D. G. Doehring, Visual spatial memory in aphasic children, *J. Speech Hear. Res.*, 3, 1960, 138-149; H. G. Furth, Visual paired associates task with deaf and hearing children, *J. Speech Hear. Res.*, 4, 172-177; C. P. Goetzinger and C. L. Rousey, A study of the Wechsler performance scale (Form II) and the Knox Cube Test with deaf adolescents, *Amer. Ann. Deaf*, 102, 1957, 388-397.

the latter for simultaneous presentation. Each type of presentation consisted of two sets of 3 through 9 digits.

The memory-span for form consisted of 50 nonsense-forms, grouped into high and low associative sets. Each set contained series of 3, 4, 5, 6, and 7 forms. There were 10 series, 5 each in the high and low associative set.

The associative value of the forms was determined by projecting 100 forms on a screen before 56 college students with instructions to write a verbal association to each form and to rate the ease of association from one to three. The 25 forms highest in associative value were placed in the high set and the 25 forms lowest in value were placed in the low set. The series of forms within each set were approximately equated in average associative value.

(3) Procedure. The adults first completed the Beta Intelligence Test. All groups followed then a similar testing procedure except that the residential deaf adolescents were not available for the digit task. The instructions for this test were given to the deaf by a person who used speech combined with manual signs. Group procedures were used throughout with the groups varying in size from 8 to 11 *S*s. The total time taken for the testing varied from 1-2 hr. depending upon the slowness of the group and the number of tasks administered. Short rest-periods (3-5 min.) were given between tasks according to the needs of the *S*s.

TABLE I

MEAN SCORE AND STANDARD DEVIATION ON MEMORY-SPAN FOR FORMS

*S*s	High associative value		Low associative value	
	successive	simultaneous	successive	simultaneous
Adolescents:				
deaf, residential	5.40 (1.49)	6.07 (1.12)	4.20 (1.04)	4.73 (1.29)
deaf, day school	5.53 (.89)	5.80 (1.04)	3.93 (.86)	4.80 (.98)
hearing	5.67 (1.13)	5.87 (1.21)	4.47 (1.63)	4.07 (1.23)
Adult:				
deaf	4.69 (1.04)	5.44 (1.41)	3.75 (1.34)	4.19 (1.51)
hearing	4.94 (1.29)	5.13 (1.41)	3.75 (.83)	3.94 (1.25)

With the digit-task, the digits were first presented successively with 1-sec. exposures and 2.2 sec. between successive exposures. A Bausch and Lomb "Balomatic 500" automatic projector was used to project 2 × 2 slides of digits on a screen. The *S*s were instructed to write down the digits in the exact order of presentation when the series ended, which was indicated by the word 'Write' on the screen. To communicate effectively with the deaf, written and spoken instructions together with demonstrations, were used. The first series of three digits was used as an example, and all the *S*s' answers were checked to see that the task was understood.

The digits were then presented simultaneously, with instructions and a demonstration of the first three-digit series. The exposure-time for each series was 1 sec. for each digit in the series, hence the exposure-time in seconds equaled the number of digits in the series.

For the form-task each group of *S*s was divided into two subgroups, one of which received the successive presentation before the simultaneous, and the other, the reverse. Within each type of presentation, the high and low associative series alternated (*e.g.* high association 3-form-series, low association 3-form-series, high association 4-form-series, etc.). Each *S* was given a packet of 20 envelopes containing the facsimiles of the forms for each series. They were told, along with a demonstration, that forms would be shown on the screen in a particular order, and that after the presentation, they were to take the facsimiles of the forms shown on the screen from the appropriate envelope and put them in the order of presentation from left to right on the table. For the first two series ('high' and 'low' association of 3 forms), *E* checked the order of the forms for all the *Ss* and made certain they understood the task.

In the successive presentation, the exposure-time for each form was 2 sec. with 2.2 sec. between successive exposures. The exposure-times for the simultaneous presentation were 8, 10, 12, 14, 16 sec. for the series from 3 to 7 respectively.

In summary, adolescent and adult groups of hearing and deaf *Ss* were tested on memory for visual digits and for nonsense forms of high and low associative value. Each of the tasks was presented simultaneously and successively. The same *Ss* performed across all tasks and an *S*'s score was the highest series of elements (digits or forms) remembered in the exact order of presentation.

TABLE II

MEAN SCORE AND STANDARD DEVIATION ON MEMORY-SPAN FOR DIGITS

*S*s	Successive		Simultaneous	
	mean	*SD*	mean	*SD*
Adolescents:				
deaf, day school	6.13	1.81	6.47	1.92
hearing	7.80	1.37	8.27	1.10
Adults:				
deaf	5.56	1.27	6.38	1.26
hearing	7.19	1.46	8.25	1.20

Results. Two separate analyses of variance of mixed factorial design were done for the form-task, one for the adolescent, the other for the adult groups.[2] The resulting *F* values were significant for Associative Value ($F = 57.02$ and 30.39, $p < 0.01$) and Type of Presentation ($F = 14.79$, $p < 0.01$ and 6.51, $p < 0.05$) at the adolescent and adult level, respectively. There were no reliable interactions except for Presentation × Deafness for the adolescent groups ($F = 6.12$, $p < 0.05$). No significant difference emerged between hearing *Ss* and residential or day school deaf *Ss*. High association was consistently helpful for all groups.

From an analysis of variance of the digit test, two results emerged with no significant interactive effects. The hearing were consistently superior to the deaf ($F = 60.09$, $p < 0.001$) and performance was generally better for simultaneous than successive presentation ($F = 4.85$, $p < 0.05$).

Discussion. The main results can be considered consistent in that they were replicated in two different age-samples both in the hearing and the deaf. Three interesting points emerged concerning the relation of deafness and memory. The deaf were consistently poorer on digits but differed minimally from the hearing on nonsense-forms; high association helped the deaf as much as it helped the hearing; the simultaneous condition was generally easier than the successive condition with an indication that on simultaneous presentation of forms the adolescent deaf did somewhat better than the hearing, while the hearing did better than the deaf on successive presentation.

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[2] E. F. Lindquist, *Design and Analysis of Experiments in Psychology and Education,* 1953, 292.

The first point illustrates that general statements about deaf people's memory-performance are in need of clarification. In our case a specific reason for their specific failure could be the continuous practice with digits to which the hearing, more than the deaf, are exposed. One need only think that hearing persons are freely exposed to telephone numbers and academic subjects concerned with numbers such as mathematics, science or history, while the deaf have only limited experience from these sources. Such an explanation in terms of past experience is parsimonious and is analogous to findings with the blind. Blind adolescents have been observed to show an oral digit span far above the average of seeing peers.[3] The reason for their superiority relates not to blindness itself but to the blind person's difficulty in writing down or looking up numbers as easily as do sighted people. The fact, however, that on nonsense-forms deaf and hearing *S*s performed similarly while on digits they differ requires further explanation.

One could have expected that a high associative value would have helped the hearing to a greater extent than the deaf since it is based on verbal associations. But as high and low associative value had similar effects on deaf and hearing *S*s, one may conclude that verbal associations as such did not contribute substantially to better memory performance. Rather it seems that high associative value may be associated with more familiar forms and for that reason these forms are both easier to remember and have more readily available verbal associations. While verbal associations were not readily available to the deaf, familiarity was apparently of equal help to both deaf and hearing *S*s.

The group which contributed most to the interaction between deafness and mode of presentation was the adolescent hearing group on low associative forms simultaneously presented, this being the only instance where a group performed more poorly on simultaneous than on successive presentation. For this reason it seems appropriate to attribute this interaction to the poorer performance of this hearing group, at least as much as to a supposed verbal factor which helped in the successive condition.

SUMMARY

Deaf and hearing adolescents and adults of both sexes were equated for age and intelligence and given test in visual memory-span. Consistent

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[3] Personal observation by H. G. Furth of the performance of blind adolescents on Wechsler's Digit Span-subtest made at Oregon and Washington State Schools for the Blind.

results at both ages indicated that deaf and hearing performed alike with nonsense-forms, but that the deaf were inferior with digits. Both hearing and deaf *S*s benefitted equally from high as compared to low associative value of forms and, with one exception, from simultaneous as compared to successive presentation. These specific results seem to support the role of previous experience rather than verbal mediation in immediate recall.

APPARATUS

A STEREOSCOPE DESIGNED FOR USE IN RESEARCH ON MOTIVATED PERCEPTION

By INGVAR BOKANDER, Lund University, Sweden

Since Wheatstone, in 1838, described the first stereoscope, there have been many modifications in the apparatus originally designed for the study of visual space perception. Moreover, in later years use of the stereoscope has been extended to the field which is known as motivated perception. In this field of work it appears fruitful to study the reactions originating when different objects are presented on corresponding retinal areas in the two eyes.

The new field of application has required the construction of special apparatus. Thus, for example, the need of variable light intensities and exposure times has influenced the design of these instrument. Engel describes a simple prism-stereoscope with adjustments for convergence and light intensity in the two picture fields.[1] Grindley presents a mirror stereoscope with semi-transparent mirrors that permit constant accommodation even when the picture fields are not lighted.[2] Adlerstein has constructed a variant of a mirror stereoscope that permits bigger objects to be presented,[3] *e.g.* human faces in full scale or projected pictures and film sequences. These constructions have in common, however, a certain ungainliness which most often requires a fixed arrangement in the laboratory.

As a complement to these various types of apparatus, we here present a portable and easily maneuvered prism stereoscope which offers the possibility of varying independently the light intensities in the two picture fields, variable adaptation-light and possibility for continuous or variable exposure time. It also has two geared rotary prisms of large aperture to permit individual adjustment of convergence. The instrument is further adjustable for height and may be tilted to any convenient angle.

Construction. The groundwork of the apparatus comes from the variable prism stereoscope made by Clement Clarke, London. It is united with two identical and entirely separated tubes, one for each eye. These tubes contain a slideholder for 5 × 5 cm. slides (E. Leitz, Wetzlar). Directly connected to the slideholder on the front is a fixed polaroid filter with a white frame resulting in a 4 × 4 cm. picture-area. In front of this white frame is a black frame with a 4.2 × 4.2 cm. aperture. The narrow white frame facilitates a stable eye convergence and accommodation when the picture field is not illuminated. This frame is always illuminated from two lamps situated in the front beside the prisms of the stereoscope. These lamps also serve as sources of the adaptation-light. When the prisms are illuminated from the sides, a certain amount of light intensity is reflected into the eye. The iris will then never be maximally open which would be the case if it was totally dark between different exposures. Thus, the light contrast at the beginning and end of exposure may be reduced. To eliminate light reflection in the polaroid filter from the front lamps, these are framed and only indirect stray light hits the filter. Because the strength of the current is variable, the light intensity of these adaptation-lamps can be adjusted to suit the light intensity of the exposed pictures.

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[1] Edward Engel, Binocular conflict and resolution. Unpublished Doctoral dissertation, Princeton University, 1955, 35-37.

[2] G. C. Grindley, A modified form of Wheatstone's stereoscope, *Quart. J. exp. Psychol.*, 7, 1955, 98-100.

[3] A. M. Adlerstein, The huma scope: A modified stereoscope, *J. Psychol.*, 45, 1958, 109-113.

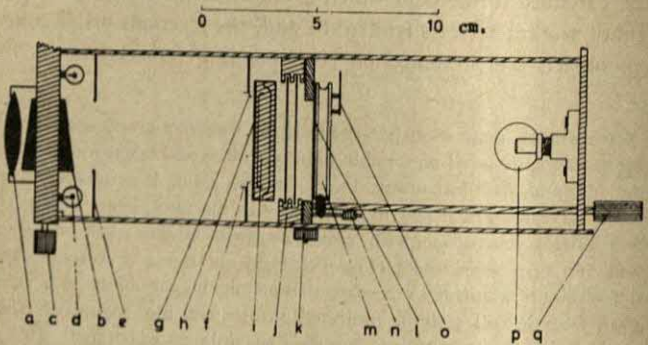

FIG. 1. ONE OF THE TUBES OF THE STEREOSCOPE AS SEEN FROM ABOVE

a—Positive lens;
b—Rotary prisms;
c—Steering knob for rotary prisms;
d—Adaptation lamps;
e—Frame for adaptation light;
f—Black picture frame;
g—White picture frame 2 mm. overlapping);
h—Fixed polaroid filter;
i—Slide holder;
j—Filter cell with two ground glass filters;
k—Screw for side plates;
l—Rotary polaroid filter;
m—Holder for rotary polaroid filters;
n—Attachment for the holder;
o—Pointer showing position of rotary polaroid filter;
p—Main lamp;
q—Steering knob for rotary polaroid filter:

Behind the slideholders are groundglass filters which serve to produce a homogeneous light field from the main lamp, situated at the back of the tube. Between the groundglass filters and the main lamp is a rotary polaroid filter, which is directed from the back of the instrument. There are two steering knobs, one for each tube. Thus, the light intensities in the tubes can be adjusted independently of each other. The position of the rotary filters can be read on protractors on the top of the apparatus.

The stereoscope is constructed with rotary prisms to permit individual adjustment of convergence. Lens cells are fitted in front of the prisms. In these cells positive lenses (+7 D) are placed to magnify the picture field, which then occupies ca. 22° in the visual field.

In Fig. 1, a schematic drawing of one of the tubes, seen from above, is presented to make the description clearer.

The adaptation-lamps (6 V., 5 w.) can be turned on and off by a separate switch. The main lamps (6 V., 2.7 w.) can either be permanently on or can be varied from outside. A built-in transformer provides suitable current. If it is desirable with shorter and well defined exposure-times, the apparatus can be varied from outside with any conventional stimulus-controller.

The apparatus is equipped with a headrest in combination with blinds to prevent disturbing room-light from entering. The slide holders have removable covers, so as not to permit stray light to pass and illuminate the slides behind the fixed polaroid filters. With covers on, experiments can be done in full daylight. In moderate light, the covers can be removed if quick exchange of slides is necessary.

As yet unpublished experimentation with this instrument indicates that it is functional, reliable, and also very easy to handle in the laboratory.

APPARATUS NOTES

A SIMPLE METHOD FOR PHOTOGRAPHING EYE-MOVEMENTS

In the course of studying visual search, we developed a simple method for photographing eye-movements.[1] The novel feature of this technique was a mirror which reflected light from a slide projector onto *S*'s eye. The reflected beam performed three functions: (1) It provided the bright illumination required for photographing the eye; (2) It marked the changing of the projected display onto the photographic record, and (3) It enabled *S* to keep his head-position constant.

The apparatus is diagrammed in Fig. 1. *S*'s left eye was photographed by an electrically driven Bolex 16-mm. reflex movie camera, fitted with a 75-mm. F 1.9 lens on a 15-mm. extension tube. The camera (C) was placed about 35 cm. in front and slightly to the left of *S*'s left eye, so that it did not block either eye's view of the display. Displays were projected by a Kodak Carousel slide-projector (P) and subtended a visual angle of about 30°. The mirror (M) was mounted on the projection surface so that it reflected a beam of light onto *S*'s left eye. The mirror can be placed anywhere outside the area that is scanned, and angled to illuminate the eye. It was placed in the center of the display shown in Fig. 2A, because *S* scanned around the circumference and never crossed the center.

Fig. 2B illustrates the appearance of the illuminated eye. The bright spot on the cornea is an image of the slide projector lens. The direction of *S*'s gaze can be inferred from the position of his iris. This is indicated in Fig. 2B, which was photographed while *S* looked at the top circle on the display. When *S*'s eyes moved during an exposure, the corneal reflection traced a streak on the film. Fig. 2C shows the streak made as *S*'s eye moved from the top circle in Fig. 2A, clockwise to the next circle on the right. When *S* followed a consistent scanning pattern, the Experimenter (*E*) could determine which object he was looking at in displays containing as many as 16 objects arranged in a 4 × 4 array. The corneal reflection was also an aid to precise focusing of the camera: through the reflex viewfinder *E* could see it shrink to a point as the camera reached perfect focus.

When the display changed, the illumination on *S*'s eye dropped sharply and the bright spot disappeared. Fig. 2D shows *S*'s eye during the dark interval between slides. The illumination and corneal reflection returned at the instant that the next slide appeared on the screen. When the trial was terminated by *S*, he closed his eyes to register his response on the film.

There were two reasons why head-movements had to be restricted: one was so the position of *S*'s eye would consistently indicate where he was looking on the display. The other was that the eye had to remain in the field of the camera. The mirror enabled *S* to keep his head in position without the discomfort of mechanical restraints and left him free to speak. Only when *S*'s eye was correctly positioned in the reflected beam could he see the lens of the slide projector in the mirror. This positioning was accomplished just before each slide was presented. While searching, *S* did not have to look at the mirror to maintain head position, since displays less than 30° in diameter are scanned almost entirely by eye movements.

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[1] This research was supported by Public Health Service Research Grant MH 08164 from the Institute of Mental Health.

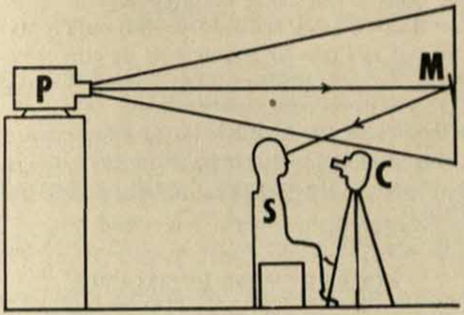

FIG. 1. SCHEMATIC DESIGN OF THE APPARATUS
P, slide-propector; M, mirror at center of display, reflecting beam of light onto *S*'s left eye; *S*, subject; and C moving picture camera.

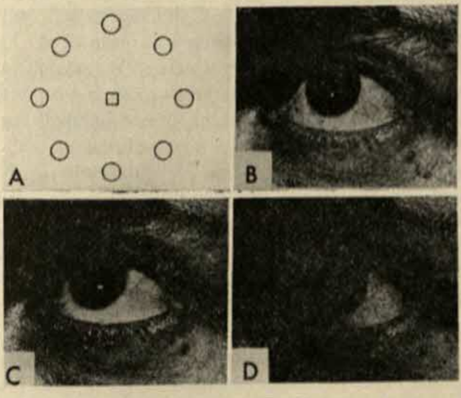

FIG. 2. POSITION OF THE DISPLAYS AND PHOTOGRAPHS OF *S*'S EYE
(A) Eight-object display with mirror in the center. (B) Photograph showing corneal reflection. (C) Corneal reflection during movement. (D) Eye during the dark interval between slides.

The photographs in Fig. 2 were taken on Tri-X film at 13.6 frames per sec. The camera speed was calibrated by photographing a clock at the beginning and end of the reel. Location and duration of fixations were determined by examining the films frame by frame with a motion-analysis projector.

New York University
Medical Center

IRA T. KAPLAN
WILLIAM METLAY

AIR BLAST: AN EFFECTIVE UNCONDITIONED STIMULUS (*US*)

Pressurized air released into a shuttle-avoidance apparatus will sustain acquisition of a conditioned avoidance response. Response to the blast is immediate, directional (away from the source), and relatively unemotional. With a *CS-US* interval of 8 or 11 sec., learning is rapid; a criterion of five avoidances in six trials is commonly attained after fewer than 10 trials, and most animals acquire the response. More than 20% of 60 normal rats thus far trained with air blast achieved the criterion immediately following the first avoidance. Once learned, the response is reliably elicited by the *CS* (buzzer) over many extinction-trials.

The efficacy of this *US* in the shuttlebox is not independent of the apparatus, which should be short (30 in.) and narrow (4 in. to 5 in.). Air is introduced horizontally from the end of the apparatus through shower-nozzles at a pressure of about 30 lb./sq. in.

North Dakota State University A. JOSEPH RAY, JR.

ACCURACY OF ESTIMATES

The data from perceptual studies in which estimations of physical dimensions of experimental stimuli are made can be examined in terms of two interdependent analyses. One kind of analysis is a comparison of the magnitude of the estimate means. This is usually accomplished by use of the *t* or *F* statistic. A second kind of analysis is in terms of error of estimates, in which: (a) error is considered to be reflected by a measure of dispersion around the mean, such as the variance or standard deviation, or (b) where error is defined in terms of the absolute deviation of estimates from the physical or true measurement of the stimulus. This latter error measure is arithmetically rather than algebraically computed so that the sign of the direction of the deviation is ignored. (If sign is not ignored, one is merely subtracting a constant, the true magnitude of the stimulus, from each estimate.) The usual form of this deviation is $/X_e-X_t/$, where X_e is the estimate of the stimulus and X_t is the true magnitude. In many studies, these deviations, or percentages or means derived from them, form the values of an analysis using a statistical technique which assumes that sampling is from a population that is normally distributed. The deviations from absolute value present, however, a 'half-normal' or one-sided distribution, since the algebraic sign is ignored. Hence a basic assumption is violated in these conditions. Even a very liberal interpretation of the 'robustness' of the assumption of normality cannot accommodate such usage. Either the data must be transformed to obtain distributional normality or the standard deviation must serve as a measure of over-all error. In any event, caution must be exercised when dealing with deviations computed without regard to algebraic sign.

Rutgers University H. R. SCHIFFMAN

NOTES AND DISCUSSIONS

THE PREROLANDIC CORTEX: ITS FUNCTION

It is generally believed and taught today that behavior activities of the striated muscular system in man and higher animals are governed by and emanate from corresponding activities in specific sections of the prerolandic motor cortex. Textbooks commonly picture an inverted figure with feet and hands in upper sections of the brain near the longitudinal fissure and with head regions lower and lateral. Our tests suggest that the exceptions to this rule greatly outnumber the instances that follow it.

As early as 1888, Mills depicted the "mosaic" of the prerolandic motor cortex supposed to govern voluntary movements which has become the general conception[1] J. F. Fulton presented the history of this common belief which he evidently shared.[2] As affording confirmatory evidence on animals, he cited findings and interpretations of Boyle (1691), Jackson (1864, 1870), Fritz and Hitzig (1870), Beevor and Horsley (1890), Ferrier (1873, 1875), Leyton (Grunbaum) and Sherrington (1917), the Vogts (1926) and Foerster (1936).[3] Affording confirmatory evidence on the human cortex, Fulton cited Bartholow (1874), Sciamanna (1882), Horsley (1887), Keen (1888), Nancrede (1888), Beevor and Horsley (1890), Bidwell and Sherrington (1893), and others.[4]

In 1951, F. M. Walshe, editor of *Brain,* protested that charts of brain motor localization are falsely assumed to "represent the essential plan of cortical organization." Instead, "they are simply a way of showing on a diagram what a certain type of experiment (*i.e.* electrical stimulation of specific localities) yields, and to regard them as more is without justification."[5]

According to Walshe, it had not been proved that the prerolandic mosaic, established by electrical stimulation and other evidence, is really the motor keyboard of centers of cortical control of skeletal muscle performance in the natural life of intact animals and man. Those who have believed that this has been proved "have failed to realize that what they observe is merely what the specific experimental operation reveals, and that it stands in no necessary or apparent relation to the normal processes of the structures stimulated."[6] Doubts and questions, at least somewhat similar, he indicated, were shared by Ferrier, Hughlings Jackson, Beevor, Denny-Brown, Liddell and Phillips, Lashley and Clark, Leyton and Sherrington and Dusser De Barenne.

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[1] C. K. Mills, *Brain,* 12, 1890, 233-358.

[2] J. F. Fulton, *Physiology of the Nervous System,* (2nd ed.) 1943, 369-393.

[3] R. Boyle, *Experimenta et observationes physicae,* (John Taylor, London, 1691), (26), 158; J. H. Jackson, *Lond. Hosp. Reports,* 1864, 388-471; *Trans. St. Andrews Med. Grad. Ass.,* 3, 1870, 162-207; G. Fritsch and E. Hitzig, *Arch. Anat. Physiol. Swiss. Med.,* 37, 1870, 300-332; C. E. Beevor and V. Horsley, *Philos. Trans.,* 181B, 1890, 49-88; D. Ferrier, *West Riding Lunatic Asylum med. Rep.,* 3, 1873, 1-50; *Philos. Trans.,* 165, 1875, 433-488; A. S. F. Leyton and C. S. Sherrington, *Quart. J. exp. Physiol.,* 11, 1917, 135-222; C. Vogt and O. Vogt, *Naturwissenschaften* 14, 1926, 1190-1194; O. Foerster, *Handb. Neurol.,* 6, 1936, 1-448.

[4] R. Bartholow, *Amer. J. med. Sci.,* 67, 1874, 305-313; E. Sciamanna, *Arch psichiat. Torino,* 3, 1882, 209-215; V. Horsley, *Amer. J. med. Sci.,* 93, 1887, 342-369; W. W. Keen, *Amer. J. med. Sci.,* 96, 1888, 329-357; 452-465; C. B. Nancrede, *Two Successful Cases of Brain Surgery* (Philadelphia, 1888); C. E. Beevor and V. Horsley, *Philos. Trans.,* 181B 1890; 129-158; L. A. Bidwell and C. S. Sherrington, *Brit. Med. J.,* 2, 1893, 988-989.

[5] F. M. R. Walshe, *Brain,* 74, 1951, 249-266.

Upon consideration of the findings and interpretations on both sides as indicated above, there appears to be as yet no utterly conclusive evidence. While there have been positive results at times (*e.g.* Penfield, 1949)[7] as yet no *experimentum crucius* has been performed. The following study is an attempt to answer the problem directly.

Methods: Subjects. Eighteen men and women, college students and office workers, were the initial *S*s. As a rule each *S* was tested for 1-2 hr. on four or five different days. The *S*s lay with eyes closed in an electrically shielded, semi-soundproofed darkened room. For EMG recordings the integrating neurovoltmeter was employed.[8] This apparatus is stable and records transient potentials accurately to a small fraction of a microvolt. We have ascertained that surface electrodes of certain size permit recording quite as accurately as platinum-iridium pin electrodes inserted directly into the muscle. The platinum-iridium surface electrodes used were 38 mm. by 63 mm. in size. They were fastened to the skin over the appropriate muscle with Scotch tape and separated from the skin by a thin layer of electrode paste (Sanborn's Redux). The EMG recordings were bipolar, with adjacent edges of electrodes at least 20 mm. apart. *S* was grounded.

Electrodes were fastened to the scalp by collodion reinforced with a thin flattened wad of cotton batting.

ECG (EEG) recordings were monopolar and were performed by standard methods. The positive electrode was placed approximately over the location of the cortical "center" corresponding to the muscles which flex or extend the hand and fingers. This location was determined by Kronlein's method, but allowance was made for discrepancies by placing several additional electrodes close to the estimated location. Furthermore, upon test, we found, that the patterns recorded from these adjacent electrodes were in the main quite similar to each other.

For ECG, only the preamplifiers, Type 481, with the Type 9806-A-C coupler of the Offiner Dynograph were used. Their outputs were connected with the inputs of band-pass filters (specially built by the Instrumentation and Controls System Co.) with adjustable cut-off frequencies. The output of these filters was photographed by means of an 8-channel D.C. recording oscilloscope with concomitant photo development. Also the output was fed into an amplifier and phase splitter provided with a bridge output, then through a resistance-capacitance integration-circuit. Thus the ECG is continuously integrated for selected frequency ranges.

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[6] *Ibid*, 265.

[7] H. Jasper and W. Penfield, *Archiv, für Psychiatrie und Zeitschrift Neurologie*, Bd. 183, 1949, S 163-164.

[8] Edmund Jacobson, *The Revue of Scientific Instruments*, 11, 1940, 415-418.

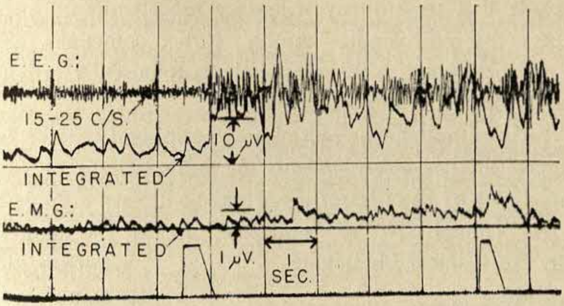

FIG. 1A. INCREASE OF PREROLANDIC POTENTIALS (EEG-ECG) IN THE REGION OF THE HAND BEFORE AND SIMULTANEOUSLY WITH THE INCREASE OF THE POTENTIAL OF THE FLEXOR MUSCLE OF THE HAND

Recorded directly in the upper curve and integrated in the second and third curves.

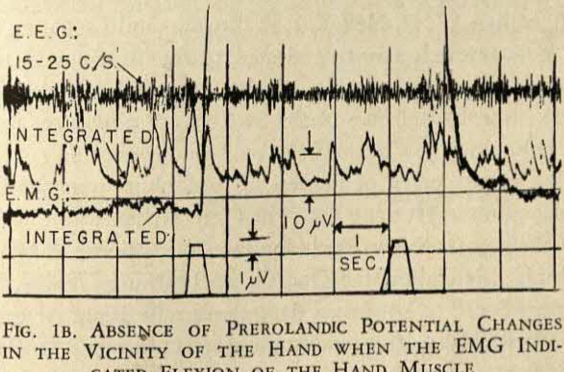

FIG. 1B. ABSENCE OF PREROLANDIC POTENTIAL CHANGES IN THE VICINITY OF THE HAND WHEN THE EMG INDICATED FLEXION OF THE HAND MUSCLE

According to programming in any one session, upon a sound signal *S* bent one hand back at the wrist or clenched the fist or squeezed a tennis ball. He was to relax promptly upon the second sound-signal. Four of the 18 *S*s who had been trained to relax, and were checked by action potential measurements.[9]

Results. In most cases specific activity of motor cortex ECG was not identified at the time of specific muscle contraction; namely, of muscles which flex or extend the fingers or the hand. Only in a minor fraction of the tests were the results positive; namely, only 97 out of 629 instances.[10]

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[9] Jacobson, *Progressive Relaxation,* 2nd ed., 1938, 309-326.

Fig. 1. presents an example of a positive and of a negative test.

We conclude that in ordinary voluntary movements of intact healthy human beings the prerolandic "motor" cortex sometimes acts electrographically to correspond with peripheral neuromuscular activity, but far from always. Indeed it is almost exceptional.

Further crucial simultaneous central and peripheral tests of function of "motor centers" in laboratory animals are indicated. In preparation is a continuation report of the present investigation in cats with electrodes implanted in the "frontal paw" or "hind paw" area of the sigmoid gyrus.

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EDMUND JACOBSON
RICHARD E. LANGE

THE 1966 MEETING OF THE NATIONAL ACADEMY OF SCIENCES

The 103rd Annual Meeting of the National Academy of Sciences was held at the building of the Academy in Washington on April 28, 29 and 30, 1966. Of the present twenty-seven American members of the Section of Psychology of the Academy, the following were present: Robert Galambos, W. R. Garner, J. P. Guilford, Heinrich Klüver, D. B. Lindsley, W. R. Miles, N. E. Miller, W. D. Neff, Carl Pfaffmann, and Leonard Carmichael. Of the 42 new research scientists elected to the Academy this year, one was a psychologist, Floyd Ratliff, of the Rockefeller University. N. E. Miller is the present Chairman of the Sections. A dinner for psychologist members was held at the Cosmos Club on Monday evening.

Among the papers given of interest to psychology were the following: "Visual Orientation in Homing Bats," by D. R. Griffin with T. C. Williams and J. M. Williams; "New Psychological Conceptions of Memory," by J. P. Guilford; "Extending the Domain of Learning," by N. E. Miller; "The Olfactory Sense and Infrared Radiation in the Large American Cockroach," by W. R. Miles. A symposium on recent advances in the study of insect behavior was also of real importance for psychologists.

The Public Welfare Medal, one of the leading awards of the Academy, was given to John W. Gardner, a distinguished psychologist, who is now Secretary of the Department of Health, Education, and Welfare.

National Geographic Society
Washington, D.C.

LEONARD CARMICHAEL

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[10] Reported in part at the XXIII International Congress of Physiological Sciences in Tokyo, Japan, September, 1965.

THE 1966 MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY

The Annual General Meeting of the American Philosophical Society was held at the house of the Society in Independence Square, Philadelphia, April 21, 22 and 23, 1966.

No psychologist was among the twenty new members elected. The twelve American psychologists who are members are F. A. Beach, E. G. Boring, Leonard Carmichael, C. H. Graham, H. F. Harlow, Heinrich Klüver, Wolfgang Köhler, W. R. Miles, Carl Pfaffmann, C. P. Richter, B. F. Skinner, and S. S. Stevens. It is interesting that all psychologists who are members of the Philosophical Society are also members of the National Academy of Sciences. For a number of years no psychologist has been elected to the Philosophical Society who was not at the time of his election already a member of the National Academy of Sciences.

E. G. Boring is Chairman of the Committee on Publications. He reported on the extensive publication program of the Society. The issues of the Transactions, the Proceedings and other publications of the Society have been well received. The number of subscriptions continues to increase.

The Committee on Research, of which Leonard Carmichael is a member, made during the year approximately 350 grants of money for specific projects. A number of these were in psychology.

The total invested funds of the Society are now in excess of $25,000,000.

The Karl Spencer Lashley prize for "useful and significant work" in the field of "neurobiology" was awarded to Hans Teuber.

The National Geographic Society LEONARD CARMICHAEL
Washington, D.C.

John Edward Anderson: 1893–1966

John Edward Anderson died at the age of almost seventy-three years in Chattanooga, Tennessee, May 10, 1966 after a long, debilitating illness. Born in Laramie, Wyoming, June 13, 1893, he took his baccalaureate work at the University of Wyoming and earned his advanced degrees in psychology at Harvard. He completed the doctorate in time to enter the military service early in World War I and became head instructor of the School of Military Psychology at Camp Greenleaf in Georgia. During that time, he married Dorothea Lynde of Chattanooga, Tennessee. From military service, he went to Yale, where he taught from 1919 to 1925. During this period he served as Secretary of the American Psychological Association, the first of the many significant offices he was to fill for his profession.

In 1925 he was offered a position at the University of Minnesota as Director of its newly established Institute of Child Welfare, one of six such institutes financed by the Laura Spelman Rockefeller Memorial Fund. This position he held for thirty years. He retired as professor from the University of Minnesota in 1961, still full of energy and plans for continuing his work, but steadily failing in health.

For his entire mature life Anderson was an active contributor to the science and profession of psychology, as well as a vigorous individualist in higher education, for he deplored all trends toward the professor as entrepreneur in the academic marketplace.

In his numerous papers, in his many contributed chapters to handbooks, and in his seven books, he made his primary contribution. As an editor of books and of the *Psychological Bulletin* from 1942 to 1946, he gave direction to the scientific writing of others. with an editorial pencil incisive, sure, and frequent.

His work ranged over surveys of parent education, papers on language and social development and theoretical contributions on the study of mental growth. Appropriate for the new science of developmental psychology many of his research papers were descriptive, normative studies, though he remained sensitive to the power of the experiment.

What was probably his most important theoretical paper appeared in in 1939 in the *Journal of Psychology*. Dealing with the overlap hypothesis, it showed how uncorrelated increments in growth may nevertheless contribute to a highly predictable end-status. The paper provided a rational basis for many phenomena observed in longitudinal studies of mental growth and constitutes one of two or three principal theoretical contributions to the mathematical expression of the phenomena of growth. Anderson also contributed a number of important surveys and critical reviews of the literature. His chapter on the methodology of child psychology in Carmichael's two *Manuals of Child Psychology* was long a classic.

As Director he established Minnesota's Institute of Child Welfare as a solid contributor to research and gave significant direction to the study of child psychology in this country for more than three decades. His work for the 1930 White House Conference on Child Health and Protection as Chairman of the Conference Committee on the Infant and Pre-school Child led to the most comprehensive program of normative studies on the status and welfare of young children conducted in this country. Without the data collected by this Conference and published in some twenty volumes, the marked progress in social welfare, health care, and educational facilities for young children in the 1930's could not have occurred so rapidly. He wrote two volumes in this series, *Nursery Education* (1931) and *The Young Child in the Home* (1935). With his colleague, Florence Goodenough, he developed the first empirically validated scale in social and child psychology for assessing socio-economic status, The Minnesota Occupational Scale, a device for insuring representativeness in sampling for normative studies. He was also a member of the 1940 and 1950 White House Conferences on Children and contributed a background paper for the 1960 Conference.

John Anderson was a dynamic, exciting teacher, capable of making ideas arresting. His students remember his many apt, aphoristic remarks. This quality made him sought after as a speaker, an activity for which he found time in a busy professional and personal life because he firmly believed in the importance of interpreting knowledge. He was also a contributor to the general education movement. His course of lectures in the early 1930's in Minnesota's General College later appeared as *The Psychology of Development and Personal Adjustment* (1949), an excellent example of cross-disciplinary, integrative, interpretative writing.

It was, however, as an officer of professional and scientific societies and a member of state and national boards and commissions that John Anderson had his most marked impact on his profession as well as on public life. He was President of the American Psychological Association in 1942-1943 at a critical time, for the Association was simultaneously caught up in World War II and in the resolution of the first major confrontation with the issue of the profesional vis-à-vis, the scientist, a dilemma which led to the synthetic reorganization of the association in 1945. He also served the National Congress of Parents and Teachers, being for five years Chairman of its Committee on the Exceptional Child, and he was Vice-President of the state of Minnesota's Congress for two different three-year terms. At different times he was Vice-President of the American Association for the Advancement of Science (Section I, Psychology), President of the APA's division on developmental psychology and its division on maturity and old age, and president of the Society for Research in Child Development.

Like so many others in the movement concerned with child development, he shifted his interest from the behavior of young children to the study of development as process, and then to a concern with these processes in the later years of life. He edited the *Psychological Aspects of Aging* (1956) for the American Psychological Association, was a member of the National Conference on Aging (1960) and the National Advisory Committee of the 1961 White House Conference on Aging, and was Chairman of its

Planning Committee on Research in Gerontology in the Psychological and Social Sciences (1961). He was also a member of the Minnesota Commission for Aging for five years.

John Anderson had a keen sense of the requirements of university life. He saw the position of professor in the state university as an office of public trust, with distinctive responsibilities. Academic freedom was for him no academic issue merely to be defended on principle; it was a right earned by unswerving honor paid to these responsibilities. On all debatable issues his position was made abundantly clear.

Administrator and scholar, teacher of university student and layman, university citizen and man of public and professional responsibilities, John E. Anderson filled all these roles energetically and with distinction. Developmental psychology today is thriving because a handful of men gave it life and careful nurture. Prominent among them was John E. Anderson.

The Pennsylvania State University DALE B. HARRIS

BOOK REVIEWS

Edited by T. A. RYAN, Cornell University

The Exercise of Influence in Small Groups. By TERENCE K. HOPKINS. Totowa, New Jersey, Bedminster Press, 1964. Pp. viii, 205. $6.50.

The Exercise of Influence in Small Groups is itself an important exercise—an exercise in theory construction. Because it is one of the finest efforts of its kind in the literature to date, it warrants a more detailed and critical examination than is usually the case in reviews. The work represents an addition to the small but growing number of attempts to set forth systems of propositions in the social and psychological sciences which endeavor to employ one of several axiomatic models. In this case, Hopkins presents fifteen two-variable propositions asserting, at least at first, a symmetric relationship between five ordinally scalable properties of small group membership-status: "influence," "rank," "centrality," "obvervability," and "conformity." Six of these propositions are purportedly derived by application of the axiomatic schema advanced by Zetterberg. Indeed, Zetterberg himself, cites this effort as "an acceptable scientific theory" and, therefore, he presumably considers it an adequate application of his model (H. L. Zetterberg, *On Theory and Verification in Sociology,* 1965, 15).

After a brief introduction, Hopkins defines the concepts, the units of analysis and properties, which are to be the components of the elementary theoretical propositions of his theory. The unit of analysis is the "small group," which is defined as that sub-class of "groups" high in observability potential, high in face-to-face interaction, and possessing no sub-groups. A "group" is defined as an aggregate of people having a relatively high degree of normative consensus, interaction density, compliance to normative expectations, member and non-member consensus on membership status, visibility of norm-relevant behaviors, and stability in status-ranking. Together, these nine properties constitute a range definition of "small group" and demarcate the theoretical domain. As such, they may be viewed as a set of limiting conditions or givens for the propositions which are advanced later. The empirical, theoretical and formal problems which these complex unit-characterizing properties present for the validation of Hopkins' concepts of membership-status and the verification of his propositions are crucial to any rational assessment of his theory. Yet only scant attention is given to them. Indeed, none of the studies called upon in support of the propositions of his theory report measures on all of these properties, only a few report measures on any of them, and only rarely is an allusion made to the possibility that a case may not fulfill the requirements. At best, this range definition of a small group sensitizes the researcher to examples of human aggregation which are to be excluded and included, but its formulation leaves an enormously large zone of indeterminacy for the selection of cases. Minimal empirical adequacy would require at least one comparative limit-setting measure for each of these properties, which Hopkins acknowledges do not exist at this time.

Within the "small group" an "interacting system" is characterized in terms of five membership-status properties. These properties are: "rank," a member's general prestige in the group relative to matters of group functioning; "centrality," the frequency-range rate of a member's interaction with other members of the group; "observability," a member's knowledge of group norms; "conformity," a member's agreement with group norms; and "influence," a member's ability to change or maintain the norms and norm-relevant behavior patterns of the group. By definition, all other properties of the "small group," fall into a residual class constituting the "environment" of the "ineraction system." In light of the conceptual and operational problems the membership-status properties raise and considerable evidence from small group studies, the assumed relative systemic autonomy of these properties appears premature. Many of these difficulties are appreciated by Hopkins and receive special attention in short appendices and in later chapters of the book. Among these problems for example, is the possible multidimensional and uncrystallized character of "rank" in small groups, the difficulties in summating frequency and range indices of interaction into a single "centrality" measure, and finally, the exclusion from the system of such major factors as the "rank of other," the "content of interaction," and the like. Most problematic of all is the validational status of Hopkins' focal variable, "influence." As the author acknowledges, in no study cited is "influence" measured in a manner consistent with his formulation of the property. By implication, in the absence of direct empirical evidence, support for his theory of "influence" rests solely upon its intuitive and heuristic significance and the logical and empirical merits of the propositional system in which it is embedded.

The nature of the relations alleged to hold between properties in the propositions is rendered problematic from the very onset by the presentation of diagrams showing one-way and two-way arrows suggesting both assymmetry and symmetry. Nevertheless, in Chapter 3 the author states 15 propositions having the form >X, >Y, and we are explicitly told they are positive, symmetric, and therefore, presumably static. Statistically, they may be represented simply by high correlations. Following Zetterberg, we are told that a full twenty propositions may be generated, but Hopkins does not bother with five "because they are not particularly interesting" (p. 52). This is a curious remark in view of the fact that, assuming Hopkins' logic to be sound, one can deductively generate all of the other 16 from only 4 of these "uninteresting" propositions, and the falsification of any one of them would render the whole system untenable. In point of fact, the logical validity of his derivations is suspect. As Costner and Leik "Deductions from 'Axiomatic Theory,'" (*ASR* 29, 1964, 819-835) have shown, the deductive procedure in this work when statistically formulated assumes the "sign rule" and is invalid if the association is less than unity. This the author does not claim for his variables. On the other hand, Hopkins does assume a relatively closed system and translates these symmetric relations into causal, assymmetric form by implication at least when he states the propositions in "sequences" taking the form >X leads to >Y. The consequent ambiguity as to the nature of the relationship(s?) renders the logical status of the derivations in the theory difficult to assess. Unfortunately, these formal problems do not appear to have received the detailed attention they require. In this connection, neither the concepts nor the propositions of the study are presented in a truly axiomatic form. While propositions are labeled "basic" and "derived," the rationale given for their ordering is unconvincing to this reviewer. We are not told that the "basic" propositions constitute a minimum set ordered either in terms of their verificational status or relationship to the focal property "influence," but merely that they are "substantively important" (p. 52). If the systemic assumption is correct, all of the propositions are equally substantively important. The evidence supporting each of the 15 propositions is rated by Hopkins as "good," "some," and "none" (p. 97). Others might find some cases in which the evidence is a bit over-stretched and a few theoretically critical cases involving his focal variable, "influence," in which Hopkins can not find any evidence at all.

In the next two chapters, Hopkins discusses three interesting applications of the theory. The first concerns an analysis of leadership using descriptive data taken from Whyte's *Street Corner Society*. The second draws upon the propositions of the theory, now in temporal sequences, to describe the distribution of "influence" in a study conducted by the author. Drawing upon a broad range of sources, the third and most important application describes a "rank-influence process" which is seen as contributing to the conjunctivity of "rank" and "influence" in small groups. While it is not possible on the basis of these applications to draw any conclusions as to the verificational status of the theory which informs them, they do provide several insightful leads and refinements that would repay systematic research.

Despite the fact that the principal conclusion of this reviewer must be that this ambitious little book does not present a minimally adequate theory of group influence, it does advance a frame of reference worthy of serious study by students of theory construction and small groups alike. Those of us who share with Hopkins a concern for the development of theories in our adolescent disciplines cannot but be encouraged by his contribution. I for one look forward to the future empirical testing and formal refinement of Hopkins' proto-theory.

State University of New York at Binghamton SAMUEL F. SAMPSON

Mach Bands: Quantitative Studies on Neural Networks in the Retina. By FLOYD RATLIFF. San Fracisco, Holden-Day, Inc. Pp. xii, 365. $11.75.

Floyd Ratliff has given us an insightful, systematic, and at the same time, exciting account of the brightness contrast effect now known as Mach bands. These perceived light or dark contours are related to certain spatial distributions of illumination, but they are completely 'unexpected' in terms of conventional physical measures of the light distributions which elicit them. Part I, the major portion of the volume, is entitled "On neural networks and the problem of objectivity" and here Ratliff presents his own integrative review and discussion of the phenomenon and related topics; in Part II entitled "Mach's papers on the interdependence of retinal points," Ratliff provides us with excellent (and long overdue) translations of six of Mach's original German papers, five published between the years 1865-1868 and the sixth in 1906. We ourselves have long considered Mach's papers to be brilliant classics and can think of nothing more complimentary to say about Ratliff's Part I than that, face-to-face, or back-to-back with the original papers, it holds its own as a remarkable updating of Mach's work.

A reviewer is hard pressed to improve on Ratliff's own overview and summary that he gives in an introductory section. Chapter 1 presents a brief sketch of Mach's life and work, and "emphasizes the origin, development, and eventual influence of his view of science and epistomology." In discussing Mach's breadth of interests, in physics, sensory phenomena and their interpretations, and philosophy of science, Ratliff conveys his own admiration for the man and his point of view. To our dis-appointment, he does not here relate Mach's thinking about sensory problems to any of his contemporaries with whom his views were in sympathy. In a later chapter concerned with the interpretation of Mach bands, Ratliff says "He dismissed as meaningless all of the then current explanations such as 'It results from an unconscious inference,' or 'It must be only an illusion'," (p. 98). Statements of this sort tend to place Mach alone in his time, which is historically misleading. Mach and Hering were contemporaries and colleagues at the German University at Prague for 25 years from 1870 to 1895, at which time Mach departed for Vienna and Hering went to Leipzig. It was against "unconscious inference," as expounded primarily by Helmholtz, that Hering argued in his own insistence on reciprocal and mutually opposed physiological processes of neural interaction to account for color and brightness contrast phenomena.

Chapter 2 summarizes Mach's original psychophysical investigations of the bands which bear his name, and discusses methods for their production and psychophysical measures of the effect. A number of the more recent psychophysical studies are reviewed in detail, and discrepancies and unresolved issues are pointed out. One such issue is that of color effects. Here Ratliff seems to accept the conclusions of later investigators who believe that luminance differences are necessary for the production of Mach bands, although we ourselves can confirm Mach's original observations which led him to state "I have repeatedly stressed that the theory is also applicable to the phenomenon of color contrast" (p. 305) and "That the law holds for any combination of colors, I am equally convinced" (p. 315).

Chapter 3 is especially impressive. In it Ratliff reviews in rigorous mathematical detail and with great expository clarity six of the mathematical models, including Mach's own, which have been proposed to explain Mach bands and similar contrast effects. Having outlined each model and categorized it as stimulus- or response-dependent, linear or non-linear, Ratliff points out the common conceptual principle on which they are all based. "Reduced to their bare essentials, the six models are really one and the same, since the same basic neural processes—excitatory influences opposed by surrounding inhibitory influences—merely masquerade in slightly different mathematical forms in each of the several models" (p. 120). It seems clear that any attempt to handle contrast effects at all must be based on this common principle. Our own model, which is a response-dependent one, non-linearly related to stimulus-inputs in three variables, applies the same principle to three response variables in order to encompass both color and brightness contrast and constancy phenomena.

Ratliff's "words of caution" at the end of this chapter are significant ones. He emphasizes that models narrowly based on the Mach band alone are unlikely to be correct, and we agree with him that opposed excitatory and inhibitory influences are so far reaching in vision that the more broadly comprehensive a model is, "the fuller will be our understanding of both the nature and significance of integrative processes in the retina and elsewhere in the nervous system" (p. 141).

Chapter 4 entitled "The functional significance of inhibitory interaction" reviews modern electrophysiological studies of inhibitory interaction in the retina with emphasis on the *Limulus* retina and the work of Hartline, Ratliff, and their associates. The evidence for inhibitory interaction and its role as a sharpening or crispening mechanism to accentuate visual contours are discussed in illuminating detail. Moreover, Ratliff takes advantage of the *Limulus* interaction material to lead the reader through a meaningful discussion of a variety of highly complex patterns of vertebrate neural responses to particular spatial, spectral and temporal patterns of illumination.

In Chapter 5 "Appearance and reality," the relevance of the Mach band is brought to bear on the broad distinction between properties of the observer's senses and properties of the object observed. Here three intriguing examples from the history of science point up the importance of the distinction, and alert the reader to comparable problems that arise in the use of instrumental aids to observation. The chapter concludes with a general discussion of "knowledge" and "objectivity."

Mach's own papers are a beautiful example of scientific method. He observed the contour phenomenon and, since it did not reflect the spatial distribution of the light-stimulus directly, he sought, and found, a description of that aspect of the stimulus to which it could be related; namely, the second derivative of the spatial light distribution. Since he had observed it first on rotating disks, he ruled out its dependence on stimulus-motion by photographing the disk and observing the photograph. To determine its dependence on eye-movements, he observed it with instantaneous spark-discharge illumination. Convinced that the phenomenon depended on opposed interactions among physiological elements in the retina, he developed a model to describe these interactions. It is of more than passing interest that Mach's own model of 1868 actually describes correctly more aspects of the Mach band than do most of the other more recent mathematical accounts.

We think this is an important volume that will appeal to many non-specialists as well as being a must for specialists in the area.

University of Pennsylvania DOROTHEA JAMESON and LEO M. HURVICH

Patriotism and Nationalism: Their Psychological Foundations. By LEONARD W. DOOB. New Haven, Yale Univ. Press, 1964. Pp. xiii, 297. $6.75.

In this book Professor Doob has covered a large amount of factual material (from psychologists such as Frenkel-Brunswik, Buchanan and Cantril, Hartley, Pettigrew; from representatives of other disciplines such as Almond and Verba, Stouffer, Quincy Wright, and James C. King; and especially from his own studies in Africa and in South Tyrol). He has used this material as a basis for a theoretical picture of the psychological factors promoting patriotism and nationalism, and, much more briefly, he has brought this picture to bear upon the problems of building an international society in which international loyalty may take the place of both patriotism and nationalism.

There are few social psychologists today who would disagree with the author's statement that "nationalism—as well as patriotism— is related to war, and war is actually and potentially so destructive that it bestows pervasive significance upon any related topic." From this standpoint perhaps the most useful part of the book is the group of three chapters (10-12) in which the author first outlines 23 typical "justifications" of nationalistic actions or demands, and then uses them as categories for a kind of content-analysis of a number of statements about war aims, including declarations of war by Hitler, Ribbentrop, Mussolini, the Japanese Emperior, Daladier, Chamberlain, Molotov, and Roosevelt. While the classificational scheme can be questioned at a number of points it has the virtue of realistic complexity. Many readers will want to condense it, but those who study it carefully will realize that they can hardly condense it much without sacrificing realism in depicting a reality that is itself highly differentiated and complex. The application of it to statements of war-aims in World War II is fascinating. Among other things, it documents and pins down the not-too-surprising but basic generalization that every major belligerent in World War II (as North and others have shown with regard to World War I) proclaimed a love of peace and described the war as forced upon it by the diabolical acts of its enemies. The Japanese Emperor, for instance, said "It has been truly unavoidable and far from Our wishes that Our Empire has now been brought to cross swords with America and Britain . . ."

The extent to which these public statements were believed by those who made them is one of the deeper questions that the author does not systematically discuss. In fact this reviewer felt a need for clearer and more thorough discussion of a number of basic issues, especially those centering around the definitions assigned by the author to the two central terms, "patriotism" and "nationalism."

As he defines it, "patriotism" is love of country, while "nationalism" consists chiefly of the "demands" that a person who loves his country may make in its behalf. His formal definitions (p. 6) are:

> "Patriotism: the more or less conscious conviction of a person that his own welfare and that of the significant groups to which he belongs are dependent upon the preservation or expansion (or both) of the power and culture of his society.
>
> "Nationalism: the set of more or less uniform demands (1) which people in a society share, (2) which arise from their patriotism, (3) for which justifications exist and can be readily expressed, (4) which incline them to make personal sacrifices in behalf of their government's claims, and (5) which may or may not lead to appropriate action."

The relationships between love, identification, and the rational pursuit of individual "welfare" are not really clarified, here or elsewhere.

There is nothing, moreover, in these definitions that brings out the strongly honorific connotations that nearly always attach to the term "patriotism" in contrast with the derogatory connotations that often attach to the term "nationalism." If this were a merely semantic question it would not be important. Any writer should have the privilege of defining his terms in any way he likes, even though it may depart from common usage, providing that his own definitions are clear; and Doob's definitions are at least fairly clear. But it can be argued that his ignoring of the positive and negative connotations normally associated with these words has led him to ignore or minimize at the same time a psychological problem of central importance: is it possible to keep what is "good" in patriotism (loyalty, let us say, or willingness to make individual sacrifices for the common good) and at the same time to eliminate or minimize what is "bad" in nationalism (*e.g.* ethnocentric prejudice, the tendency to see certain other countries as diabolical, or an excessive concern with one nation's power, prestige, and absolute sovereignty)?

This is not a merely semantic question, nor can it be avoided by calling it a question of personal values and therefore not a proper subject for scientific investigation. The reality ordinarily denoted by the term "loyalty" is simply not the same as the reality ordinarily denoted by the term "ethnocentric prejudice," even though they may be closely related. Unless they are distinguished we are not likely to get very far in either our psychological analysis or our practical efforts to avoid war in a world in which nationalism is, as Doob says, perhaps "the most important problem of this century."

George Washington University RALPH K. WHITE

The Explanation of Behavior. By CHARLES TAYLOR. New York, The Humanities Press, 1964. Pp. x, 278. $6.75.

It is a human failing to tire of those ideas that we can neither refute nor accept. Because most of the ideas in what we call "learning theory" are too vague to be contradicted and too contrived to be plausible, it is difficult to think about them long enough to find out what exactly is wrong with them. Many tell us to forget them and to find some better ideas to put in their place; but such advice is impertinent. Until we understand exactly what is wrong with the old ideas, how can we hope to recognize, let alone discover, new ideas that are better? However, when this advice is embellished with general remarks about scientific theories, the history of science, and with a quotation or two from Conant or Bergmann, many readers seem willing to accept such spiritual uplift in place of practical advice. It is always comforting to think that our failure to clear up our fuzzy ideas reflects a broad cosmic failure of scientific theory in general. If the physicist can tolerate the indeterminancies of quantum-mechanics, what right have we to complain about the fuzzy indeterminancy of our own theoretical ideas?

Charles Taylor's book, *The Explanation of Behavior,* is directed to those readers who reject such spiritual uplift, and who want instead to know exactly what is wrong with these old ideas. Taylor, a philosopher by trade, has made it his business to study the experimental literature of animal learning, and he believes that a careful examination of all the arguments of all the S-R theorists and of the behavioral evidence, shows that such theories cannot account for the purposive character of behavior. To illustrate, he believes that Harlow's studies of learning sets prove that animals have the ability to select cues that they believe are appropriate to what the animal thinks is the problem to be solved. In such problems the animals "learned what a given positive or negative trial '*proved*'." Neither Restle nor Broadbent, he says, can deal with such evidence without begging the question. Taylor also believes that the latent learning and the spatial learning studies show that animals have the ability to improvise a new act that they think will fit the demands of the problem to be solved. Although many S-R psychologists have tried to explain away these facts, Taylor argues effectively that none of these explanations will account for the facts. Although most psychological readers will be familiar with the issues in this controversy, few, if any, will ever have seen these issues treated as thoroughly and as clearly as Taylor treats them in this book.

There is, I think, one serious fault in this book, but it is a fault that was perhaps inescapable. Let me explain. Taylor devotes the first half of his book to an exposition of what he means by purposive behavior and by a purposive explanation of purposive behavior. In the second half, called "Theory and Fact," Taylor presents his marvelous critique of the shortcomings of all S-R theories. Few readers will make much sense out of the first half until they have studied and understood the second half. Once you grasp exactly what is wrong with each of the S-R theories then, and only then, are you prepared to consider what he has to say in the first half of the book. Of course it is an easy matter to read the book backwards (and I recommend such a reading especially to psychologists), but when you do this, you suddenly realize how much better the book would have been had it been written in the reverse order. Thus, after reading the second half of the book, the first half seems, by contrast, weak, hesitant, abstract, and even vague. It seems, in fact, to be no more than a routine philosophical analysis of the idea of purpose done in the manner of the ordinary-usage philosophers.

When I think of what Taylor might have said about the purposive analysis of behavior if he had begun with the various facts that discredit the S-R theories, I get a picture of a book that might have shaken the world of psychology from its deep dogmatic slumber. But it is a mean and nasty critic who damns a good book because he thinks it could have been better. Let me end by saying then that Taylor's book is the best book that has ever been written on the logical and empirical problems in the explanation of behavior. Those who doubt this reckless statement must read the book and decide for themselves. Those who believe this reckless statement must read the book to see what I mean. If you will only follow this simple advice, the book will get the wide audience it deserves.

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BENBOW F. RITCHIE

The Mass Consumption Society. By GEORGE KATONA. New York, McGraw-Hill Book Company, 1964. Pp. viii, 343. $7.95.

Psychologists will probably view this work of George Katona with ambivalence. On the one hand, he establishes an important role for psychology in understanding and directing the working of our economy. On the other, he advocates some positions which violate the general social ideology of psychologists, such as that stimulation of people's wants is a good thing because, as their wants grow, they find new ways of earning money and this will increase the productivity of the economy.

Perhaps I have sharpened Katona's position on consumer wants just a trifle (although certainly not much) for the sake of drama. But the fact is that this book is full of surprises for both the economist and the noneconomist. Some of the surprises come directly from his view of our present economy. The central thesis of his approach is that many things that the consumer buys—houses, such durable goods as automobiles and appliances—should properly be considered, at least in part, as an investment. Furthermore, the consumer has a good deal of discretion as to how much he will spend over a particular time-period. Therefore, the consumer's spending and intention to spend are not merely at the disposal of the output of the economy but an important factor in its growth. For this reason, Katona and his colleagues at the Survey Research Center have worked for the past two decades in perfecting their methods of measuring consumer buying intentions as a method for predicting the performance of the economy.

The logic of his position and his data certainly support the notion that the consumer's expectations of the future affect his intentions to buy and that these intentions to buy affect his behavior. Even if Katona's data indicated that to this date his procedures had had no success (and this is certainly not true), one would have to stick with his logic and contend that he had only yet not hit on the proper method of measuring intentions. But, as I have said, he has already demonstrated considerable success, and my reservations are solely in my own competence to judge the importance of his contributions to economic forecasting.

On the other issues, his record is a heartwarming testimony to gathering data while others pontificate. It has been taken for granted in some quarters that pension plans will encourage people to spend freely rather than provide for their retirement.

Katona's data, however, support the contradictory hypothesis; namely, when people have a security base in the form of some reasonable pension plan, this encourages them in the worthwhileness of making the effort to save additional funds for their retirement.

Or, it seemed self-evident that if people expected inflation and had plans to buy something fairly expensive, they would hasten to buy it before prices increased. There are two relevant findings in his data that are not self-evident. The basic one is that the American people distinguish between a gradual inflation and a runaway inflation. At various points in the postwar period (World War II), they have correctly anticipated inflation. But this did not lead to any panicky fear of runaway inflation. And, on the basis of this anticipation of the gradualness of the inflation, they acted in just the opposite way from that posited in the sentence with which I opened the paragraph: Rather than running to buy things, the exhibited their disapproval of what they considered to be unfair prices and deferred their purchases!

Katona would be the first to say that this is an unusual book for a psychologist to write, or read, or for that matter to review. He has been doing unusual pioneering work. It is probable that few psychologists will want to read the entire book, although I would hope that some psychologists and many economists would. Basically, it is a testimony to the value of the economic information one can gather directly from people via the survey method. May we hope that, in the future, books such as this will be less unusual.

Harvard University RAYMOND A. BAUER

Child Development: Volume II. Universal Stage of Infancy. By SIDNEY W. BIJOU and DONALD M. BAER. New York, Appleton-Century-Crofts, 1965. Pp. x, 183. $1.75.

Behavior and development in the human infant are currently attracting considerable attention from psychologists. A brief text on infancy could be especially timely, integrating knowledge in a field in which there is much new research. But, although developmental psychologists will want to be familiar with this systematic statement about infancy by two respected colleagues, probably few will find it useful as a text, partly because its coverage is so truncated.

This is not a book about infants nor about psychological approaches to infancy; instead it is an account of how Skinnerians think about infants. Little attention is given to other work which psychologists find interesting. Erikson is mentioned not at all and Piaget is mentioned but twice, in sentences which will carry meaning only to those already familiar with his contributions. The voluminous research literature on the infant disciplines and their possible implications for psychological development is disposed of by a parenthetical allusion to Caldwell's excellent 1964 review. The challenging studies of maternally deprived infants are dealt with by a brief discussion of early Spitz and Goldfarb papers and Rheingold's 1956 study, with no reference to the more recent work by Yarrow, Bowlby, and others. Harlow's exciting program of research on the effects of rearing conditions on development in the rhesus macaque is represented by a three-paragraph description of a few of his earliest findings, published in 1958. Bayley's extensive studies of intellectual development in infancy receive one sentence. There is no mention of the work of Bridger, Escalona, Graham, Kagan, Lipton.

How, then, do the authors fill their 180 pages? They apply the Skinnerian approach to development—as outlined in the first volume in their projected four-volume series of paperbacks on *Child Development*—to the infancy period. On the face of the matter, Skinner and an infant would seem to be a good match. The young human is an organism whose behavior can readily be understood as adaptively mediating between strongly experienced needs and the reinforcement contingencies in his surroundings. He possesses neither language nor other cognitive sophistications; his simple and crude psychological functioning seem isomorphic to the heavy-footed simplicities of Skinner's approach. It is no accident that in human research operant conditioning has been especially successful with infants and with mentally retarded and brain-damaged young children.

The skillful behavioral technician needs, however, to know his subject intimately, and this book is not very successful in acquainting its readers with human infants. The authors survey the behavior repertoire of the infant and catalog the stimuli to which he is responsive. But the abstract, clumsy, and obfuscatory vocabulary of their theory, and their singleminded devotion to convincing the reader of the correctness of their theoretical preferences, make their labor Sisyphean.

Infancy is "the universal stage" because the infant's commerce with his world is closely delimited by emerging expressions of his rapidly developing neuromusculature. The common biological substrate for behavior is what makes infancy "universal," the term Bijou and Baer choose for the subtitle of this volume. The authors' coverage of human infant biology is limited by their reliance on secondary sources which appeared a decade or two ago, and on primary sources from the 1930's.

The value of a clear and specific theory in generating hypotheses to guide experimentation in a laboratory is so well understood that it needs no defense here. The authors' own experiments provide abundant evidence of the merits of adherence to theory. Orientation to a single theory may also be appropriate in an advanced treatment of a topic, intended to serve the needs of graduate students and other novice investigators. But it is another matter altogether to rely on a single theory for guidance in the preparation of an elementary text. This book provides good coverage of the relevant Skinnerian literature, but that literature is at present too scanty to provide an empirical foundation for a well rounded treatment of infancy. The result is a book which is specialized without being advanced, which displays the convictions of the authors without affecting those of the reader.

Stanford University ALBERTA ENGVALL SIEGEL

Distortion and Selectivity. By PER RAND. New York, Humanities Press, 1964. pp. xii, 272. $6.50.

The somewhat enigmatic title of this book tends to conceal the fact that Dr. Rand is primarily concerned with the educational process. He reports the methods and results of an investigation involving 176 pupils attending the sixth grade in the Norwegian town of Porsgrunn. The major variables studied were test-anxiety, level of aspiration, performance in a number of arithmetical tasks, and recall of correctly completed as well as interrupted tasks.

The author's own research is prefaced with a detailed and critical review of related efforts, the initial two-thirds of the book being devoted to a discussion of experiments in level of aspiration, perception, and memory. Rand brings together a number of topics which, although obviously related, have not heretofore been subjected to any serious attempt at theoretical unification. The author's approach however, is critical as well as integrative. For example, in reviewing the literature dealing with experiments on level of aspiration, Rand questions the commonly accepted generalization that knowledge of the results obtained by others in an experimental task influences level of aspiration more than knowledge of one's own performance. A subject who needs to feel successful may set his aspirations rather low and perceive his performance as rather high. Estimates of performance are frequently distorted, and the distortion, according to Rand, takes place as a result either of selective perception or of differential forgetting and retention during the interval from perception to recall.

These considerations lead the author into a detailed discussion of memory and perception. Several of the most widely accepted generalizations in this area are either called into question or are qualified. Considerable space is devoted to the "new look" in perception, and in particular, to the concept of perceptual defense. Although giving due weight to such criticisms as those of Howes and Solomon, Rand concludes that perceptual selectivity and distortion is most probably explained in such terms as set, subception, and interference.

Rand invokes cognitive dissonance theory to supply certain elements lacking in theories of aspiration. He views the amount of dissonance resulting from a trial on a certain task as a function of the difference between level of aspiration and level of performance when level of performance is the lower of the two.

The author's own research enters as something of an anti-climax to the book. He sets himself the task of examining 37 hypotheses dealing with anxiety, aspiration, performance, and recall. This is a formidable undertaking, particularly when the sample consists of only 91 girls and 85 boys. The results are too complex to report here, but, in general, it was found that highly anxious boys tended to over-estimate their performances and to remember selectively tasks that they had completed correctly. The girls, on the other hand, gave more realistic estimates of their performances and remembered mainly those tasks which they did not complete within the allotted time. The author refers rather diffidently to the samples that he studied as well as the measuring instruments that he employed; the former are conceded as rather small and the latter as not very precise. In addition, a considerable number of the observed tendencies did not reach a satisfactory level of statistical significance.

In general, the book is to be recommended for its comprehensive and scholarly treatment of several related areas of research bearing upon problems of cognition and achievement. Rand has not merely reviewed the literature but has evaluated it in critical and insightful fashion. Anyone who contemplates research in any of the several areas that Rand has covered would be well advised to read his monograph. His own experiments, while not providing answers to the many questions that he raises, will suggest possibilities that other researchers may pursue.

University of Maryland ELLIOTT MCGINNIES

Paired-Associates Learning: The Role of Meaningfulness, Similarity, and Familiarization. By ALBERT E. GOSS and CALVIN F. NODINE. New York, Academic Press, 1965. Pp. xiii, 322. $13.50.

The first two sentences of this volume are: "Summarized and evaluated in this monograph are data and theories on the role in paired-associates (*PA*) learning of meaningfulness, similarity, and familiarization of stimulus and response members of *PA* lists. Presented also are the designs, results, and conclusions of 12 new experiments on *PA* learning in which one or more of the variables have been meaningfulness, similarity, and familiarization of stimulus-members, response-members, or both" (p. 1). The authors treat the issues of meaningfulness, similarity, and familiarization in *PA* learning in eight chapters consecutively entitled "Introduction," "Scalings of Meaningfulness," "Meaningfulness in Paired-Associates Learning," "Theoretical Analyses," "Similarity," "Familiarization," "Meaning and Association," "Summary, Significance, and Suggestions." In addition, 12 experiments are reported in the Appendix.

The contribution of this book lies in its comprehensive summary of the methods and findings of studies in which paired-associate learning was studied as a function of meaningfulness, similarity, and familiarization. Discussion of these topics is well organized and the volume thus serves as a reference to the area. Also, the 12 experiments presented in the Appendix contribute to the literature of the area.

One major deficiency of the book is that each topic is presented in terms of the methods of study and research findings with virtually no discussion of the relations between findings or the theoretical issues raised by the findings. Perhaps this style of presentation may be best described as analogous to reading only the method and results section of a series of journal articles. The unfortunate outcome of this mode of presentation is that the theoretical significance of the various topics is not developed. Chapter 4, entitled "Theoretical Analyses" does little to remedy the problem, although the use of recognition-responses is of interest.

A second deficiency of the work is that it suffers from writing that may be best described as cumbersome. The authors, apparently trying to encompass virtually all of the facets of a statement within one sentence, have produced a style with a sentence structure which tends to obscure salient points.

A third deficiency is that the 12 experiments reported in the Appendix are not integrated adequately. The authors mention the results of these experiments in the first eight chapters when the results are pertinent, but the authors provide little rationale involving the relation of the experimentation to the issues of the book; *e.g.* why is FORM an important variable in the study of meaningfulness, similarity, and familiarization? Why is the recall vs. anticipation comparison important? The only apparent answer is that it is of interest to generalize results.

Finally, it is of interest to ask what influence this work will have compared to the most recent book in the area (B. J. Underwood and R. W. Schulz, *Meaningfulness and Verbal Learning*, 1960). There have been 118 citations of Underwood and Schulz's book in the *Journal of Experimental Psychology* (since 1961) and *Journal of Verbal Learning and Verbal Behavior* (since 1962). In the next five years, there may be as many citations of the Goss and Nodine work. Many of the former citations, however, have been in reference to conceptual notions such as the response-learning—associative learning distinction and the spew hypothesis, whereas citations of Goss and Nodine probably will involve reference to their summary of experimentation or to their specific experiments. Thus, because of the general lack of conceptual distinctions or theoretical development, the impact of the Goss and Nodine work will probably be considerably less than that of Underwood and Schulz's book.

University of Pittsburgh JAMES F. VOSS

Organizational Psychology. By EDGAR H. SCHEIN. Englewood Cliffs, New Jersey, Prentice-Hall, 1965. Pp. xi, 114. $3.95 (cloth), $1.50 (paper).

This modest volume—like its predecessors in the Foundations of Modern Psychology Series—was written with a very specific objective in mind. Its purpose is to provide the introductory psychology student with a concise but lucid and more detailed treatment of *Organizational Psychology* than is possible in the typical, general textbook in psychology. The author has done his work well. He writes in a simple and direct fashion; avoids the lure of complete coverage at the expense of detail and clarity; and perhaps most important, his judiciously selected concepts and findings are discussed in terms of a metatheory which enables him to provide a tightly organized and coherent picture of this comparatively new field.

What is organizational psychology and how and why did it evolve? These questions are answered by Schein in the first two chapters of the volume. Individual approaches to such traditional organizational problems as recruitment, selection, training, allocation and worker motivation, provided only partial solutions. What has evolved is a modern approach which conceives of the organization as a complex system which acts upon and in turn is continually acted upon by the broader social context in which it is embedded. Given this orientation, the various psychological problems of an organization noted above are not only seen as interrelated but their analysis moves to a concern with organizational groups and sub-systems, if not the total organization itself. Some of these traditional problems are examined in detail in Chapter 3 and subjected to analysis within the framework of a systems approach.

In Chapter 4 Schein systematically considers the coördination of behavior in organizational settings via particular types of power or authority structures. In this discussion the assumptions about the nature of man that underlie particular managerial strategies are carefully examined and evaluated. Thus, there is some truth in the "rational-economic," the "social," and the "self-actualizing" conceptions of man, but it is the integration of all of these assumptions into a conception of a "complex man" that is the most tenable approach in the light of existing data. The discussion that follows (Chapter 5) focuses on the nature of formation of psychological groups in the organization—particularly with respect to the focal issue of their capacity to integrate organizational goals and the needs of the individual. The no less thorny problems of intergroup competition and conflict are then considered in the final section of this chapter.

The final two chapters of the volume (Chapters 6 and 7) are much shorter than the earlier ones and for this reason tend to be somewhat less effective. This is especially true of Chapter 6 which examines various conceptual models of organizations as complex systems, *e.g.* Homans, Likert, Trist, etc. In this instance brevity combines with abstractness and renders the discussion somewhat less than clear for the beginning student. The final chapter deals with organizational effectiveness, and within a systems framework this refers to the extent to which an organization can survive, grow, and adapt to an ever *changing* environment. Thus, the criterion of organizational effectiveness is a multiple one which must necessarily involve such relevant factors as the organization's ability to sense environmental change, to respond appropriately to it, and to evaluate its efforts to adapt to this change.

Writing a concise but meaningful account of a psychological field of inquiry is a difficult task to say the least. Insofar as *Organizational Psychology* is concerned, Schein has more than met this challenge and produced a very useful volume.

Brooklyn College HAROLD PROSHANSKY

The Rat: A Study in Behavior. By S. A. BARNETT. Chicago, Aldine, 1963. Pp. xvi, 288. $7.95.

This book is a timely attempt to write an introductory text summarizing the major threads of work by experimental psychologists (animal learning and motivation types), physiologists (neurophysiological and endocrine types), and zoologists (ethological types), and weaving them into a coherent picture as sets of phenomena codetermining all animal behavior, while using the rat as an example. The author writes on the assumption that "all of these activities are desirable, and that none is morally superior to any other" (p. xv).

Two decades ago this assumption would have been merely idealistic; at present it is practical. Using the author's imagery (p. xv), it is no longer easy to sit astride a maze, at an oscilloscope, or behind a binocular, ignoring or criticizing the others. Attempts at interaction between these groups are slow and painful, *e.g.* the recent confrontation in Rome (W. M. Shoenfeld and S. H. Baron. Ethology and experimental psychology. *Science,* 147, 1965, 634-635.), but steady, *e.g.* the recent formation of the interdisciplinary Animal Behavior Society, already 500 strong, with its aggressive support of active collaboration with groups in both the American Psychological Association and the American Physiological Society.

The author's logistics are well thought out. The book is brief, well organized, clearly written, with a 41 item glossary and a well selected 344 item bibliography containing handy page references to their use in the text. The plates and figures are pleasantly achromatic, containing the best collection of photographs and striking line drawings of "stop-action" rat behavior that the reviewer has seen.

The author's examples are taken from two variations of wild rats as well as the laboratory albino. His topic selection is very broad, ranging from ecological factors in simple behavior patterns, through social behavior, hunger, thirst, reproductive behavior, followed by a description of the classical types of learning and the theoretical issues to which they lead in modern behavioral theory. All this is rounded out by a summary of "brain and behavior," running from the work of Beach and Lashley through a discussion of some limbic and reticular determinants of behavior.

Although the author has a zoological bias, and is obviously most competent in the ethological literature, the coverage is surprisingly uniform. This is not readily apparent, as reflected by the results of an informal poll carried out on several of the reviewer's colleagues. The comments were consistent; one ethologist thought its ethology was inadequate and superficial, but found the physiological section adequate, another berated its implementation of a comparative approach by limiting itself to the rat, yet found that the learning section wasn't bad. A physiologist thought the physiological coverage was superficial but the ethological section strong. A learning oriented experimentalist felt its theoretical summary incomplete, and so on. . . .

The results of this poll reflect the still prevalent *territoriality* of investigators of animal behavior, a condition that this book, among the aforementioned trends, may well help correct. Because of this some instructors may only be slightly ahead of the students in sections outside of their specialty. However, the work necessary to correct this will be minimal and the rewards well worth the effort.

The students who use this book will also profit from this effort, as they will learn early in their career that the study of animal behavior is truly interdisciplinary, whether one calls it Ethology as Barnett chooses, or by any other name.

University of Illinois HARRY L. JACOBS

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(The books listed here have not as yet been noted in our pages. Listing here does not, however, preclude their later review.)

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IMPROVEMENT IN HABIT-REVERSAL AS A FUNCTION OF AMOUNT OF TRAINING PER REVERSAL AND OTHER VARIABLES

By R. C. GONZALEZ, BARRY D. BERGER, and M. E. BITTERMAN,
Bryn Mawr College

Spatial habit-reversal as a function of amount of training per reversal has been studied in a variety of experiments with the rat.[1] In some cases, the variable seemed to have little effect. In other cases, however, more training per reversal produced somewhat poorer performance in early reversals but somewhat more rapid improvement, with differences among groups tending rapidly to disappear as training continued. The potential importance of these results for an understanding of the processes underlying progressive improvement in habit-reversal prompted the further experiments on amount of training per reversal which are reported here. In the present experiments, visual rather than spatial problems were studied, partly out of interest in the generality of the earlier spatial effects, and partly in the expectation that, if general, the effects would be larger and more stable in visual problems, which usually yield less rapid improvements on amount of training per reversal which are reported here. In the of interest in generality) with both pigeons and with rats—the pigeons

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[1] A. J. North, Improvement in successive discrimination reversals, *J. comp. physiol. Psychol.*, 43, 1950, 442-460; Performance during an extended series of discrimination reversals, *ibid.*, 43, 1950, 461-470; B. H. Pubols, Jr., Successive discrimination reversal in the white rat: A comparison of two procedures, *ibid.*, 49, 1956, 243-248; Serial reversal learning as a function of the number of trials per reversal, *ibid.*, 55, 1962, 66-68; E. J. Capaldi and M. G. Senko, Effects of trials-per-problem on successive discrimination reversal learning, *Psychol. Rep.*, 8, 1961, 227-232; R. G. A. Stretch, B. McGonigle, and R. S. Roger, Serial position-reversal learning in the rat: A preliminary analysis of training criteria, *ibid.*, 56, 1963, 719-722; Serial position-reversal learning in the rat: Trials/problem and the intertrial interval, *ibid.*, 57, 1964, 461-463.

trained by two different methods, and the rats trained with two different amounts of reward. Some relevant data are also reported for an experiment with the Japanese quail in which number of trials per session was varied in a series of criterion-reversals.

METHOD

Subjects. The *Ss* were 70 white Carneaux cocks, 48 Wistar rats, and 28 Japanese quail, all experimentally naïve and individually housed in temperature-regulated laboratory rooms. The animals were on a 24-hr. training and feeding schedule for six days each week; on the seventh day they were only fed. The pigeons were maintained at 75% and the rats at 85% of free-feeding weight. The quail were fed to satiation each day.

Pigeon-techniques. Two different techniques were used for the pigeons—the conventional key-technique and a new ingestive or magazine-technique.[2] In each case, the animal was trained in a ventilated picnic-chest divided into two compartments by an aluminum panel, one compartment for *S* and the other for equipment. To the key-animals, the panel displayed three pigeon-keys at head-level and the aperture of a grain-magazine at floor-level, below the center key (*CK*). To the magazine-animals, the panel displayed a single (centered) key (the *CK*) at head-level and the apertures of two grain-magazines at floor-level, below the *CK*. In each case, a trial began after a 6-sec. intertrial interval (*ITI*) in darkness with the illumination of the white *CK*. A peck at the *CK* turned it off and presented the discriminanda—red and green side-keys for the key-animals (which pecked at one of the keys), and red and green illumination of the magazine-apertures for the magazine-animals (which responded by inserting the head into one of the apertures, choice being detected by photocells in the lips of the apertures). In the key-situation, correct choice turned off the key-lights and presented the grain-tray for 3 sec., during which time the magazine-aperture was illuminated with white light. In the magazine-situation, correct choice turned off the colored aperture-lights and produced grain in white light for 2.5 sec. (the variation in reinforcement-duration serving to equate the amount of grain that could be taken in the two cases). A limited correction-method was used throughout: an incorrect choice produced a time-out in darkness of 6 sec., following which a peck at the white *CK* produced another opportunity for choice; after five repetitive errors, a peck at the *CK* produced the correct alternative alone (guidance), and response to it was reinforced. The spatial arrangement of the discriminanda varied from trial to trial in Gellermann-orders, and 40 trials per day were given.

Four groups of 10 *Ss* each were trained in the magazine-discrimination. One was reversed daily, a second after two days on each problem, a third after four days on each problem, and a fourth each time it met the criterion of no more than six errors on a given day and no more than three errors in the last 20 trials of that day. Three groups of 10 *S*s each were trained in the key-discrimination. One was reversed daily, and a second was reversed each time it met the criterion already specified; a third group was trained to criterion in each reversal and then given three days of overtraining.

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[2] E. M. Stearns and M. E. Bitterman, A comparison of key-pecking with an ingestive technique for the study of discriminative learning in pigeons, this JOURNAL, 58, 1965, 48-56; R. C. Gonzalez, B. D. Berger, and M. E. Bitterman, A further comparison of key-pecking with an ingestive technique for the study of discriminative learning in pigeons, *ibid.*, 59, 1965. 217-225.

Rat-technique. The rats were trained in a gray-painted Lashley apparatus modified to eliminate punishment for errors; instead of jumping, the animals traversed narrow elevated pathways leading from the starting platform to each of the two windows. One of the discriminanda was a black card on which a 3-in. white circle was centered; the other was a card covered with ½-in. black and white vertical stripes. There were 20 trials per day by the guidance-method. The correct card was unfastened, and the animal could push it over to gain access to the feeding platform at the rear. The incorrect card was locked in place; having made contact with it, the animal was brought back to the starting platform and permitted to respond again, an incorrect choice being prevented by manual guidance. The lateral arrangement of the two cards varied from trial to trial in Gellermann-orders. Six groups of 8 *S*s each were trained in a 2 × 3 factorial experiment involving two amounts of reward and three amounts of training per reversal. The low reward was one 45-mg. Noyes pellet, and the high reward was 10 sec. of access to a dish of wet mash. The amounts of training were one, two, and four days per reversal.

Quail-technique. The apparatus for the quail was exactly the same as the key-apparatus for the pigeons, except that the keys were several inches lower and the magazine presented corn instead of a mixture of other grains. The guidance-method (permitting no repetitive errors) was used. One group of 16 *S*s was trained in daily sessions of 20 trials to the criterion of no more than three errors in any session. A second group of 12 *S*s was trained in daily sessions of 40 trials to the criterion used for the pigeons—no more than six errors in any session, and no more than three errors in the last 20 trials. The training of both groups was continued for 20 reversals.

RESULTS

The principal results of the magazine-experiment with pigeons are given in Fig. 1, which shows mean initial errors on the first day of the original problem (Reversal 0) and each of 24 subsequent reversals. The curves for the four groups are quite similar. Analysis of variance yields a significant Reversals-effect ($F = 28.30$, $df = 23/828$, $p < 0.01$), but not a significant Groups-effect ($F < 1$).[3] There is, however, a significant interaction ($F = 2.36$, $df = 69/828$, $p < 0.01$), which may reflect the somewhat more rapid fall in the criterion and 4-day curves than in the 1-day and 2-day curves. It should be noted that the 1-day and 2-day groups continued to improve in subsequent reversals. A plot for the 1-day group shows a steady decline over the 96 reversals to the level of about 4 errors, with indication that there would have been further improvement if training had been continued. These results suggest that number of reversals is the main factor in improvement, but that the role of general experience cannot be disregarded.[4]

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[3] The data for Reversal 0 were not, of course, included, either in this analysis or in those reported below.

Within-reversals error-curves for the four groups also look very much the same, and the four curves for selected reversals are pooled in Fig. 2 to show the nature of the improvement in the first 24 reversals. A pooled curve for Reversals 95 and 96 of the 1-day group also is plotted in Fig. 2 to show the nature of the improvement after considerably more reversal-training. The curves suggest two sources of improvement: The first, which is evident in the earlier reversals and in the earliest stage of each reversal, may be characterized as a reduction in negative transfer—a decline in the strength of the preference carried over from each preceding reversal. The second, which is evident in the later reversals and in the later stages of each reversal, may be characterized as an increase in the rate at which the new preference becomes established.

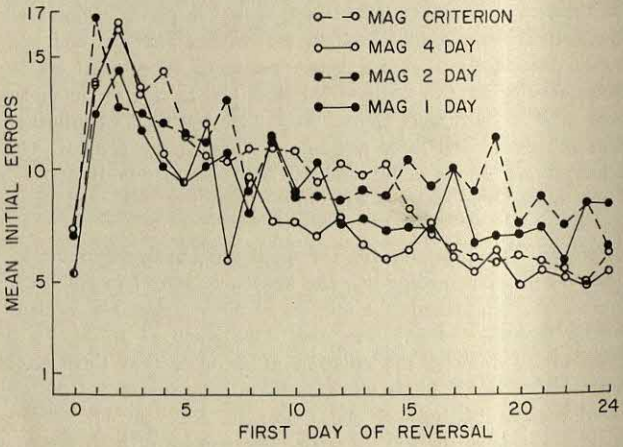

FIG. 1. MEAN INITIAL ERRORS OF THE FOUR PIGEON-MAGAZINE GROUPS ON THE FIRST DAY OF THE ORIGINAL PROBLEM AND EACH OF 24 SUBSEQUENT REVERSALS

The principal results of the key-experiment with pigeons are given in Fig. 3. The curves show mean initial errors on the first day of the original problem and each of 26 subsequent reversals for the three groups. Here

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[4] This curve may be found in Fig. 3 of Gonzalez, Berger, and Bitterman, *op. cit.*, 221.

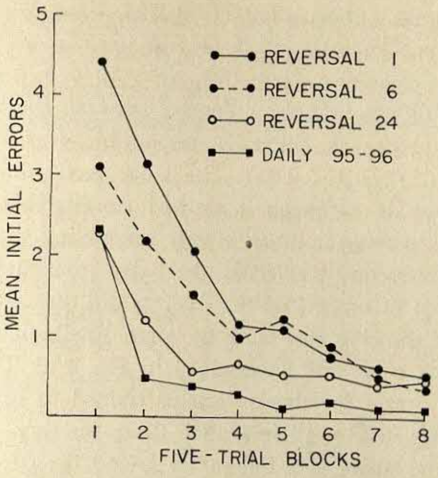

FIG. 2. WITHIN-REVERSALS LEARNING CURVES OF THE POOLED PIGEON-MAGAZINE GROUPS FOR REVERSALS 1, 6, AND 24 AND OF THE 1-DAY GROUP ALONE FOR POOLED REVERSALS 95-96

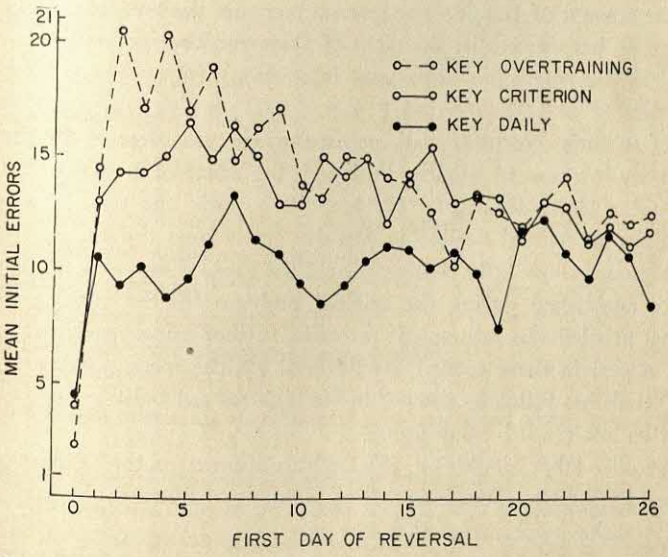

FIG. 3. MEAN INITIAL ERRORS OF THE THREE PIGEON-KEY GROUPS ON THE FIRST DAY OF THE ORIGINAL PROBLEM AND EACH OF 26 SUBSEQUENT REVERSALS

there is a significant Groups-effect ($F = 3.68$, $df = 2/27$, $p < 0.05$) as well as a significant Reversals-effect ($F = 3.36$, $df = 25/657$, $p < 0.01$) and a significant interaction ($F = 2.01$, $df = 50/675$, $p < 0.01$). Orthogonal analysis indicates that the difference between the 1-day group and the other two groups is primarily responsible for the Groups-effect ($F = 7.03$, $df = 1/27$, $p < 0.05$). The 1-day group did not improve in the first 26 reversals, although it showed considerable within-reversals learning, its within-reversal curves closely resembling that for Reversal 6 in Fig. 2. In 70 subsequent reversals, the 1-day group did improve, reaching the same level of error (5-6 per day) as did the magazine-animals in 24 reversals, and showing very much the same distribution of errors within reversals (see the curve for Reversal 24 in Fig. 2).[5] The early first-day within-reversals curves for the key-groups trained to criterion and overtrained are higher in over-all level than those for the 1-day group; with continued training, there is a fall in level, and the curves for the three groups become indistinguishable.

The principal results of the experiment with rats are given in Fig. 4. The two sets of curves, one for high and one for low reward, are plotted in terms of mean errors on the first day of the original problem and each of 18 subsequent reversals. As in the key-experiment with pigeons, increasing the amount of training per reversal increases the level of error in early but not in later reversals; the effect of Days-per-Reversal is not significant ($F < 1$), but there is a significant interaction of Days-per-Reversal with Reversals ($F = 3.92$, $df = 34/714$, $p < 0.01$) which reflects the washing out of an early effect (as well, perhaps, as the crossover of the curve for the 4-day low-reward group). Although the effect of Reversals is significant ($F = 15.71$, $df = 17/714$, $p < 0.01$), only the two 4-day groups show any substantial decline in first-day errors over the 18 reversals; the other groups show little change beyond the rapid disappearance of a preference established during the original problem for the positive stimulus of that problem. In subsequent reversals, further improvement does, however, appear in these groups. By Reversal 72, the mean number of errors in 20 trials has fallen to about 7 in the high-reward 1-day group and about 4 in the low-reward 1-day group.

The only other significant effect which appears in the analysis of first-day errors for 18 reversals is that of Amount of Reward: the high-reward groups make more errors than the low ($F = 10.00$, $df = 34/714$, $p <$

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[5] The performance of the 1-day group plotted in terms of mean errors per reversal for 96 reversals may be seen in Fig. 3 of Gonzalez, Berger, and Bitterman, *op. cit.*, 221.

0.01). The difference is due, not to poorer learning in the high-reward groups, but to the fact that the high-reward groups tend to develop stronger preferences in each reversal and therefore to show more negative transfer in the next. This effect may be seen in Fig. 5, which displays some representative within-reversals plots for the two 4-day groups. The high-reward error-curves begin higher but fall more sharply and reach lower levels in the later trials of each reversal, the total number of errors made in the four days of each reversal being very much the same for the two amounts of reward. The persistence of the effect of amount of reward may be seen in the 1-day curves for Reversal 72; here, too, the high-reward group shows

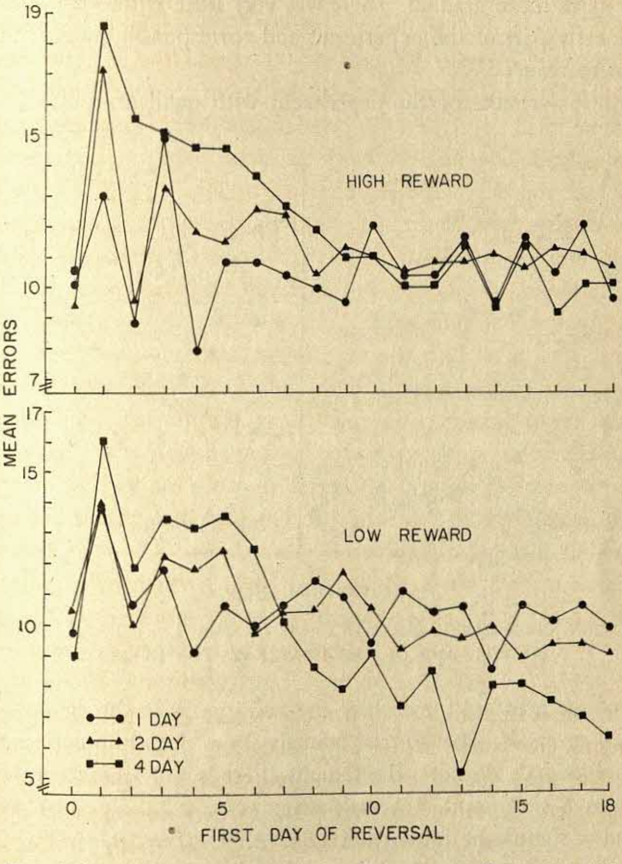

FIG. 4. MEAN ERRORS OF THE SIX GROUPS OF RATS ON THE ORIGINAL PROBLEM AND EACH OF 18 SUBSEQUENT REVERSALS

a stronger initial preference for the previously reinforced alternative. The curves of Fig. 5 show also that progressive improvement in the 4-day groups took the form of a progressive decline in negative transfer—that is, a progressive decrease in the strength of the preference carried over from one reversal to the next. In the 1-day groups, the course of improvement was much more gradual. There was very little within-reversals learning in the early part of the experiment and correspondingly little negative transfer to be reduced.

The principal results of the experiment with quail are given in Fig. 6 in terms of mean errors for each of the two groups in the first 40 trials of the first 20 reversals. The 40-trial animals show clear improvement, while the 20-trial animals do not—the Groups-effect is not significant ($F < 1$), but there is a significant Reversals-effect ($F = 2.72$, $df = 19/494$, $p < 0.01$) and a significant interaction ($F = 3.23$, $df = 19/494$, $p < 0.01$). Smaller groups were continued for 18 additional reversals, during which time the 40-trial animals continued to improve, reaching the level of about nine errors, while the 20-trial group showed little improvement.

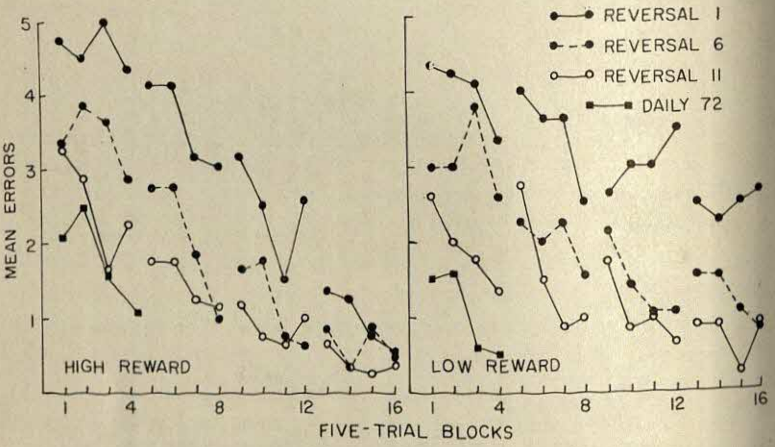

FIG. 5. WITHIN-REVERSALS LEARNING CURVES OF THE 4-DAY RATS FOR REVERSALS 1, 6, AND 11, AND OF THE 1-DAY RATS FOR REVERSAL 72

The within-problems performance of the 40-trial quail for pooled Reversals 1-2 and 23-24 is plotted in Fig. 7 along with comparable data for the pigeons which were trained to criterion in the key- and the magazine-situations. The curves show the dramatic improvement in the performance of the quail—much more marked even than the improvement of the

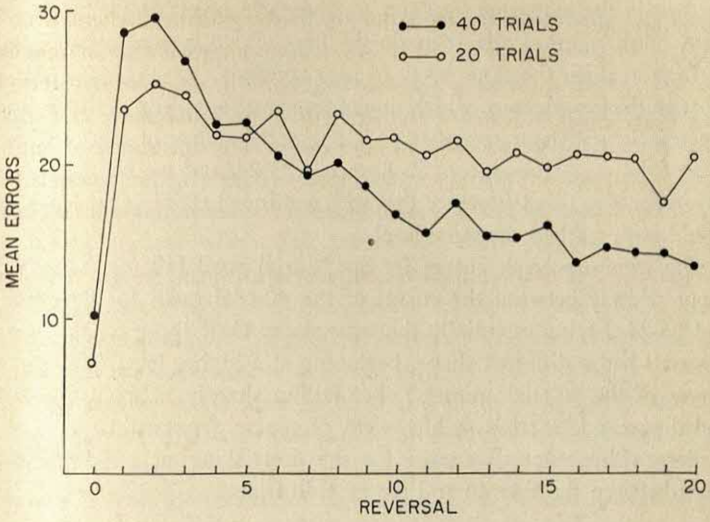

FIG. 6. MEAN ERRORS OF THE TWO GROUPS OF QUAIL IN THE FIRST 40 TRAILS OF THE ORIGINAL PROBLEM AND EACH OF 20 SUBSEQUENT REVERSALS

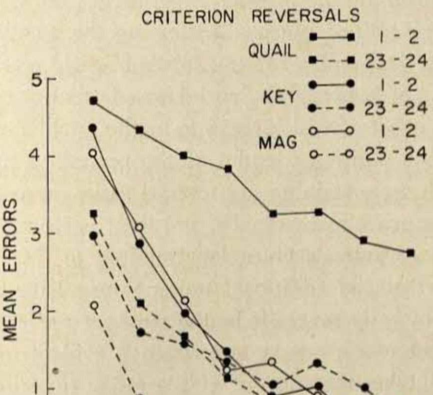

FIG. 7. WITHIN-REVERSALS LEARNING CURVES OF THE 40-TRIAL QUAIL, THE KEY-PIGEONS TRAINED TO CRITERION, AND THE MAGAZINE-PIGEONS TRAINED TO CRITERION, FOR POOLED REVERSALS 1-2 AND 23-24

pigeons in the magazine-situation. In the early reversals (1-2), the quail learn much more slowly than do the key- and magazine-pigeons, which perform at about the same level. In later reversals (23-24), the quail catch up with the key-pigeons, which do not improve as rapidly as the magazine-pigeons. In still later reversals (37-38), performance of the quail is like that of the magazine-pigeons in Reversals 23-24, and we know from other experiments in this laboratory that with continued training the key-pigeons would have reached the same level,

The within-reversals curves for the 20-trial quail fall for the most part about midway between the curves of the 40-trial quail for Reversals 1-2 and 23-24, having essentially the same slope. Only those for the first few reversals had a different shape, beginning at a higher level (like the early curves of the 40-trial animals), but falling sharply between the first 20 and the second 20 trials (which were, of course, separated by a day). The earliest within-reversals curves for the 40-trial animals show a similar effect between the first 40 and the next 40 trials.

DISCUSSION

An effect obtained in previous experiments on spatial habit-reversal in rats is found again in the present experiments on visual habit-reversal in rats and in key-trained pigeons: increasing the amount of training per reversal increases the number of errors made in early reversals. The animals are not helped, but hindered, by an increased amount of training per reversal, the main effect of which seems to be the establishment of a stronger preference for the positive stimulus of the preceding problem. Although the animals with more training per reversal make more errors in the early reversals, they improve more rapidly, and their performance soon becomes as good as that of animals given less training, or, in some cases, better, which suggests that the additional training may have a beneficial effect that is masked in early reversals by the stronger preferences established.[6] The improvement which occurs, and which is evident in the earliest stage of each reversal, takes the form of a progressive reduction in the strength of the preference carried over from each reversal to the next—a reduction in negative transfer.

In the magazine-trained pigeons, a significant interaction of Groups × Reversals suggests more rapid improvement in groups given more training per reversal, but the Groups-effect is small and insignificant, presumably because the magazine-technique is so efficient that amounts of training beyond one 40-trial day do not produce substantially greater preferences for the positive stimulus. (If this interpretation is correct, an experiment with fewer trials per reversal would yield a more substantial Groups-effect.) The pattern of improvement in the magazine-situation is, however, exactly the same as that found in the key-situation and in the experiment with rats.

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[6] The appearance of a crossover in the curves for the high-reward but not the low-reward rats duplicates earlier results for spatial habit-reversal. Two experiments by Pubols (*opp. citt.* 49, 1956. 243-248; 55, 1962, 66-68) with a one-pellet reward showed the effect, but others, with mash, did not.

In the experiment with quail, number of trials per day rather than amount of training per reversal was the nominal independent variable, but the criterion varied also, being more severe for the 40-trial group, which had in consequence more training per reversal. The results obtained are like those of the experiments in which training per reversal was varied directly—a greater initial level of error and more rapid improvement in animals with more training. The pattern of improvement in these animals is the same as that previously encountered, but much more dramatic than in pigeons trained to criterion in the key-situation, because the initial performance is so much worse. The difference need not be attributed to species, because it is about what might have been expected from the difference in training procedure. In work with pigeons, Stearns and Bitterman have reported that guidance produces a higher level of error in early criterion-reversals and more rapid improvement than does correction.[7]

The effects of amount of reward are different to some extent from those of amount of training per reversal. Like an increase in amount of training, an increase in amount of reward tends to strengthen the preference which is carried over from reversal to reversal. The high-reward animals make more errors in the early trials of each reversal than do the low-reward animals, but fewer errors in the later trials, because the rate of within-reversals learning increases with amount of reward. This effect, which has been described recently for a single reversal by Theios and Blosser,[8] is a quite persistent one. In the present experiment with rats, there is no hint of a significant Amount of Reward × Reversals interaction over 18 reversals; high reward continues to increase negative transfer long after increased training per reversal has ceased to do so.

A prominent feature of the improvement in habit-reversal which appears in these experiments is a decrease in negative transfer—a decrease in the strength of the preference for the positive stimulus of one reversal which is carried over to the next. This decline in negative transfer is not attributable simply to a decline in the strength of the preference *established* in each reversal. For one thing, animals trained to criterion show the same pattern of improvement as do animals given a fixed amount of training per reversal, although it might be argued that the attainment of a given criterion does not necessarily imply the same strength of preference at each stage of training. More important is the fact that the improvement in the initial level of performance in each reversal may be accompanied, as in the case of the low-reward 4-day rats (Fig. 5), by improvement in the terminal level of performance in each reversal—that is, by the development of progressively *stronger* preferences within reversals.[9] It seems, then, that the decline in negative transfer must be attributed, at least in large measure, to a decline in retention.

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[7] Stearns and Bitterman, *op. cit.*, 52-53.

[8] John Theios and Dennis Biosser, Overlearning reversal effect and magnitude of reward, *J. comp. physiol. Psychol.*, 59, 1965, 252-257.

That a decline in retention should be produced by reversal-training (with both number of reversals and total number of training days playing significant roles) is not, of course, surprising in the light of the research on proactive inhibition in human learning. Further evidence of the correctness of this interpretation may be found in the data of animals trained for more than one day on each reversal. If the interpretation is correct, performance in the early trials of a *nonreversal* day should grow *worse* relative to performance in the later trials of the previous day as reversal-training continues, and that is what seems to happen. Consider, for example, the data for the 2-day magazine-trained pigeons, which completed 48 reversals. In the first 12 problems, the animals averaged about 0.2 more errors in the first five trials of the second day than in the last five trials of the first day. This decrement in retention increased with continued training to about 0.8 errors in the last 12 problems, the performance of the animals becoming progressively better in the later trials of the first day and worse in the early trials of the second. Some rather dramatic instances of day-to-day within-reversals retention-decrements also may be seen in Fig. 5, particularly in curves of the high-reward group, whose improvement was particularly marked. What is required for a thorough test of this interpretation is a 2-day experiment with a number of animals large enough to provide stable trial-by-trial learning curves for individual reversals.

To the extent that improvement in habit-reversal can be traced to a retention-decreme-nt, it becomes unnecessary to postulate some higher-order process such as learning to reverse or the development of a reversal "set."[10] A more satisfactory alternative is provided, also, to the interpretation in terms of compounding,[11] which exaggerates the rat's capacity to discriminate among heteromodal stimulus-compounds (particularly those involving traces), and which is contradicted by data on the spacing of trials.[12] The Mackintoshes' recent interpretation of progressive improvement in terms of the strengthening of a set for the relevant stimulus-dimension,[13] which has been shown to be inadequate at a number of points,[14] fails also to deal with the present data. According to that interpretation, improvement should rest, not on a reduction in the strength of preferences carried over from each reversal to the next, but on an increase in the rate of within-reversals learning.

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[9] A like effect may be found in the results of earlier experiments on daily reversal, both visual and spatial, in the rat (M. E. Bitterman, Jerome Wodinsky, and D. K. Candland, Some comparative psychology, this JOURNAL, 71, 1958, 100-101).

It should be clear, however, that the retention-decrement interpretation is itself open to a number of objections. For one thing, there was an indication of another effect in the later reversals of the 1-day magazine-group—an increase in the rate of learning within reversals as distinct from a decline in negative transfer. More evidence on this point should be sought in an experiment with a larger number of animals. Another objection is based on the fact that progressive improvement has failed to appear in a substantial number of experiments with the fish.[15] This failure would be easier to understand if improvement in the rat were due to some second-order process rather than to a process seemingly so fundamental as proactive inhibition. It may be well perhaps to look at retention within and between reversals in the fish as well as in an animal that does show progressive improvement.

SUMMARY

Visual habit-reversal was studied in pigeons and rats as a function of amount of training per reversal (in the pigeons by two different techniques, in the rats with two different amounts of reward) and in quail as a function of number of trials per session confounded with criterion (which served in effect as a variation in amount of training per reversal). Animals given more training per reversal tended to make more errors in the early reversals, but they improved more rapidly and soon were performing as well or better than animals with less training per reversal. The improvement in performance took the form of a decrement in the strength of the preferences carried over from one reversal to the next, and the possibility was suggested that much of the improvement which is found in such experiments may be accounted for in terms of proactive inhibition. Amount of reward was found to increase the rate of within-reversals learning; the rats trained with high reward tended to make more errors in the early stages of each reversal but fewer errors in the later stages of each reversal than did the rats trained with low reward.

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[10] G. A. Kimble, *Conditioning and Learning*, 1961, 388.

[11] C. L. Hull, *A Behavior System*, 1952, 114-116.

[12] North, *opp. citt.*, 442-460, 461-470.

[13] N. J. Mackintosh and J. Mackintosh, Performance of *Octopus* over a series of reversals of a simultaneous discrimination, *Anim. Behav.*, 12, 1964, 321-324. The interpretation is derived from N. S. Sutherland's model for discriminative learning (The learning of discrimination by animals, *Endeavour*, 23, 1964, 148-152).

[14] A. F. Schade and M. E. Bitterman, Improvement in habit reversal as related to dimensional set, *J. comp. physiol. Psychol.*, 2, 1966, 43-48.

[15] Bitterman, Wodinsky, and Candland, *op. cit.*, 95-103; E. R. Behrend, V. B. Domestick, and M. E. Bitterman, Habit reversal in the fish, *J. comp. physiol. Psychol.*, 60, 1965, 407-411.

A TEST OF SOME ASSUMPTIONS ABOUT PSYCHOLOGICAL SPACE

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A number of workers have found it theoretically helpful to think of stimuli as points and stimulus-similarity as distance between points in "psychological space". If stimulus $A+$ is located as some point in psychological space a units from $A-$, then a is assumed to be a direct measure of the experienced similarity between $A+$ and $A-$. Shephard has used the concept as an aid to the understanding of stimulus and response generalization,[1] however, and Pollack discusses discrimination-processes in much the same terms.[2] Of primary interest to the present investigation, however, are the experimental studies by Attneave[3] and the more extensive work reported by Osgood, Suci, and Tannenbaum.[4] In both studies the multidimensional geometric properties of psychological space were investigated.

Using visual material, Attneave attempted to discover in what way the distinct physical parameters of compound stimuli contribute to the experienced similarity between stimuli which differ in more than one respect from each other. More formally, if $A+$ is a units from $A-$ and $B+$ b units from $B-$, then what relation do the distances a and b bear to the experienced similarity between Stimulus $A+B+$ and Stimulus $A-B-$? Attneave presents evidence to suggest that psychological space is non-Euclidean and that the similarity of $A+B+$ is simply $(a+b)$, a model derived from the work of Householder and Landahl (cited by Attneave). In contrast, the large amount of useful experimentation on the "semantic differential" would seem to indiacte that Euclidean assumptions may hold in semantic space and that the experienced similarity between $A+B+$ and $A-B-$ is probably $(A^2+B^2)^{1/2}$.

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[*] Received for publication November 29, 1965. Medical Research Council, Applied Psychology Research Unit. The author wishes to thank Dr. E. C. Poulton for his advice and the British Royal Navy for supplying subjects and equipment.

[1] R. N. Shephard, Stimulus and response generalisation, *Psychol. Rev.*, 65, 1958, 242.

[2] Irwin Pollack, Selected developments in psychophysics, with implications for sensory organization. in W. A. Rosenblith (ed.) *Sensory Communication*, 1961.

[3] Fred Attneave, Dimensions of similarity, this JOURNAL, 63, 1950, 516-556.

[4] C. E. Osgood, G. L. Suci, and P. H. Tannenbaum, *The Measurement of Meaning*, 1957, 1-342.

That psychological space is a helpful concept to a theorist does not imply that it is necessarily valid as a "picture" of stimulus-similarity and it is the purpose of the present study to investigate some aspects of the general usefulness of the notion.

Three questions are therefore asked: (1) Since visual and semantic space would appear to require different models, what of auditory space? Clearly if auditory space is fitted by neither existing model, the notion of *a* psychological space is probably too broad and might be substituted by a space for each modality. (2) If spatial models are useful, one such model should perhaps predict more reliably than a non-spatial model previously demonstrated to have some predictive power in similar situations. The non-spatial model chosen for comparison was an "Independence" model similar to that employed by Eriksen and Hake[5] and Corcoran.[6] (3) If one test-situation differs from another only in requiring a different form of response to indicate the degree of similarity between stimuli, there should be no fundamental change in the relation between predicted and empirical values. If, however, changing the form of response appears to change the characteristics of the space, the equivalence of space and similarity becomes suspect.

Method. (1) Stimulus-material. Sounds were generated on a Peters SPD5 audiometer and tape recorded. The sounds were synthesised from five separate dimensions which we shall call A, B, C, D, and E. Each sound had one of two possible values on each of the dimensions. The five dichotomies therefore resulted in a population of 32 different sounds.

The A dimension was pitch: sounds were either 1000 c/s ($A+$) or 1250 c/s ($A-$). The tone was regularly interrupted either three times per second ($B+$) or twice per second ($B-$). The interruption was not, however, a complete break in the continuity of the tone. The amplitude of the "off" period could be either 40% that of the "on" or 85% of the on. The former parameter we shall refer to as 60% amplitude modulated ($C+$) and the latter as 15% amplitude modulated ($C-$). The tone itself was superimposed upon masking noise, which was set either at 15 db or 10 db below the amplitude of the tone. The former characteristic will be designated $D+$ and the latter $D-$. The noise itself varied in band width, being either broad-band white noise ($E+$) or narrow band ($E-$). The narrow band noise was composed of frequencies determined by the frequency of the signal *i.e.* 250 cycles on each side of the chosen signal frequency.

(2) Procedure. A single sound $A+B+C+D+E+$ was chosen as the "Standard." This sound was presented for 10 sec. and followed 5 sec. later by another "comparison"-sound, which also remained on for 10 sec. Ten sec. after the termination of the comparison-sound the standard would be presented again, followed by the

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[5] C. W. Eriksen and H. W. Hake, Multidimensional stimulus differences and accuracy of discrimination, *J. exp. Psychol.,* 50, 1955, 153-160.

[6] D. W. J. Corcoran, The prediction of responses to multidimensional stimuli from response to undimensional stimuli, *J. exp. Psychol.,* 71, 1966, 47-55.

next comparison-stimulus and so on until all 32 stimuli had been presented twice. The order in which the comparison-stimuli were presented was determined by a random procedure, but the second half of the test was an exact replica of the first. In all, the test contained 64 presentations of the standard alternated with 64 presentations of the comparison-stimuli. The *Ss* in Group I were presented with a foolscap sheet containing 64, 2-in. lines. Their instructions were as follows:

> *Instructions:* This is a test of how well you can judge the similarity between sounds.
>
> When I switch on the tape recorder *pairs* of sounds will come over the loudspeaker. The first of each pair will always be the same sound, but the second will usually be different. Only on a very small number of occasions will the second sound be absolutely identical with the first.
>
> You must indicate how similar the second sound is to the first. You do this as follows. On the sheets you have there are 64 lines. Imagine that the first sound of each pair is situated at the left of each line. At the extreme right imagine a sound completely different from the one on the left. As each pair of sounds is presented put a mark somewhere on the line indicating by its distance from the left hand end how similar the sound is to the first sound of each pair. Do this for each of the 64 pairs. You will have about 10 sec. to record your answer before the next pair is presented.

The *Ss* in Group II were requested to respond with a number between 0 and 100. A 0 was to be used if the comparison was exactly identical with the standard and 100 when the comparison was completely different.

The loudspeaker was placed at a convenient position at the front of the room with the gain set such that the sounds were clearly audible at all seating positions in the room.

(3) Subjects. Group 1 comprised 38 women whose ages ranged from 27 to 59 yrs. (Mean 46) and 24 enlisted men aged between 17 and 19 yrs. Group 2 was made up of 11 Ratings of the British Royal Navy aged between 21 and 24 yr.

RESULTS AND DISCUSSION

Treatment of data Seven men and eight women were eliminated from the analysis of Group 1 and two *Ss* were eliminated from Group II. Fourteen of these *Ss* were rejected for not using intermediate positions on the lines, *i.e.* comparison-sounds were either considered exactly the same or completely different from the standard sound. Such responses probably indicated a failure to understand the instructions. The other three *Ss* were eliminated for failing to identify the standard as even moderately similar to itself, and probably indicate poor hearing or lack of attention.

An error in recording caused one sound to be absent for some *Ss* but this error was later rectified. The data for Group 1 are based on 37 *Ss*, except for the eliminated sound which is based on an *N* of 19 *Ss* only. Only the second half of the test was used in the analysis since the first half was considered necessary for familiarization with the stimulus-population.

Group I. The degree of judged similarity between standard and comparison-stimuli was estimated by measuring the distance of the mark from the extreme left of the line to the nearest 1/10th of an inch. In this way each response received a score ranging from 0 to 20.

There was no reliable difference between men and women, and Table I contains the mean scores of the total group for each stimulus. Starting from

TABLE I

EMPIRICAL AND PREDICTED VALUES OBTAINED FROM JUDGING DISTANCE ALONG A LINE (GROUP I)

I Stimuli	II Empirical estimates	III Effective empirical estimates	IV Non euclidean predictions	V Euclidean predictions	VI Independence predictions
+	1.2				
A	12.7	11.5			
B	2.2	1.0			
C	1.8	0.6			
D	3.2	2.0			
E	4.1	2.9			
AB	12.5		13.7	12.7	11.2
AC	15.3		13.3	12.7	11.2
AD	14.2		14.7	12.9	11.8
AE	12.5		15.6	13.4	12.2
BC	3.9		2.8	2.4	2.8
BD	3.0		4.2	3.5	4.0
BE	4.5		5.1	4.3	5.0
CD	2.7		3.8	3.3	3.8
CE	4.0		4.7	4.2	7.6
DE	5.4		6.1	4.7	6.0
AB	5.0		6.7	4.8	6.4
AC	5.9		7.1	4.9	6.6
AD	4.6		5.7	4.3	5.6
AE	4.0		4.8	3.5	4.6
BC	12.4		17.6	13.2	13.2
BD	15.3		16.2	13.1	12.6
BE	15.4		15.3	12.9	12.2
CD	15.7		16.6	13.1	12.6
CE	13.8		15.7	12.9	12.2
DE	15.1		14.3	12.8	11.4
A*	13.9		7.7	4.2	7.2
B	14.5		18.2	13.2	13.4
C	15.0		18.6	13.3	13.4
D	16.1		17.2	13.1	14.2
E	15.1		16.3	12.9	12.4
—	16.1		19.2	13.3	13.8

* $N = 19$

the top of Table I the first stimulus (+) is identical with the standard. The next five stimuli each differ from the standard in one respect. Thus *A*– differs in terms of pitch only, *B*– in terms of interruption rate, *C*– in amplitude modulation, *D*– in noise-amplitude, and *E*– in the band-width of noise. These five stimuli will, for reasons which will be evident later, be termed the "predictor stimuli." The next 10 stimuli differ from the standard in two respects, the following 10 in three respects, the next 5 in four respects, and the last stimulus differs in all 5 respects from the standard.

The adequacy of the three models will be judged by their ability to predict the empirical values from $A-B-$ to $-$ by considering only the values attributed to the "predictor" stimuli. The distances or numbers judged to represent the similarity of the predictor to the standard will be considered to represent distance along five separate coördinates. The three models will make different use of these judged distances.

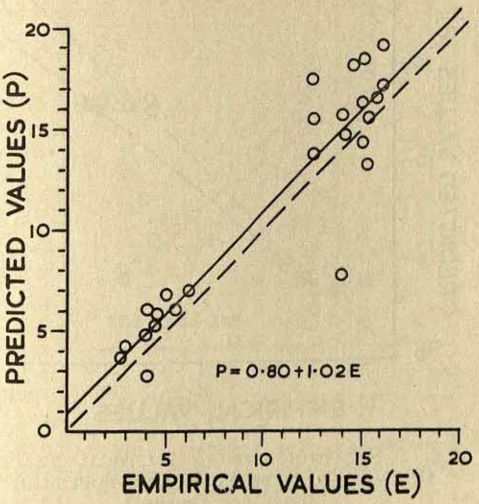

Fig. 1. Relation between Empirical Estimates of Distance along the Response-Line and Values Predicted on the Non-Euclidean Model in Group I

Adequacy of the models. The non-Euclidean model as used by Attneave would predict that Stimulus $A-B-$ would be judged to lie at that point $(a + b)$ away from the standard, *i.e.* at the sum of the distances chosen for Stimulus $A-$ and Stimulus $B-$. Table I shows, however, that the test-stimulus which was identical with the standard was itself judged by the group to lie 1.2 units from the standard. It is therefore necessary to subtract this value from the estimates for the five predictor stimuli.

This computation results in the Effective Empirical Estimates, shown in Column III of Table I. The predictions shown in Column IV are arrived at by adding 1.2 to the sum of the appropriate effective estimates. Thus, Stimulus $A-B-$ for example should lie at a distance $(1.2 + a_1 + b_1)$ *i.e.* 1.2 +

11.5 + 1.0 = 13.7 units from the end of the line. Where a_1 and b_1 are the effective estimates. Fig. 1 shows the relation between empirical (E) and predicted (P) distance estimated by the non-Euclidean model. The broken diagonal represents perfect prediction and the solid line the best fitting linear relationship, computed by the method of least squares. The two lines can be seen to agree very closely.

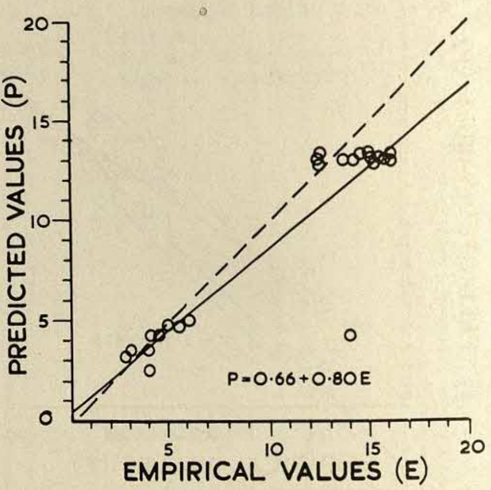

FIG. 2. RELATION BETWEEN EMPIRICAL VALUES AND VALUES PREDICTED ON THE EUCLIDEAN MODEL FOR GROUP I

The Euclidean model predicts that Stimulus $A-B-$ would be judged to lie at a distance $1.2 + (a_1^2 + b_1^2)^{1/2}$ from $A+B+$. In Fig. 2 the empirical distances are plotted against the Euclidean predictions. The best-fit line is perhaps less satisfactory than that shown in Fig. 1, for the non-Euclidean model, since it has both a positive intercept and a slope less than unity.

The application of the Independence model depends upon the assumption that the judged distance between standard and comparison-stimulus is an inverse measure of the probability that the comparison would be considered identical with the standard. Thus, a judged distance of 2 units from the zero point represents 0.1 of the distance between 0 and 20 so that the probability of confusing this test sound with the standard would be 0.9. A probability-value of this kind was obtained for each predictor stimuli.

It was shown previously (Corcoran), with stimulus-material similar to that used in the present experiment, that the probability of calling $A-B\ A+B+$ could be estimated by the equation

$$P_{A-B-} = P_{A-B+} \times P_{A+B-}/P_{A+B+}$$

where P_{A-B+} is in the probability that Stimulus $A-B+$ will be called $A+B+$, P_{A+B-} is the probability that $A+B-$ will be called $A+B+$ and P_{A+B+} is the probability of correctly identifying $A+B+$. The model has the general form:

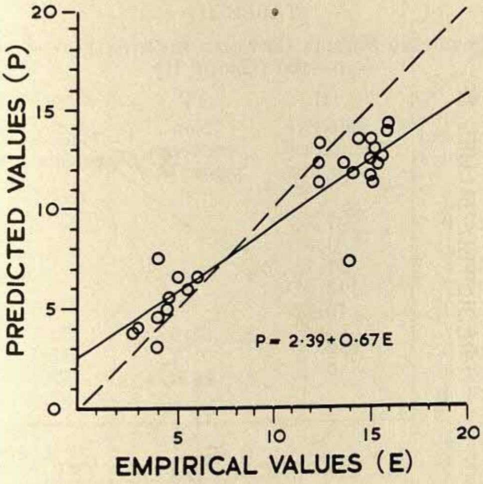

FIG. 3. RELATION BETWEEN EMPIRICAL VALUES AND VALUES PREDICTED ON THE INDEPENDENCE MODEL FOR GROUP I

$$P_{X-Y-\cdots Z-} = (P_{X-Y+\cdots Z}{}^{+} \cdot P_{X+Y-\cdots Z+} \cdot P_{X+Y+\cdots Z-})/(P_{X+Y+\cdots Z+})i-1$$

where X, Y and Z are separate dimensions and i is the number of such dimensions. The adequacy of this model may be judged from Fig. 3.

Each prediction on each model was subtracted from the corresponding empirical judgment regardless of sign and these values were compared. No reliable differences were evident.

Group II. The predictions of the non-Euclidean model bore much the same relationship to the empirical values in Group II as in Group I. In both groups the slope of the P/E relationship was near to unity, but Group II had the larger intersect (see Figs. 1 and 4). When reduced to the same units as Group I, the best linear fit for Group II reads $P = 0.193 + 0.98\,E$ as compared with $P = 0.80 + 1.02\,E$ in Group I.

In Group I, the predictions of the Euclidean and Independence models tended to over-estimate the judgments made to the stimuli which were similar to the Standard, while over-estimating the judgments to the less similar stimuli. Much the same relationships occurred in Group II. The similarity of the P/E plots of Group I to Group II may be judged by comparing Figs. 2 and 5 and Figs. 3 and 6.

TABLE II

EMPIRICAL AND PREDICTED MODELS OBTAINED FROM SELECTING A NUMBER FROM 0—100 (GROUP II)

I Stimuli	II Empirical estimates	III Effective empirical estimates	IV Non euclidean predictions	V Euclidean predictions	VI Independence predictions
+	2.2				
A	50.0	47.8			
B	6.1	3.9			
C	13.3	11.1			
D	17.2	15.0			
E	18.3	16.1			
AB	51.7		53.9	50.2	48.7
AC	65.0		65.1	51.3	52.5
AD	64.4		65.0	52.3	54.6
AE	64.4		66.1	52.6	55.2
BC	21.1		17.2	14.0	12.7
BD	12.4		21.1	17.7	16.5
BE	16.1		22.2	18.8	17.6
CD	19.4		28.3	20.9	22.7
CE	8.2		29.4	21.8	23.7
DE	40.0		33.3	24.2	27.1
AB	34.1		44.4	26.9	33.7
AC	36.1		37.2	24.6	28.3
AD	29.5		33.3	22.2	25.0
AE	19.7		3.2	21.3	24.1
BC	63.9		81.1	54.9	61.0
BD	51.7		77.2	53.9	59.3
BE	66.7		76.1	53.5	58.8
CD	55.0		70.0	53.2	56.0
CE	72.2		68.9	52.3	55.3
DE	61.1		65.0	51.5	53.3
A	60.6		48.3	27.2	30.9
B	52.8		92.2	56.0	64.6
C	74.4		85.0	55.0	61.8
D	71.1		81.1	54.0	60.0
E	67.2		80.0	53.7	59.4
–	67.2		96.1	56.1	65.1

As with Group I, no one model was superior in predictability to any other. It is perhaps legitimate, however, to consider the *P/E* relationships which might have existed had the stimuli been composed of six or more dimensions rather than five. The present evidence suggests that both the Euclidean and Independence models would diverge more from the empirical judgments as the number of dimensions increased, whereas the divergence of the non-Euclidean predictions from the empirical values remain about constant, owing to its slope of unity.

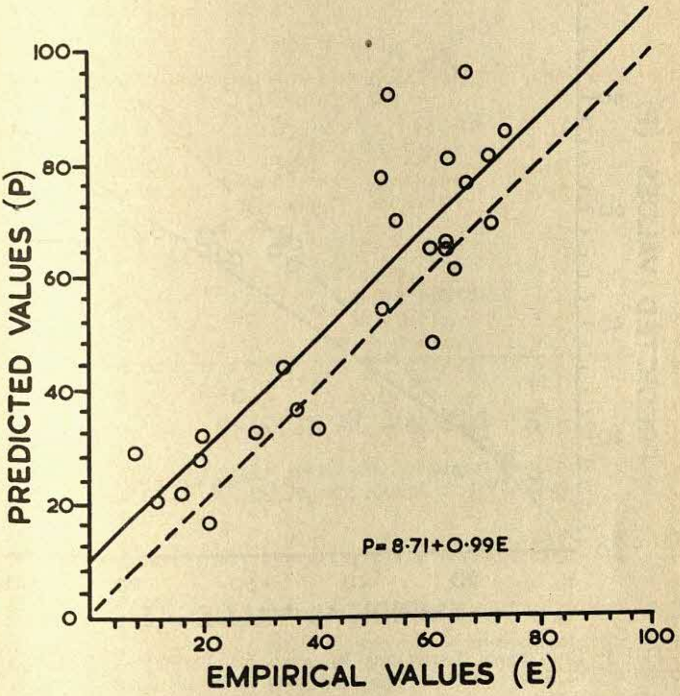

FIG. 4. RELATION BETWEEN EMPIRICAL ESTIMATES AND VALUES PREDICTED ON THE NON-EUCLIDEAN MODEL FOR GROUP II

Referring to the questions posed in the Introduction, we may draw the following conclusions: (1) Since no differences in predictability existed between the models, it is not possible to determine whether auditory space is similar to visual space, to semantic space or to neither; (2) The non-spatial model was not found to be reliably inferior to the spatial ones and it cannot therefore be concluded with any confidence that spatial models are or are not useful relative to spatial ones, and (3) Since the *E/P* plots of Group I were very similar to those of Group II, it would seem likely that the nature of the space was unaffected by the way in which *S*s communicated their experience. If this finding has generality it implies that future spatial model builders may design situations without regard to the actual response measures taken and may refer legitimately to experiments in which response measures different to their own were used.

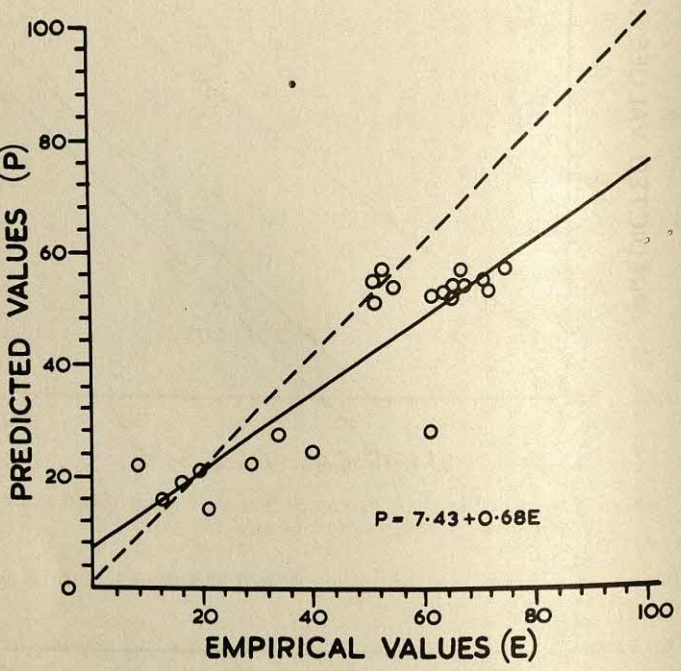

FIG. 5. EUCLIDEAN ESTIMATES RELATED TO EMPIRICAL ESTIMATES FOR GROUP II

SUMMARY

Auditory stimuli were synthesized by dichotomizing the dimensions of Pitch *A*, interruption-rate *B*, amplitude-modulation, *C*, amplitude of tone above masking noise *D*, and nature of the noise, *E*.

Each of the 32 stimuli was compared with a standard drawn from the population and 73 *S*s indicated the similarity between the standard and

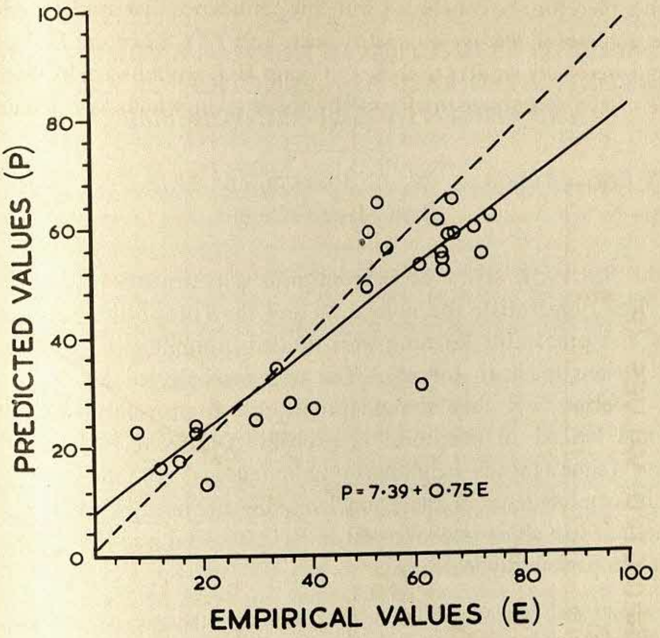

FIG. 6. RELATION BETWEEN EMPIRICAL ESTIMATES AND PREDICTIONS FROM THE INDEPENDENCE MODEL FOR GROUP II

comparison stimulus by distance along a line (Group I) or by a number (Group II).

Two spatial and one probability-models were used to predict the judgments to stimuli differing in two or more dimensions from the standard, by considering the judgments to the stimuli differing in a single respect.

The implications of the findings to the usefulness of spatial models of similarity was discussed.

RESISTANCE TO EXTINCTION IN NORMAL AND EXTENSIVELY DECORTICATED RATS AS A FUNCTION OF AMOUNT AND PERCENTAGE OF REINFORCEMENT

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In the first of a series of neurosurgical experiments patterned after those which differentiate the normal rat and the (pre-cortical) fish, habit-reversal and probability-learning were studied in adult rats with extensive cortical lesions made in infancy.[1] The operates behaved like normals in spatial problems, *i.e.* they showed progressive improvement in habit-reversal and tended to maximize in probability-learning. Only in visual problems (simultaneous brightness-discriminations) did the role of the cortex become evident. In these problems, the operates behaved like fish, *i.e.* they failed to show improvement in habit-reversal and showed random matching in probability-learning.

Normal rats and fish also differ to some extent in the manner in which their resistance to extinction is affected by partial reinforcement. In normal rats, resistance to extinction is increased by partial reinforcement (*PRE*) whether trials are massed or spaced.[2] In fish, by contrast, the *PRE* appears in massed trials,[3] but not in spaced trials.[4] The results for fish require nothing beyond a sensory-carryover interpretation,[5] but the spaced-trials effect in rats suggests the operation of some additional process, the cortical basis of which was explored in the present

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[1] R. C. Gonzalez, W. A. Roberts, and M. E. Bitterman, Learning in adult rats with extensive cortical lesions made in infancy, this JOURNAL, 77, 1964, 547-562.

[2] Solomon Weinstock, Resistance to extinction of a running response following partial reinforcement under widely spaced trails, *J. comp. physiol. Psychol.*, 47, 1954, 318-322.

[3] R. C. Gonzalez, R. M. Eskin, and M. E. Bitterman, Extinction in the fish after partial and consistent reinforcement with number of reinforcements equated, *ibid.*, 55, 1962, 381-386.

[4] Nicholas Longo and M. E. Bitterman, The effect of partial reinforcement with spaced practice on resistance to extinction in the fish, ibid, 53, 1960, 169-172; R. C. Gonzalez, E. R. Behrend, and M. E. Bitterman, Partial reinforcement in the fish: Experiments with spaced trials and partial delay, this JOURNAL, 78, 1965, 198-207.

[5] V. F. Sheffield, Extinction as a function of partial reinforcement and distribution of practice, *J. exp. Psychol.*, 39, 1949, 511-526; C. L. Hull, *A Behavior System*, 1952, 120-121; E. J. Capaldi, Effect of N-length, number of different N-lengths, and number of reinforcements on resistance to extinction, *J. exp. Psychol.*, 68, 1964, 230-239.

experiment. Since amount of reward has been shown to be an important parameter in spaced-trials experiments,[6] amount of reward and schedule of reinforcement were combined in a 2 × 2 design, both for operated rats and for normal controls.

METHOD

Subjects. The *Ss* were 82 albino rats of both sexes—40 operates and 42 normals—bred in the laboratory from stock of the CF-Nelson strain.

Surgical and histological procedures. The surgical procedure has been described in detail elsewhere.[7] Cortical tissue was removed by aspiration through fine glass tubing when the animals were 15-17 da. old. The operates were returned to their mothers about an hour after surgery. Five days later, they were housed in individual cages in the main colony-room of the laboratory, where they were maintained on a free-feeding schedule until the age of 90 da. The normal animals were reared in the same way as the operates.

Upon completion of the experiment, the operates were sacrificed, and their brains were removed, frozen, sectioned at 40 micra, and stained. Every tenth section was mounted and examined for evidence of subcortical damage. The sections for each animal then were examined to determine, for each of four experimental groups, the largest lesion, the smallest lesion, and the two lesions of intermediate extent. Then these lesions were reconstructed and plotted on conventional Lashley-diagrams.

Apparatus. The apparatus consisted of a gray start-box (9 in. long × 4 in. wide), runway (60 in. × 4 in.), and goal-box (12 in. × 6 in.). Each unit was made of wood, with walls 6 in. high, and had a hinged Plexiglas cover. Attached to the back wall of the goal-box was a food-cup, the contents of which could not be seen by *S* from the runway. A manually operated guillotine door (D_1) controlled entrance into the runway from the start-box. A second (sliding) door (D_2), which served to prevent retracing from the goal-box, was set into the runway 10 in. before the goal-box. Set into one wall of the runway were two photocells, one 6 in. beyond D_1, and the other 1 in. before the goal-box, which were illuminated by small lamps set into the opposite wall. The raising of D_1 tripped a microswitch mounted on the door-frame which activated a Standard Electric timer. When the first light-beam was broken by *S*, the photocell-relay stopped the first timer and activated a second, which was stopped by interruption of the second light-beam. Latency and running-times thus were measured separately in units of 0.01 sec.

Procedure. The experiment was conducted in two phases. First, 40 *Ss*—20 operates and 20 normals—were studied under the low-reward (five 0.045-gm. Noyes pellets) condition. The experiment then was replicated with 42 *Ss*—20 operates and 22 normals—under the high-reward (15 0.097-gm. pellets) condition.

Preliminary training. At the age of 90 da., the animals were placed on a 24-hr. feeding schedule, handled daily, and reduced gradually to 85% of their satiated bodily weights. When the reduction in weight was completed, the animals were given experience with the pellets of food which were to serve as reward in the experiment: they were fed pellets once in their individual home cages, and then, on each of three successive days, on a table in the laboratory in the course of handling. On each of the next three days, every *S* was fed in the goal-box with D_2 closed. Finally, every *S* was given eight inforced trials, one per day, on each of which latency and running-time were recorded. On the basis of their performance during these eight days, each group—the 20 low-reward operates, the 20 low-reward normals, the 20 high-reward operates, and the 22 high-reward normals—was divided into two matched subgroups: a 100% subgroup and a 50% subgroup.

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[6] S. H. Hulse, Jr., Amount and percentage of reinforcement and duration of goal confinement in conditioning and extinction, *ibid.*, 56, 1958, 48-57; A. R. Wagner, Effects of amount and percentage of reinforcement and number of acquisition trials on conditioning and extinction, *ibid.*, 62, 1961, 234-242.

[7] Gonzalez, Roberts, and Bitterman, *op. cit.*, 549.

Experimental training. Throughout the experiment-proper, each *S* was given one trial per day. In the first stage of the experiment, which lasted 40 days, the 100% groups had 40 reinforced trials, while the 50% groups had 20 reinforced and 20 unreinforced trials in accordance with selected Gellermann-orders. Each trial began with the opening of D_1 by *E* and terminated with the interruption of the second light-beam by *S*, after which *E* closed D_2. On reinforced trials, *S* was removed from the goal-box as soon as it had eaten the food and returned to its home-cage. On unreinforced trials, *S* was removed from the goal-box 20 sec. after having entered it. One hour after its daily trial, each *S* was fed the remainder of its daily ration in its home-cage. In the second stage of the experiment, all *S*s were given 30 unreinforced trials, one per day. These trials were identical in all respects with the unreinforced trials of Stage 1.

RESULTS

Normal controls. The performance of the four groups on the last 24 training trials is plotted in Fig. 1. In the 50% as well as in the 100% animals, latencies and running-times were significantly lower with the larger reward. (For latency, $F = 11.9$, $df = 1/37$, $p < 0.01$; for running time, $F = 68.9$, $df = 1/37$, $p < 0.01$.) These results duplicate those of previous experiments.[8] An effect of reinforcement-schedule on asymptotic performance in acquisition was evident only in longer running-times for the 50% animals under the low-reward condition. (For latency, $F < 1$; for running-time, $F = 4.9$, $df = 1/37$, $p < 0.05$. Subsequent orthogonal comparisons showed significance only for the difference between the two low-reward groups.) With respect to the effects of partial reinforcement on asymptotic performance in acquisition, there is, of course, some discrepancy in the literature. In this experiment, as in Hulse's experiment, there was no indication of superior performance in the 50% groups.[9]

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[8] Hulse, *op. cit.*, 48-57; Wagner, *op. cit.*, 234-242.

[9] Hulse (*op. cit.*, 48-57) found superior performance for consistently reinforced animals in all segments of the runway, but experiments by Wagner (*op. cit.*, 234-242), K. P. Goodrich (Performance in different segments of an instrumental response chain as a function of reinforcement schedule, *J. exp. Psychol.*, 57, 1959, 57-63) and D. F. Haggard (Acquisition of a simple running response as a function of partial and continuous schedules of reinforcement, *Psychol. Rec.*, 9, 1959, 11-18)

In Fig. 2, the course of extinction in the four groups is given in trial-by-trial plots of latency and running-time. The curves show more rapid extinction for high than for low reward, and a *PRE* for high but not for low reward. Statistical analyses of the latency-data support these generalizations. (For amount of reward, $F = 6.08$, $df = 1/37$, $p < 0.05$; for schedule, $F = 3.52$, $df = 1/37$, $p < 0.05$, with Scheffé tests showing significance only for the difference between the two high-reward groups; for the interaction of schedule with amount of reward, $F = 4.50$, $df = 1/37$, $p < 0.05$.) The analysis of running-times, which showed the same effects less markedly, yielded significant interactions of trials with amount of reward ($F = 3.36$, $df = 29/1073$, $p < 0.01$) and with schedule of reinforcement ($F = 1.79$, $df = 29/1073$, $p < 0.05$). Previous work by Hulse and by Wagner also showed an increase in the *PRE* with amount of reward,

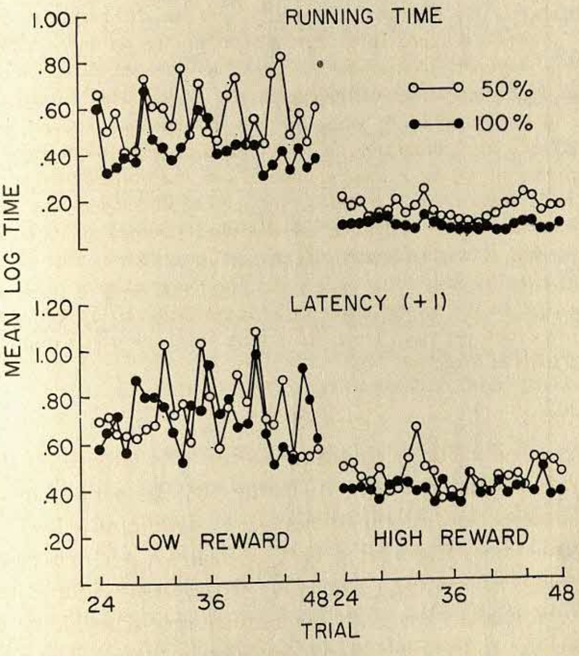

FIG. 1. MEAN LOG LATENCY (+1) AND MEAN LOG RUNNING-TIME ON EACH OF THE LAST 25 TRIALS OF ACQUISITION FOR THE FOUR GROUPS OF NORMAL RATS

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showed superior performance for consistently reinforced animals in later segments, but superior performance for partially reinforced animals in early segments.

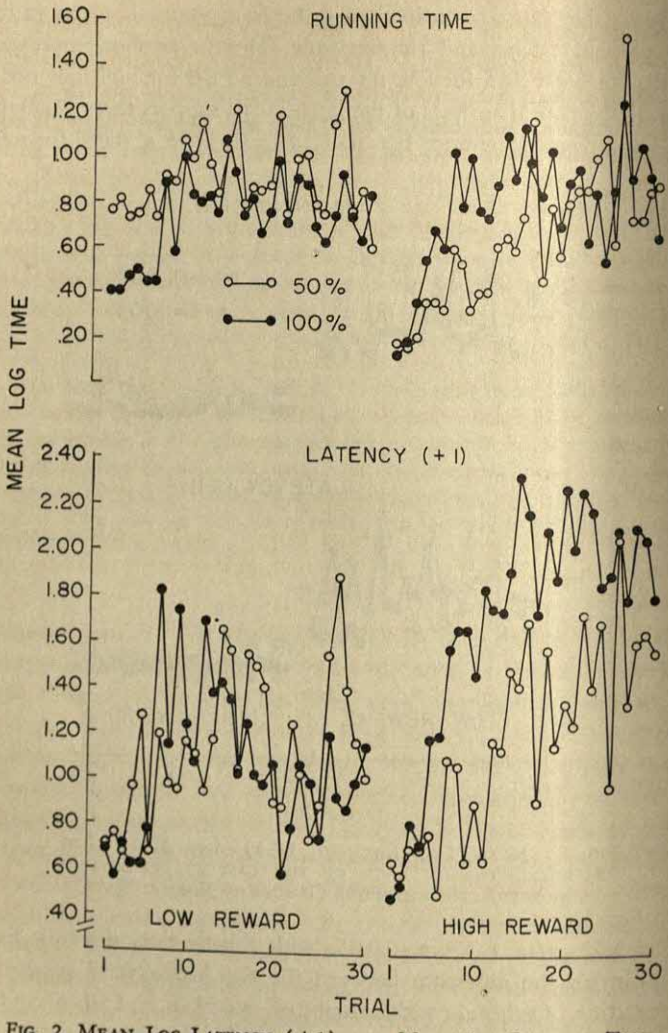

FIG. 2. MEAN LOG LATENCY (+1) AND MEAN LOG RUNNING-TIME ON EACH TRIAL OF EXTINCTION FOR THE FOUR GROUPS OF NORMAL RATS

but the relation among their groups was rather different.[10] Neither Hulse nor Wagner found a main effect of amount of reward. In their experiments, a larger reward increased resistance to extinction in 50% animals but decreased resistance in 100% animals, a pattern of results which follows from frustration-theory.[11] In the present experiment, resistance to extinction decreased with amount of reward in 50% as well as in 100% animals, but more markedly in the 100% animals, a pattern of results which follows from the discrimination-hypothesis.

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[10] Hulse, *op. cit.*, 48-57; Wagner, *op. cit.*, 234-242.

The discrepancy between the present results and the earlier ones of Hulse and of Wagner may perhaps be due to the difference in the absolute magnitudes of reward which were employed. In the earlier experiments, the low reward was 0.08 gm. of food (rather than the present 0.23 gm.) and the high reward was 1.0 gm. (rather than the present 1.46 gm.). Suppose that, for each schedule of reinforcement, resistance to extinction is a nonmonotonic function of amount of reward, increasing at first to a maximum at some relatively small amount, and then decreasing. Suppose also that the 50% function reaches its maximum at a somewhat larger amount of reward than the 100% function and declines less rapidly thereafter. On these assumptions, hypothetical curves can be drawn which give the Hulse-Wagner results for 0.08 and 1.0 gm., but the present results for 0.23 and 1.46 gm. An experiment designed to map these functions would have considerable value.

Operates. The performance of the operates in the last 24 training trials is shown in Fig. 3.[12] Unlike the normal controls, the operates showed an effect of amount of reward only with the partial schedule. (For latency, $F = 6.48$, $df = 1/31$, $p < 0.05$; for running time, $F = 12.97$, $p < 0.01$. Subsequent Scheffé tests showed significance only for the differences between the two 50% groups.) Another way to describe the difference is to say that the high-reward operates did not respond as quickly as the high-reward normals, and this formulation suggests that there simply may have been some sort of motor impairment in the operates. The curves of Fig. 3 show also that in the operates, as in the normals, partial reinforcement impaired asymptotic performance in the low-reward but not in the high-reward case; the effect was shown by the operates in both measures. (For latency, $F = 4.17$, $df = 1/31$, $p < 0.05$; for running-time, $F = 5.33$, $p < 0.05$. Scheffé tests showed significant differences only between the two low-reward groups.)

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[11] Abram Amsel, The role of frustrative nonreward in noncontinuous reward situations, *Psychol. Bull.*, 55, 1958, 102-119.

[12] The lesions produced in the operates were very much the same as those of the previous experiment with rats extensively decorticated in infancy (Gonzalez, Roberts, and Bitterman, *op. cit.*, 551). The smallest lesions in the four groups ranged in size from 45.8-47.1% of the total neocortical surface; the four largest, from 65.3-68.7%; the eight intermediate, from 54.2-57.9%.

In Fig. 4, the course of extinction in the four groups of operates is shown in trial-by-trial plots of latency and running-time. Like the normal controls, the operates show the *PRE* with high reward but not with low reward. For neither measure was the variance due to schedule statistically significant, but the interaction of schedule with amount of reward was significant in each case (for latency, $F = 5.12$, $df = 1/31$, $p < 0.05$; for running-time, $F = 8.43$, $p < 0.01$, and subsequent Scheffé tests showed the differences between 50% and 100% groups to be significant only with high reward.

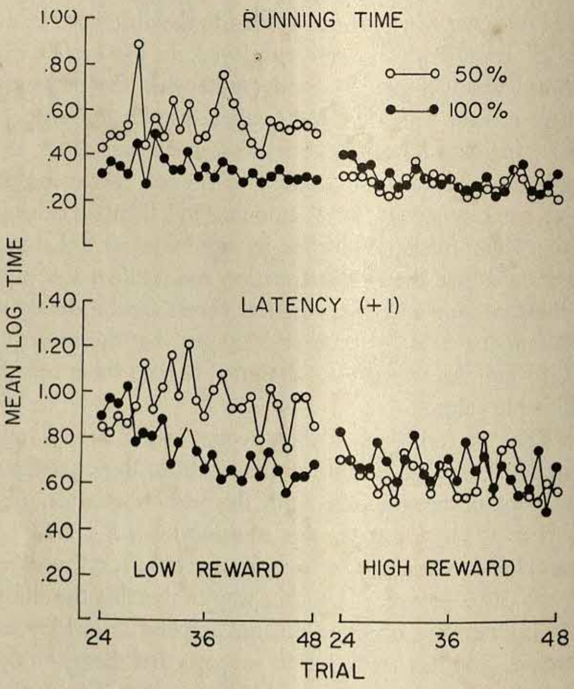

FIG. 3. MEAN LOG LATENCY (+1 AND MEAN LOG RUNNING-TIME ON EACH OF THE LAST 25 TRIALS OF ACQUSITION FOR THE FOUR GROUPS OF OPERATES

These results provide a clear answer to the question which prompted this experiment. The fact that the operates showed the *PRE* in spaced trials means that extensive decortication in infancy did not produce a fish-like mode of adjustment to partial reinforcement. That the operation did, however, have some effect is shown by certain striking differences in the curves of Fig. 2 and 4. In the 100% operates, increased reward did not decrease resistance to extinction as it did in the 100% normals of this experiment and of the previous experiments by Hulse and by Wagner. The resistance

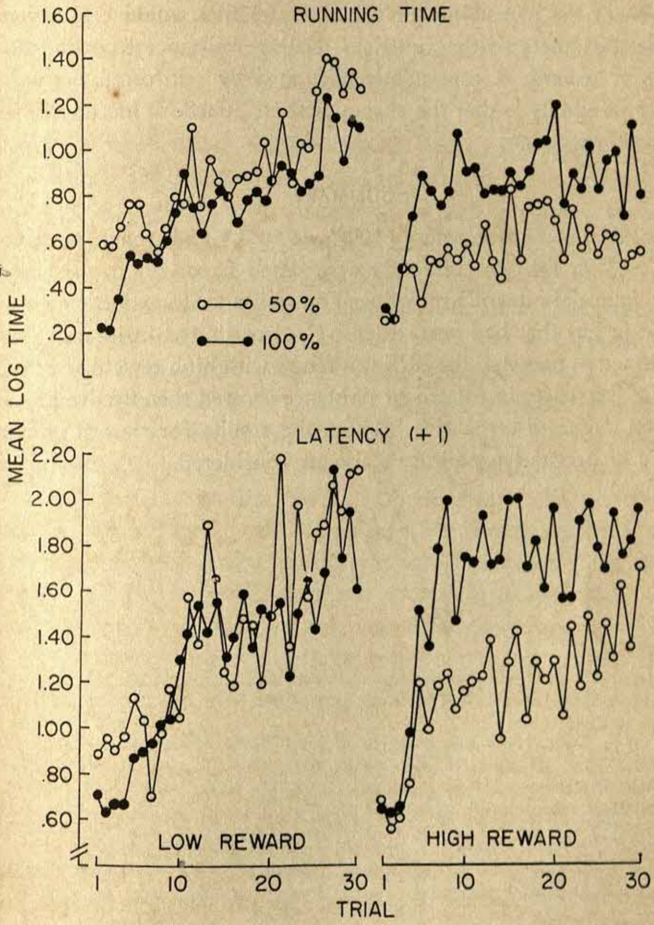

FIG. 4. MEAN LOG LATENCY (+1) AND MEAN LOG RUNNING-TIME ON EACH TRIAL OF EXTINCTION FOR THE FOUR GROUPS OF OPERATES

of the operates, both 50% and 100%, was already very low with the lower reward—much less than that of the normals. In general, resistance to extinction in the operates tended to increase as amount of reward increased, more so with 50% than with 100% reinforcement, while resistance to extinction in the normals tended to decrease as amount of reward increased, more so with 100% than with 50% reinforcement. It is conceivable that these differences may be accounted for in great part by ablation-produced changes in the incentive-values of the amounts of reward employed. Some indication of the plausibility of this interpretation would be provided by more detailed plots of the functions relating resistance to extinction and amount of reward in consistently and partially reinforced normal rats. Another possibility is that the shapes of those functions are themselves altered by the operation.

SUMMARY

Consistency of reinforcement (100% *vs.* 50%) and amount of reinforcement (0.23 *vs.* 1.46 gm. of food) were varied factorially in a spaced-trials (24-hr. intertrial interval) experiment on resistance to extinction in normal rats and in rats that had been extensively decorticated in infancy. Both in normals and in operates, the *PRE* was found with high reward and not with low, but differences in pattern of resistance showed the effectiveness of the operation. Some discrepancies between the results for normal animals of this and of previous experiments also are considered.

SERIAL REACTION-TIME AND THE TEMPORAL PATTERN OF PRIOR SIGNALS

By WILLIAM BEVAN, LLOYD L. AVANT, and H. G. LANKFORD,
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Classical experiments on reaction-time have focused on the isolated stimulus-response event. Modern studies, in contrast, increasingly have viewed performance as a reflection of variables that function across a sequence of stimulus-response events. Of particular interest to current investigators, for both practical and theoretical reasons, is the relationship between response-time and the regularity of signal occurrence. A number of experiments report that reaction-time is increased by the introduction of variation into the schedule of presentation intervals.[1] The same relationship holds in the literature on the related area of vigilance. Signal detection is more efficient for regular than for irregular schedules.[2]

The prevalent idiom for discussion of these relationships is probabilistic. In the case of the reaction-time literature, the language is frequently that of information theory.[3] In the case of vigilance, the widely influential concept of expectancy has been linked habitually to signal probability. For example, Colquhoun and Baddeley treat vigilance decrement in terms of a drop in signal probability.[4] And while Deese and Baker both emphasize the im-

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[1] See, *e.g.* Jack Botwinick and Joseph F. Brinley, An analysis of set in relation to reaction time, *J. exp. Psychol.*, 63, 1962, 568-574; E. R. F. W. Crossman, Entropy and choice-time: The effect of frequency unbalance on choice-response, *Quart. J. exp. Psychol.*, 5, 1953, 41-51; Lawrence Karlin, Reaction-time as a function of foreperiod duration and variability, *J. exp. Psychol.*, 58, 1959, 185-191; Edmund T. Klemmer, Time uncertainty in simple reaction-time, *J. exp. Psychol.*, 51, 1956, 179-184; J. Alfred Leonard, The effects of 'machine' lag on a serial choice task with balanced and biased input frequencies, *Ergonomics*, 2, 1958, 44-51; P. D. McCormack and A. W. Prysiazniuk, Reaction-time and regularity of inter-stimulus interval, *Percept. mot. Skills*, 13, 1961, 15-18.

[2] See, *e.g.* C. H. Baker, Attention to visual displays during a vigilance task: II. Maintaining the level of vigilance, *Brit. J. Psychol.*, 50, 1959a, 30-36; W. P. Colquhoun, The effect of 'unwanted' signals on performance in a vigilance task, *Ergonomics*, 4, 1961, 41-51; W. P. Colquhoun and A. D. Baddeley, Role of pretest expectancy in vigilance decrement, *J. exp. Psychol.*, 68, 1964, 156-160.

[3] Crossman, *op. cit.*, 41-51; E. T. Klemmer, Simple reaction-time as a function of time uncertainty, *J. exp. Psychol.*, 54, 1957, 195-200; Wendell R. Garner, *Uncertainty and Structure as Psychological Concepts*, 1962, Ch. 2.

[4] Colquhoun and Baddeley, *op. cit.*, 156-160.

portance of mean level of expectancy, associated with a process of averaging, both also view as implicit in their treatment the importance of the relative probability of occurrence of the several intervals that make up the signal program.[5] Thus Baker explains the function of knowledge of results as providing "nothing more than knowledge of the true nature of the temporal structure of a series in order that observers can make accurate temporal extrapolations."[6]

The definition of expectancy in terms of probable occurrences requires two further assumptions: (a) The modal interval, as the most probable duration between signals, is logically identified as the best estimate of the average expectancy-value; (b) since relative frequency of occurrence of the several scheduled intervals is the significant property of the stimulus-program, expectancy necessarily must change with significant changes in the shape of the distribution of intervals.

Given, however, another conceptual orientation, neither of these assumptions, nor the tie between them, remains necessary. The pooling model of adaptation-level specifies the mean of the several series intervals as the best estimate of average expectancy-level, and relative frequency is important only as it influences this integrated magnitude. Thus, if mean, range, and number of interval-sizes are held constant, serial reaction-time should not vary as a function of the shape of the distribution of presentation-intervals. In contrast, a simple relative frequency-model, given the same circumstances, must expect differences among the distributions as the position and relative frequency associated with the mode varies. It further is presented with an ambiguous situation for prediction when confronted by rectangular and bimodal distributions. The purpose of this experiment is to compare average response-times for temporal series having different statistical structure and also response-times to test intervals of mean and other durations *following* series of different structure.

It is interesting to note that while there has been general agreement about the importance of series-structure, there has been some variation in what is meant by this concept. In most studies the mean interval and the number of interval-sizes employed have been held constant. Some experiments simply have compared data for constant and variable interval schedules; however, the variable interval schedules have been normal,[7] rectangular,[8] and skewed.[9] When variable schedules have been compared, it has been usual for mean and shape to have been held constant while range has been varied.[10] A few have allowed mean and variability,[11] or even mean, shape and variability to vary together.[12] In one earlier experiment, shape was the crucial variable.[13]

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[5] James Deese, Some problems in the theory of vigilance, *Psychol. Rev.*, 62, 1955, 359-368; Baker, Toward a theory of vigilance, *Canad. J. Psychol.*, 13, 1959b, 35-42.

[6] Baker, *op. cit.*, 1959b, 40.

[7] William Bevan, Donald L. Hardesty, and Lloyd L. Avant, Response latency with constant and variable interval schedules, *Percept. mot. Skills*, 20, 1965, 969-972; Donald Hardesty and William Bevan, Response latency as a function of the temporal pattern of stimulation, *Psychol. Rec.*, 15, 1965, 385-392.

[8] Karlin, *op. cit.*, 185; Jack A. Adams and Lawrence R. Boulter, Spatial and temporal uncertainty as determinants of vigilance behavior, *J. exp. Psychol.*, 67, 1964, 127-131.

[9] Baker, *op. cit.*, 1959a, 30-32.

[10] McCormack and Prysiazniuk, *op. cit.*, 15, L. R. Boulter and Jack A. Adams,

EXPERIMENTAL SITUATION

Experimental design. A simple independent groups design was used. Each of five equal-sized groups received a different temporal pattern of signals. One, the control group (C), received a program of 20 signals at constant intervals. The others received one or another variable-interval schedule: Group N, a normal distribution of intervals; Group R, a rectangular distribution; Group B, a bimodal; and Group S, a skewed distribution.

Table I presents the number of trials and the relative frequency with which the several presentation intervals were used.

In addition to the 20 signals of the adaptation-series each *S* received one additional trial as a test-trial. For half the *Ss* in each group this was after an interval of 20 sec.; for a fourth, after an interval of 10 sec.; and for the remaining fourth, after an interval of 5 sec.

TABLE I

THE NUMBER OF TRIALS AND RELATIVE FREQUENCY WITH WHICH EACH PRESENTATION-INTERVAL APPEARED IN THE SEVERAL ADAPTATION-SERIES

	Number of presentation-intervals in sec.										
Group	10		14		20		26		30		Ave. in sec.
Constant (C)	–	—	–	—	20	100%	–	—	–	—	20.00
Normal (N)	2	10%	4	20%	8	40%	4	20%	2	10%	20.00
Rectangular (R)	4	20%	4	20%	4	20%	4	20%	4	20%	20.00
Bimodal (B)	6	30%	3	15%	2	10%	3	15%	6	30%	20.00
*Skewed (S)	5	25%	5	25%	1	5%	2	10%	7	35%	20.25

* To maintain the mean at approximately 20 sec. with 20 signals in the skewed distribution, durations of 15 and 25 sec. replaced the 14 and 26 sec. intervals.

Subjects. Two hundred undergraduate students, men, served as *Ss*, 40 in each major group. The subgroups for test purposes contained 20, 10 and 10 *Ss*, respectively.

Apparatus. The experimental task required the detection of the onset of a small amber light. The visual display consisted of a large black screen, separating *S* and *E*, with a 0.5 in. circular aperture at approximately eye-level. A small pilot lamp behind an amber filter provided the signal illumination. A silent (low friction) switch mounted on *E*'s side of the display, when activated, simultaneously illuminated the signal-lamp and started a Standard Electric Clock. A similar switch,

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Vigilence decrement, the expectancy hypothesis, and intersignal interval, *Canad. J. Psychol.*, 17, 1963, 201-209. J. F. Dardano, Relationships of intermittent noise, intersignal interval, and skin conductance to vigilance behavior, *J. appl. Psychol.*, 46, 1962, 106-114.

[11] Klemmer, *opp. citt.*, 1956, 179; 1957, 195-196.

[12] James J. McGrath and Albert Harabedian, Signal detection as a function of intersignal-interval duration in Donald N. Buckner and James J. McGrath (eds.), *Vigilance: A Symposium*, 1963, Ch. 8.

[13] Leonard, *op. cit.*, 45.

mounted on a wooden apron on *S*'s side of the screen, when pressed, extinguished the light and stopped the clock. Response-latencies were read to 1/100 sec. White noise was fed through head-phones to mask possible extraneous noise.

Procedure. S was seated before the display and asked to remove his watch. Next he was told that he was participating in a reaction-time experiment. It was explained that a light would come on periodically and that he was to keep his preferred hand on the switch in front of him so that he could turn the light off as quickly as possible. Then he was given the 20 adaptation- and 1 test-trials. The order of presentation for each series was random.

RESULTS

Variable versus constant interval series. In an earlier study, Bevan, Hardesty and Avant compared a constant-interval series and a normally distributed variable-interval series at six different mean interval values.[14] In five of the six cases, the variable-interval series had a larger average response latency than the companion constant-interval series. For the constant condition, mean time varied from about 270 m.sec. for the shortest interval to about 390 for the longest. Comparable values for the variable condition ranged from about 300 to 430 m.sec. The difference between constant and variable schedules was approximately 25 m.sec. at the shortest mean duration and about 40 at the longest. These relationships were not confirmed in the present data. The largest mean *RL* was found in the constant interval group. The shortest occurred in the normal and the skewed variable interval-groups. The reason for this failure of confirmation is unclear, although the present results are in line with those of Boulter and Adams who compared a constant interval series with rectangular distributions covering different ranges.[15] These results, of course, also are consistent with Adaptation-Level Theory, which identifies central tendency rather than dispersion as the significant variable in the determination of response.

The importance of central tendency is indicated further by an analysis of response-latencies for each of the interval-durations used in the variable adaptation-series. Fig. 1 suggests the shortest response-times to be associated with the 20-sec. period, thus identifying the series mean as the expected duration on each successive trial. Statistical analysis revealed a significant difference between the intervals source of variance [F_{interval} $(4,624) = 2.76$; $p < 0.05$] with one-tailed t-tests (39 df) indicating clear significance between the 20-sec. and all other intervals except the 30-sec. interval ($t_{20\text{-}10} = 2.87$, $p < 0.005$; $t_{20\text{-}14} = 3.12$, $p < 0.005$; $t_{20\text{-}26}$

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[14] Bevan, Hardesty, and Avant, *op. cit.*, 970.
[15] Boulter and Adams, *op. cit.*, 202-204.

$= 2.07$, $p < 0.025$; $t_{20\text{-}30} = 1.32$, $p < 0.10$). The small F for distributions unequivocally points out the irrelevance of relative frequency as a determinant of mean response latency [$F_{\text{distributions}}$ (3,156) $= 0.63$; $p > 0.05$].

Response-latency and the shape of the distribution of variable intervals.

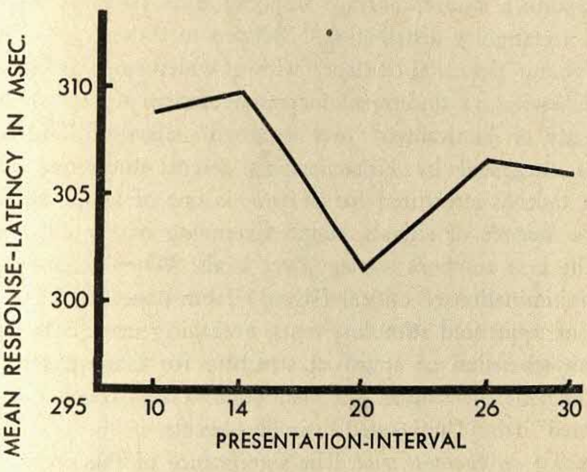

FIG. 1. MEAN LATENCIES OF RESPONSE FOR EACH OF THE SEVERAL INTERVAL-DURATIONS USED IN THE ADAPTATION-SERIES

TABLE II

GROUP MEANS FOR RESPONSE-LATENCY AND VARIABILITY IN RESPONSE-LATENCY FOR THE SEVERAL ADAPTATION-SERIES

Group	Mean *RL* in m.sec.	Mean series *SD*	Mean range for 20 sec. interval
C	316	46.9	169
N	304	36.9	105
R	309	48.1	84
B	311	41.2	49
S	304	36.7	

Column 2 of Table II presents the mean response latency for the several adaptation-series. The over-all magnitude of these response values are typical of those found with this sort of task.[16] Moreover, there are *no* reliable differences between constant and variable schedules, as noted above, nor between any combination of the variable schedules [over-all F $(4, 195) = 0.52$; $p > 0.05$]. The contention of Adaptation-Level Theory thus would appear to be confirmed. At the same time the discrepancy between these and earlier findings demand reconciliation. Klemmer and Dardano, comparing rectangular distributions, both found reaction-time to increase with foreperiod and intersignal range, respectively; while Leonard reports a shorter average response time for a skewed as compared to a rectangular distribution.[17] Related to these results are Baker's report of poorer detection-efficiency with a wider-range skewed distribution and Colquhoun's finding of increased efficiency with an increase in the frequency of "unwanted" over "wanted" signals.[18] The key to the solution is suggested by Kristofferson's recent studies of attention.[19] Where the task, as structured for or by *S*, is one of simply detecting the presence or absence of signals, simple averaging of the critical property provides the best response strategy over trials. When, however, the task requires discrimination of critical (signal) from non-critical (non-signal) properties of sequenced stimulus-events, averaging must be supplemented by the comprehension of statistical structure for efficient responding to occur. In each of the studies just mentioned, *S* was required to differentiate "wanted" from "unwanted" stimulus-events.

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[16] Klemmer, 1956, *op. cit.*, 180-182; Karlin, *op. cit.*, 186-89; Bevan, Hardesty, and Avant, *op. cit.*, 970-972.

Performance on the test-trial. The significance of the simple averaging of criterion magnitudes as a principle of response is demonstrated by performance on the test-trials. Here it is seen (Fig. 2) that the decrements in response-latency are smallest when the test interval coincides with the mean of the adaptation-series [$F_{\text{between intervals}}$ $(2,185) = 9.16$, $p < 0.001$], a relationship uninfluenced by the statistical structure of this series [$F_{\text{between group}}$ $(4,185) = 2.28$, $p > 0.05$; $F_{\text{group x interval}}$ $(2,185) = 1.57$, $p > 0.05$]. The data are especially impressive for the bimodal and skewed distributions, for in these sequences the mean interval occured only twice and once respectively while 45 and 50% of all trials had intervals closer than the mean to the test-values. These data are in agreement with the results of Turner and Bevan who report no difference in average response latency to a test-trial at the series mean when the series were constant interval, normal variable, rectangular variable, and rectangular variable transformed over trials to a constant interval schedule.[20] They also confirm Hardesty and Bevan's finding that ΔRL *approximates* zero for a test-interval of mean duration and is significantly larger for both shorter and longer intervals.[21] Recently Bevan, Bell, and Taylor have found the same relationship to hold when the criterion variable was pitch—in contrast to duration of presentation interval—on the test-trial.[22]

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[17] Klemmer 1956, *op. cit.*, 180-182; Dardano, *op. cit.*, 105-118; Leonard, *op. cit.*, 96-98.

[18] Baker, *op. cit.*, 1959a, 32-33; Colquhoun, *op. cit.*, 94-98.

[19] Alfred B. Kristofferson, Attention in time discrimination and reaction-time, NASA CR-194, 1965.

[20] Edward D. Turner and William Bevan, Response latency as a function of the statistical structure of presentation intervals, *J. gen. Psychol.*, (in press).

Variability in the series responses. Although the magnitude of the series norm—given an invariant input mean, range, and set of unit sizes—is uninfluenced by the frequency-distribution of the input variable, it is possible that statistical structure may relate in some other significant fashion to responsivity. For example, Bevan and Saugstad report that while differences in unit-size had no effect upon a judgmental norm, it

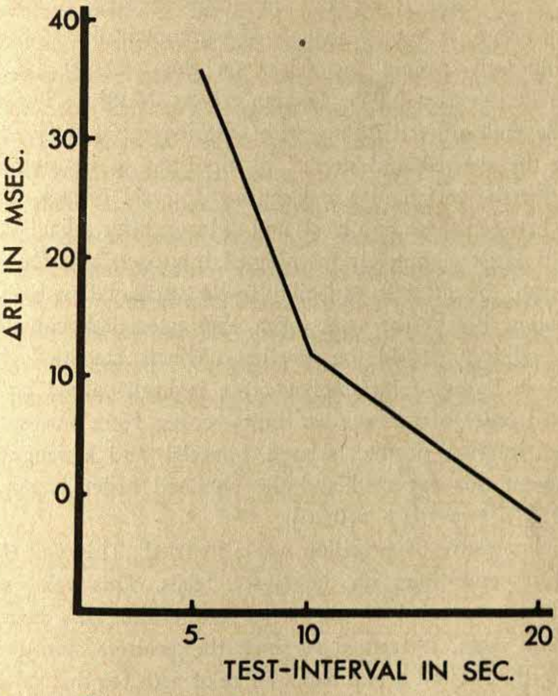

FIG. 2. DECREMENT IN LATENCY OF RESPONSE ON THE TEST-TRIAL AS A FUNCTION OF THE DURATION OF THE TEST-INTERVAL
MEAN DURATION OF THE PRESENTATION-INTERVAL DURING THE ADAPTATION-SERIES WAS 20 SEC.

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[21] Hardesty and Bevan, *op. cit.*, 388-389.

[22] William Bevan, Russell A. Bell, and Curtis Taylor, Changes in response latency following shifts in the pitch of a signal, *Kansas State Univ., ONR tech. Report. No. 36, 1965.*

did affect the clarity with which the judgemental scale was articulated, as inferred from the variability of each judge's response about his own mean.[23] They found that while training on an easy discrimination had no effect upon the adaptation-level for a more difficult task, it did result in more precise judgements than those following training on a task of equal difficulty to the test-task.

Column 3 of Table II contains the group averages of the individual *SD* in each group. A simple analysis of variance indicated the groups to differ significantly among themselves on this measure [$F_{\text{between group}}$ $(4{,}195) = 11.59$, $p < 0.05$]. Duncan's New Multiple Range Test applied to the rank-ordered means, with criterion set at 0.05, showed variability for the skewed and normal distributions to be significantly less than that for the rectangular and constant-interval groups. The bimodal group fell between these two levels and did not differ reliably from either. Such results resist completely meaningful interpretation. One reasonably may anticipate the greatest variability to be associated with the rectangular distribution, but by the same token, that associated with the constant-interval condition should be smallest. When statistical structure is thought of in terms of its ambiguity for a single-valued response-logic, the expected order of average variability scores, from smallest to largest, is: constant interval, normal, skewed, bimodal, and rectangular. The relation between this expected and the obtained order is not significant [$\rho = 0.30; S(d^2) = 14; p > 0.20$].

A second measure of precision was also tried. This was the range of individual response-times on the 20-sec. trials. This value was selected because it corresponded to the series mean and also because, as the middle most value, it seemed to show the greatest spread of response times. Since the skewed group had a range of zero for this value it was not included in the comparison. The rightmost column of Table II contains these values. Again, analysis of variance revealed a significant between-groups source [$F_{\text{between groups}}$ $(3{,}156) = 28.24$; $p < 0.001$]. Duncan's New Multiple Range Test, with criterion set at 0.001, indicated the constant interval to differ from both the rectangular and bimodal groups, and the bimodel from all others. Again, the significance of the order is obscure (it correlates with the expected order negatively: [$\rho = 0.80$; $S(d^2) = 18$, $p > 0.10$], except that it is perfectly negatively correlated with the frequency of 20-sec. trials.

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[23] William Bevan and Per Saugstad, Breadth of experience, ease of discrimination, and efficiency of generalization, *Brit. J. Psychol.*, 46, 1955, 13-19.

SUMMARY

This experiment examined the relationship between response-latency to serially-presented simple visual signals and the frequency-distribution of presentation-intervals within the stimulus-series, when mean duration, range of duration, and the number of different sized intervals was held constant. A total of 200 *S*s (men) were tested with the following types of distributions: constant interval; normal variable interval; skewed variable interval; bimodel variable interval, and rectangular interval. No differences among the series means were obtained for any of the distributions. Response-times were shortest, however, for the mean as compared to the other intervals used in the adaptation-series. *S*s also were given one additional test-trial. Response-latencies were shortest when this test-interval corresponded to the mean of the series, and was longer as the test-interval deviated from the mean. Again, the statistical structure had no relationship to response on the test-trial. Variability of response differed among the several types of distribution, but the significance of this variability is obscure. These results emphasize the role of the mean interval—in contrast to the statistical shape of the interval program—as a determinant of the occurrence of signals on successive trials.

METHOD, STANDARD DURATION, AND INTER-STIMULUS DELAY AS INFLUENCES UPON JUDGMENT OF TIME

By W. J. RICHARDS and P. V. LIVINGSTON, University of Arkansas

Perception of the passage of time is a psychological process which has attracted much experimental study in recent years, as is indicated by the number of studies summarized by Wallace and Rabin.[1] One finding concerning time-perception seems to be well established. Woodrow cites evidence to support the statement: "In general, the just noticeable difference (j.n.d.) is smallest at some relatively short magnitude or range of magnitudes and increases both above and below this middle region."[2]

Andreas suggests that ability to discriminate short intervals more accurately than long ones might result from the fact that less retention is demanded of *S*. That is, "for the comparison of longer intervals, a subject has to retain his perception of the first interval until the second one is terminated."[3] He hypothesizes that if short intervals were separated by a compensatory inter-stimulus interval, judgments would be comparable in accuracy to those for long intervals.

Kowalski attempted to determine the influence of varying delay-intervals upon the duplication of short temporal intervals.[4] Delay-interval or inter-stimulus interval may be defined as that amount of time which elapses between the end of the first stimulus and the starting of the second stimulus. Seven delay-intervals ranging from 2.5 to 30 sec., and seven stimulus-durations ranging from 0.48 to 16.2 sec. were completely randomized and presented to 30 high school *S*s. His results supported the conclusion that stimulus-duration was a more important determinant of over- or under-estimation of a temporal interval than was interstimulus-interval. He also found a tendency for accuracy of time-duplication to increase with increase of the delay-interval, although the trend was nonsignificant.

It would appear from Kowalski's work that not only is length of the delay-interval of doubtful importance, but that accuracy of reproduction is at least as good, if not better, under the longer delay intervals used than it is with a short delay.

A second study bearing on the question of the effect of inter-stimulus interval on estimation of time was done by Fox.[5] He investigated the effects of different delay-

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* Received for publication October 18, 1965.

[1] Melvin Wallace and A. I. Rabin, Temporal experience, *Psychol. Bull.*, 57, 1960, 213-236.

[2] Herbert Woodrow, Time perception, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1951, 1224.

[3] B. G. Andreas, *Experimental Psychology*, 1960, 332.

[4] W. J. Kowalski, The effect of delay upon the duplication of short temporal intervals, *J. exp. Psychol.*, 33, 1943, 239-246.

[5] G. J. Fox, The effect of interval and duration of visual stimuli upon the time error, *Psychol. Newsltr.*, 45, 1952, 1-17.

intervals and different durations of visual stimuli upon the time-error and variability of response in a successive comparison of stimulus-durations. Three stimulus-durations from 2 to 6 sec. and five delay-intervals from 1 to 8 sec. were used. Two trained *Ss* made judgments of these combinations of stimulus-duration and delay-interval by the method of constant stimuli. Fox concluded that the time-error changed from negative to positive as delay-interval increased, and that stimulus-duration was not an important influence on *S*'s judgment.

Thus, Fox and Kowalski have drawn contradictory conclusions regarding the influence of the delay interval on time-perception. A possible explanation for the contradiction lies in the fact that Kowalski used the method of reproduction while Fox used the method of comparison (*i.e.* constant stimuli).

Bindra and Waksberg describe the methods commonly used in studies of time perception.[6] They define the method of comparison as being a variation of the method of reproduction, inasmuch as both involve an operative presentation of the standard and a judgment which also refers to an operatively delimited duration. This conclusion may be correct, but it should be supported by experimental evidence.

The method of comparison readily lends itself to studies of the perception of time. It allows stimuli to be presented in random order, and eliminates judgments based upon objective time-units (*i.e.* both intervals may be presented without mention of a conventional time-unit). It would be desirable to have data relating this method to the method of reproduction, since the latter method is probably most frequently used in time studies.

The present study used two methods, reproduction and limits, and the same levels of standard and delay for each method. The method of limits differs from the method of constant stimuli employed by Fox, but both methods require *S* to make judgments of successive comparison. Thus, one purpose of the study was to compare accuracy of time-estimation as measured by the two methods.

There are two aspects of accuracy in discrimination. One may ask how closely *S*'s estimate approaches the objective value of the standard, or he may ask how variable are *S*'s judgments around his own mean estimate. The first concept of accuracy is measured by *S*'s constant error; the second by an evaluation of the variability of *S*'s judgments. A value for the constant error (*CE*) may be determined both for the method of reproduction and for the method of limits. As a measure of variability one may use the semi-interquartile range (*SIQ*) for the method of reproduction and the DL for the method of limits. These measures seem most appropriate since time data are usually skewed.

A second purpose of the present study was to test, using both methods, Andreas' hypothesis concerning the effect of a compensatory delay interval on judgments of short temporal intervals. He hypothesized that if a compensatory delay-interval were introduced between the standard and comparison stimulus, accuracy of estimation of short durations would not differ significantly from those for long durations.

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[6] Dalbir Bindra and Hélène Waksberg, Method and terminology in studies of time estimation, *Psychol. Bull.*, 53, 1956, 155-159.

Method. (1) Subjects There were 32 *S*s, 13 male and 19 female volunteers from undergraduate psychology classes. Ages ranged from 18 to 49 yr, with over 80% falling within a range of 18 to 25 yr. All were naïve with respect to the studies of time-estimation.

(2) Apparatus Stimulus-durations and delay-intervals were controlled by three Hunter Model 111A interval-timers. A Hunter Model 120 Klock-Kounter served to record *S*'s reproductions and to monitor time-intervals when the method of limits was used. Recording equipment was located in a room adjoining *S*'s, but *E* and *S* could see one another through a glass panel.

Equipment in *S*'s room consisted of a flat black plywood panel containing a white stimulus-light, a red warning light, and a warning buzzer. A telegraph-key was attached to the table and was used by *S* to give his time-reproductions. A microphone and speaker were used for communication between *E* and *S*. A fan served to mask distracting sounds from outside the room.

Procedure Each *S* was tested with each of the three stimulus-durations, one of the two methods, and only one delay-interval. The method to which each *S* was assigned was determined by the flip of a coin. Only one delay interval was used with each *S* in order to avoid an overly long session. The interval used for a given *S* was drawn randomly, without replacement. Therefore, four *S*s were required for one replication under each method. The *S*s were given six practice trials with a stimulus-duration and delay-interval not used in the experiment proper.

Four delay-intervals (2, 8, 16, 25 sec.) were used with each of three stimulus-durations (2, 8, 16 sec.). All combinations of stimulus-duration and delay-interval were used with each method, making a total of 24 different sets of conditions.

Method of limits At the start of each session *S* was told that a warning buzzer would sound before the beginning of each trial. He was instructed to judge the second duration in terms of the first. He was also instructed not to count or use any aid to measure the intervals.

Four series of stimuli counterbalanced for ascending or descending order were used. The order of presentation of standard and comparison was randomly determined with the restriction that each must appear an equal number of times in first and second positions.

(3) Method of reproduction Instructions were given concerning the warning buzzer. The stimulus-light was turned on for the appropriate duration. After the correct delay interval, the red warning light flashed to indicate to *S* that he should begin his reproduction.

Each combination of stimulus-duration and delay-interval was presented 20 times.

Results. All data were analyzed with respect to the two types of measures of accuracy of discrimination mentioned earlier. Table I shows the mean *PSE*-values (the first measure of accuracy) obtained with the two methods. The values are expressed as percentages of the standard duration to make them comparable for different values of the standard.

Bartlett's test for homogeneity of variance yielded a Chi-square value significant beyond the 0.01 level. The test was then applied to the two methods separately. The test yielded, for the method of limits, a non-significant value; for the method of reproduction, a value significant beyond the 0.01 level. A square root transformation was made on the data for the method of reproduction and a Barlett's test performed on the transformed data. The Chi-square value obtained was not significant at the 0.05 level. A Barlett's test was then made for the transformed data of the two methods combined. Again, the value obtained was significant beyond the 0.01 level. Consequently, it was decided to analyze data for the two methods separately, using transformed data for the method of reproduction and original data for the method of limits.

TABLE I

MEAN PSE AS % OF THE STANDARD

Standard (in Sec.)	Delay (in Sec.)				
	2	8	16	25	M
Method of Reproduction					
2	105.613	96.163	104.237	106.837	103.212
8	98.717	99.695	95.047	99.387	98.212
16	89.377	95.240	92.125	104.970	95.428
M	97.902	97.033	97.136	103.731	
Method of Limits					
2	100.463	98.750	97.812	96.562	98.397
8	101.952	101.155	101.172	100.000	101.070
16	99.705	103.217	97.657	102.552	100.783
M	100.707	101.041	98.880	99.705	

The analysis of variance for the method of reproduction showed that only the standard duration contributed a significant amount to the total variance ($F = 4.734$; df 2, 24; $p < 0.05$). The same analysis for the method of limits showed the standard duration to be significant ($F = 3.920$; df 2, 24; $p < 0.05$) as well as the interaction between standard and delay interval ($F = 3.584$; df 6, 24; $p < 0.05$).

From the data on *PSE*s one may determine the value of the constant error (*CE*) for the various conditions. Fig. 1 shows the mean *CE* at each standard duration, *i.e.* the points represent the mean *CE* for the four delay intervals.

Table II shows *DL* and *SIQ* values for the methods respectively. The values shown are expressed as percentages of the standard duration.

Bartlett tests indicated heterogeneous variance for the data of the combined methods, and also showed that the data from the method of reproduction produced the major part of the total variance. Therefore, a square root transformation of the data for the two methods was made and an analysis of variance performed on the transformed data. The analysis showed only one factor to be significant, and this was the standard duration ($F = 7.346$; df 2, 48; $p < 0.01$).

Data from each method were also evaluated separately. Transformed data for the method of reproduction were used in the analysis of variance, and no significant F-values were obtained.

TABLE II

DL and SIQ as Percentage of the Standard

Standard (in Sec.)	Delay (in Sec.)				
	2	8	16	25	*M*
Semi-Interquartile Range					
2	12.77	17.75	18.81	20.75	17.52
8	10.98	17.09	10.86	11.79	12.68
16	14.27	8.15	13.23	13.43	12.27
M	12.67	14.33	14.30	15.32	
Difference Limen					
2	13.55	16.85	20.95	14.70	16.51
8	14.83	15.22	12.89	17.17	15.03
16	10.48	13.17	11.13	11.42	11.64
M	13.07	15.08	14.99	14.43	

Original data (*DLs* expressed as percentages of the standard) were used in the analysis for the method of limits. Only the standard duration contributed a significant amount to the total variance ($F = 7.143$; df 2, 24; $p < 0.01$).

Discussion. A comparison of the variances of *PSE* showed that the measures for the method of reproduction were considerably more variable than were those for the method of limits. This finding is understandable if one recognizes that the two methods are not equally sensitive to the large amount of variability in the judgments of the untrained *S*. When the method of limits is used, comparison-stimuli are paired with a standard and *S* is asked to judge the relationship of the second duration to the first duration. Comparson-stimuli are selected so *S* is always able to judge correctly stimuli at the extremes of the series. Since each ascending or descending series yields only one set of measures (*e.g. PSE* or *DL*) the contribution of any single judgment in the series is relatively small.

When the method of reproduction is used, each judgment of *S* contributes equally to the determination of the mean *PSE*. Further, there is no restriction on the setting which *S* may make, which is not true with the method of limits. Thus, it seems inevitable that the judgments of untrained *Ss* tested with the method of reproduction would be more variable than would those of *S*s tested with the method of limits.

The analysis of variance for the *PSE* data showed the standard stimulus duration contributed significantly to the total variance with both methods. The standard-delay interaction was significant only for the method of limits.

Although the delay-interval did not, according to the analysis of variance, contribute significantly to the total variance, the effects should be examined. Data for the method of reproduction averaged over the three standards show that the value of the *PSE* appeared to increase from the shortest to the longest delay interval (Table I). This suggests a change in the *CE* from a negative to a positive value. Although such changes were not statistically significant in either study, the changes in the *CE* observed in the present study were similar to those reported by Kowalski.[7]

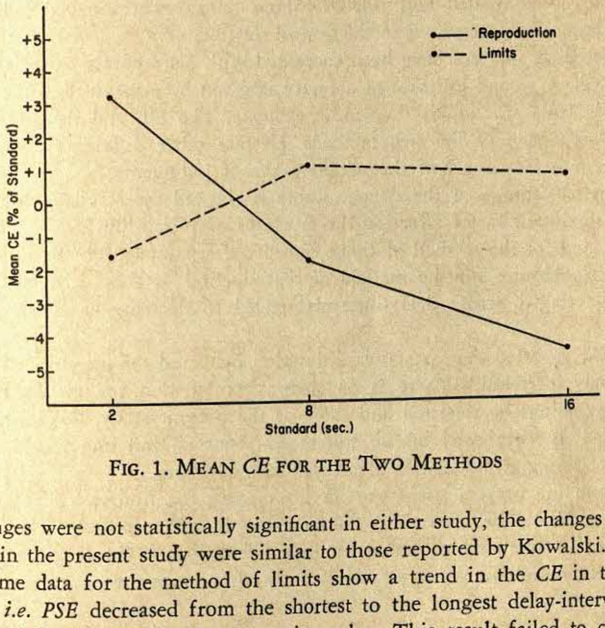

FIG. 1. MEAN *CE* FOR THE TWO METHODS

The same data for the method of limits show a trend in the *CE* in the reverse direction, *i.e. PSE* decreased from the shortest to the longest delay-interval, or the *CE* changed from a positive to a negative value. This result failed to confirm the finding of Fox who concluded that the value of the *CE* progressed from a negative to a positive value as length of the delay interval increased.[8] The two studies are, however, not directly comparable, since the longest delay-interval used by Fox was 8 sec. These statements concerning the effect of the delay-interval for the method of limits are somewhat oversimplified, since a significant interaction was found between the standard duration and the delay-interval.

The variable shown by the analysis of variance to affect significantly the results with both methods was the standard stimulus-duration. Fig. 1 shows the *CE* for each method plotted as a function of the standard duration. The curve showing the mean of the four delay intervals for the method of reproduction shows that the *CE* changes from a positive to a negative value as the stimulus-duration increases. This finding is consistent with the results obtained by other investigators that there is a tendency to overestimate the short durations (1 sec. or fraction thereof) and to underestimate the long ones.[9] In the present study the 2-sec. duration was overestimated. Prior studies have usually shown this duration to be underestimated. These facts suggest that the tendency to overestimate may begin somewhere between durations of 1 to 2 sec.

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[7] Kowalski, *op. cit.* 245ff.
[8] Fox, *op. cit.* 16ff.

The function for the method of limits shows a tendency to underestimate the 2-sec. duration and to overestimate the 8- and 16-sec. durations. This trend is in an opposite direction from that obtained with the method of reproduction, and is contrary to the general statement of Wallace and Rabin that relatively short durations tend to be overestimated and relatively long ones underestimated.[10] There have, however, been other exceptions to the general statement.[11]

The preceding analyses have been concerned with *S*'s accuracy as indicated by the *PSE* and *CE*. A second estimate of accuracy is given by a measure of the variability of *S*'s judgments around his own mean estimate. The *DL* and the *SIQ* were used for this comparison of the two methods. Despite other differences, each of these techniques measures one half the middle range of judgments.

Analysis of variance of the data in Table II showed the standard duration to be the only significant factor affecting the measures of variability for the two methods combined, and for the method of limits considered separately. The factor approached statistical significance for the method of reproduction. It should be noted that *DL* and *SIQ* averaged across delay-intervals tended to decrease as standard duration increased.

Woodrow reported that as standard duration increased the measures of variability progressively increased.[12] There is no obvious explanation for the discrepancy between the findings he reported and those of the present study. One difference was that trained *S*s were used in the studies he reported, but one would expect the effects of training to be similar at all standard durations.

Data from the present study provide no support for Andreas' hypothesis that a compensatory delay between short durations would make accuracy of judgment for these durations comparable to that for longer durations.[13] The length of the delay-interval was found not to affect significantly either measure of accuracy of judgment.

SUMMARY

Effects of method, standard duration, and delay interval upon time-estimation were measured by *PSE, DL,* and *SIQ*. The methods of limits and reproduction were employed. Thirty-two *S*s volunteered from undergraduate classes in psychology. Each *S* participated in one method, all

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[9] Kowalski, *op. cit.,* H. J. Smythe and Sanford Goldstone, The time sense: A normative, genetic study of the development of time perception. *Percept mot. Skills,* 7, 1957, 49-50.

[10] Wallace and Rabin, *op. cit.,* 221.

[11] Wallace and Rabin, *op. cit.,* 219.

[12] Woodrow, *op. cit.,* 1225.

[13] Andreas, *op. cit.,* 332.

standard durations, and one delay-interval at each standard duration. Standard durations of 2, 8, and 16 sec., and delay-intervals of 2, 8, 16, and 25 sec. were used. Major findings were: (1) Variance of the *PSE* for the method of reproduction was significantly greater than for the method of limits; (2) Standard stimulus duration affected judgments significantly for all measures except *SIQ;* (3) As length of standard duration increased, *CE* for the method of reproduction changed from positive to negative; for the method of limits, the trend was reversed; (4) Accuracy of discrimination as measured by *DL* and *SIQ* increased as standard duration increased.

AN EXACT TEST OF THE SIZE-DISTANCE INVARIANCE HYPOTHESIS

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An object o lying in a fronto-parallel plane, subtends an angle Θ at an observer. Its radial distance from him is d_o and its frontal size is s_o. It is sometimes asserted that the observer's (O's) visual space is so structured that, if s_o' and d'_o are the perceived frontal size and radial distance of o,

$$s_o'/d_o' = f(\Theta) \qquad [1]$$

where $f(\Theta)$ is a positive monotonic function. This equation carries the implication that the same ratio of perceived size to perceived distance occurs for all objects subtending the same visual angle.[1]

More specifically, many writers have suggested the relationship,

$$s_o'/d_o' = ks_o/d_o \qquad [2]$$[2]

Both Equations [1] and [2] have been called the *size-distance invariance hypothesis,* and both account for the easily demonstrated facts that where Θ is increasing, an object appearing to remain the same size appears to approach the observer and an object appearing to remain the same distance, appears to increase in size. Similarly they account for the fact that if two objects subtend the same visual angle, that which appears the further also appears the larger. The linear function of Equation [2] has the particular appeal for many authors, for example Ittelson,[3] that it implies that O's visual space is structured in accordance with the geometry of retinal perspective.

During the past 15 yr. numerous investigations have been concerned

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[1] W. H. Ittleson, *Visual Space Perception*. 1960, 58; R. P. McDonald and P. T. O'Hara, Size-distance invariance and perceptual constancy, this Journal, 77, 1964, 276-280; A. M. Hartman, Effect of reduction on the relationship between apparent size and distance, this Journal, 77, 1964, 353-366. None of these authors gives Equation [1]. This is, however, a convenient mathematical expression of the various principals suggested.

[2] Ittelson, *op. cit.*, 68; William Epstein, The known-size-apparent-distance hypothesis, this Journal, 1961, 74, 333-346; E. E. Rump, The relationship between perceived size and perceived distance, *Brit. J. Psychol.*, 52, 1961, 111-124; W. C. Gogel, E. R. Wist, and G. S. Harker, A test of the invariance of the ratio of perceived size to perceived distance, this Journal, 76, 1963, 537-553. For some of these authors. $k = 1$. This does not however, affect the argument. Some authors write Θ_o in place of S_o'/d_o' in Equation [2]. For small Θ these are, however, equivalent.

[3] Ittelson, *op. cit.*, 55-58.

with the way in which visual angle determines perceived sizes and distances. Many techniques have been employed for this purpose. For example, Over asked his *O*s to estimate the sizes and distances of objects in physical units.[4] Gogel, Wist, and Harker asked theirs to match the sizes and distances of objects kinesthetically.[5] Probably the most common procedure has been that of asking *O* to make visual size- and distance-matches.[6] The present discussion is concerned with this last technique.

The arrangement illustrated schematically in Fig. 1 will be taken as representative of that employed by investigators using this technique.

The central channel represents a *Standard* object. Its frontal size is s_{st} and its radial distance is d_{st}. The right hand channel represents a *Size variable*. An object of variable size is placed in this channel at a constant distance ($d_{va(s)}$). Its size is adjusted so that it appears equal to that of the variable. The setting of the Size Variable at the *PSE* will be symbolised as $s_{va(s)}$. The left hand channel represents a *Distance Variable*. An object of constant size ($s_{va(d)}$) may be varied in distance. This is so adjusted that it appears equal to the distance of the standard. The setting of the Distance Variable at the *PSE* will be symbolised as $d_{va(d)}$.

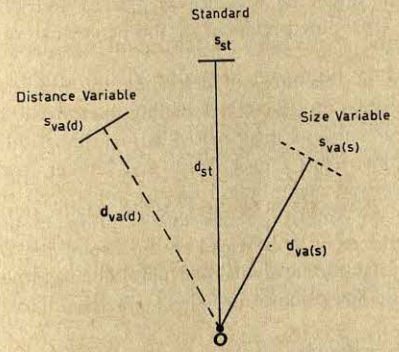

FIG. 1. A SCHEMATIC REPRESENTATION OF AN EXPERIMENT IN SIZE AND DISTANCE-PERCEPTION

The Standard is viewed under the conditions for which the hypothesis is to be tested. The Size-and Distance-Variables are usually viewed under conditions providing good cues to distance: they are placed in fields providing many perspective cues and are commonly viewed binocularly.

It is possible, on the basis of such data, to make weak tests of the size-distance-invariance hypothesis. For example, suppose that changes are made in the size and distance of the standard in such a way as to keep its visual angle constant. Then, if increases in $d_{va(d)}$ over the different conditions are accompanied by increases in $s_{va(s)}$, the data is in keeping with the hypothesis. Such considerations, however, do not enable a strict test to be made of Equations [1] or [2].

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[4] Ray Over, Size-and-distance-estimates of a single stimulus under different viewing conditions, this JOURNAL, 76, 1963, 452-457.

[5] Gogel, Wist, and Harker, *op. cit.*, this JOURNAL, 76, 1963, 552f.

[6] Epstein, *op. cit.*, 344f; Rump, *op. cit.*, 122f; Hartman, *op. cit.*, 365f.

Some authors have attempted to use such data to carry out a strict test. For example, Hartman used Standard objects subtending a constant visual angle.[7] He described $s_{va(s)}$ as the 'apparent size' of the standard and $d_{va(d)}$ as its 'apparent distance.' He then tested the relationship,

$$s_{va(s)}/d_{va(d)} = constant \qquad [3]$$

and regarded this as a test of Equation [1]. Similarly, Rump tested Equation [3] under conditions of constant visual angle, by testing for a linear relationship between $s_{va(s)}$ and $d_{va(d)}$.[8] Extending this procedure to situations in which Θ is not constant, the relation

$$s_{va(s)}/d_{va(d)} = cs_{st}/d_{st}, \qquad [4]$$

where c is a constant, might be regarded as a strict test of Equation [2]. The question arises as to what interpretations of the perceptual variables would justify such procedures.

The common usage in this area, of describing the physical magnitudes of the size- and distance-variables as the corresponding 'perceived' or 'apparent' magnitudes of the standard, suggests that perceptual and physical magnitudes are regarded as commensurate. Thus Gogel, Wist, and Harker, consider the possibility of an identity of perceived and physical distance in a well structured field. They reject this possibility on empirical rather than *a priori* grounds.[9] Tests of Equations [3] or [4] would seem to be based on an assumption that within the channels containing the variables, perceptual and physical magnitudes are identical. (In this case of course, the size-distance hypothesis *must* hold for at least these conditions).

Two numerical interpretations of 'perceived' sizes and distances suggest themselves. First, perceived spatial extents might be defined as *O*'s estimates in physical units, of physical spatial magnitudes. In this case the procedures described might be based on the assumption that an *O* asked to estimate, say in inches, the physical magnitude of $d_{va(d)}$ and $s_{va(s)}$, would do so correctly. There is evidence, however, that even in well structured fields, estimates of physical magnitudes may be systematically in error. Thus Gogel, Hartman, and Harker found, under such conditions, a tendency to underestimate distances.[10] Moreover, their data imply a degree of nonlinearity in the relationship between estimated and physical distance.

Secondly, 'perceived spatial extents' might be interpreted as values on a judgmental scale. Under this interpretation different physical extents are characterised by numbers which represent the observer's quasi-quantititive judgments of these extents. Typically, such scales are obtained by fractionation or equisection. The scale values assigned to various physical extents by such techniques are not unique and the unit of the scale is independent of the unit of physical extent. In this case, an assumption that perceived and physical extents are identical, is illegitimate on purely *a priori* grounds.

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[7] Hartman, *op. cit.*, 367.

[8] Rump, *op. cit.*, 124.

[9] Gogel, Wist, and Harker, *op. cit.*, 542.

[10] W. C. Gogel, B. D. Hartman, and G. S. Harker, The retinal size of a familiar object as a determinant of apparent distance, *Psychol. Monogr.*, 71, 1957, (No. 442) 1-16.

It will be seen that Equations [3], and [4] would provide legitimate exact tests of the size-distance invariance hypothesis, provided that certain assumptions held regarding the relationships of the physical and perceptual scales. It is possible, however, to carry out exact tests of Equation [2] using matching procedures without making such assumptions.

Consider any two objects, *a* and *b*, and assume that the linear form of the size-distance-invariance hypothesis holds. Then from Equation [2], $s_a'/d_a' = ks_a/d_a$ and, $d_a' = s_a'd_a/ks_a$; similarly, $d_b' = s_b'd_b/ks_b$. Dividing,

$$d_a'/d_a' = (s_a'/s_b')\ (d_as_b/d_bs_a). \qquad [4]$$

Suppose that *a* and *b* are such that *O* judges them to be equal in frontal size. Under both interpretations of perceived extent discussed above, they would be of the same perceived size $(s_a' = s_b')$.[11]

In this case, Equation [4] simplifies to,

$$d_a'/_b' = d_as_b/d_bs_a \qquad [5]$$

By a corresponding argument, if instead *a* and *b* are judged to lie at the same *distance*.

$$s_a'/s_b' = d_bs_a/d_as_b. \qquad [5a]$$

Consider now the three objects illustrated in Fig. 1 and suppose that, by the procedure described, size and distance matches have been made.

Then by Equation [5],

$$d_{st}'/d_{va(s)}' = d_{st}s_{va(s)}/d_{va(s)}s_{st}. \qquad [6]$$

Similarly by Equation [5a],

$$s_{st}'/s_{va(d)} = d_{va(d)}s_{st}/s_{va(d)}d_{st}. \qquad [6a]$$

But since $d_{va(d)}$ was obtained by making a distance match, $d_{st}' = d_{va(d)}'$. Thus substituting this equality into Equation [6],

$$d_{va(d)}'/d_{va(s)}' = d_{st}s_{va(s)}/d_{va(s)}s_{st}, \qquad [7]$$

The ratio $d'_{va(d)}/d'_{va(s)}$ may, however, be estimated independently, *provided that size-adjustments of the Size-Variable do not produce changes in its perceived distance.*

Suppose, maintaining the distance variable at the setting $d_{va(d)}$, that the Size Variable is so adjusted that it now appears the same size as the *Distance Variable.* This second setting of this variable will be designated by $\bar{s}_{va(s)}$. Then applying Equation [5],

$$d_{va(d)}'/d_{va(s)}' = d_{va(d)}\bar{s}_{va(s)}/d_{va(s)}s_{va(d)} \qquad [8]$$

Combining Equations [7] and [8],

$$d_{va(d)}\bar{s}_{va(s)}/d_{va(s)}s_{va(d)} = d_{st}s_{va(s)}/d_{va(s)}s_{st}$$

and,

$$\bar{s}_{va(s)} = s_{va(d)}d_{st}s_{va(s)}/s_{st}d_{va(d)} \qquad [9]$$

If this equality does not hold, the linear form of the size-distance-invariance hypothesis does not hold.

Alternatively, a similar test may be based on a second adjustment of the *Distance Variable.* Suppose that when the Distance Variable is adjusted, *no change occurs*

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[11] This is the case by definition where perceived extent is interpreted as value on a judgmental scale. In the case of the alternative interpretation, it may be assumed that the estimated physical magnitudes of extents that are judged equal, will be the same.

in its apparent size. Maintaining the setting of the Size Variable at $s_{va(s)}$, the Distance Variable is so adjusted that it appears at the same distance as the Size Variable. This setting will be designated $\bar{d}_{va(d)}$. Then, if the size-distance-invariance hypothesis holds, by an argument analogous to that employed above,

$$d_{va(d)} = d_{va(d)} d_{va(s)} s_{st} / d_{st} s_{va(s)} . \qquad [9a]^{12}$$

It is of interest to contrast Equations [9] and [9a] with Equation [4].

Equation [9] holds when it is possible to alter the Size Variable without changing its apparent size. In this case, re-arranging Equation [9],

$$s_{va(s)} / d_{va(d)} = (s_{st} / d_{st}) \ (\bar{s}_{va(s)} / s_{va(d)}) . \qquad [10]$$

If the size-distance-invariance hypothesis is correct, Equations [9] and [10] will hold. In this case therefore, Equation [4] can hold only if,

$$\bar{s}_{va(s)} / s_{va(d)} = c \qquad [11]$$

In any other case, an experimenter carrying out the usual procedure and testing Equation [4] will draw the incorrect conclusion that the size-distance-invariance hypothesis is false.

Under what circumstances will Equation [11] hold? In the hypothetical case under consideration, the size-distance-invariance hypothesis is correct. In this case, applying Equation [2],

$$\bar{s}_{va(s)} / s_{va(d)} = d_{va(d)}' d_{va(s)} / d_{va(s)}' d_{va(d)} . \qquad [12]$$

If therefore, Equation [11] holds,

$$d_{va(d)}' d_{va(s)} / d_{va(s)}' d_{va(d)} = c$$

and

$$d_{va(d)}' d_{va(s)}' = c d_{va(d)} / d_{va(s)} . \qquad [13]$$

That is to say that in the channel containing the Size Variable and that containing the Distance Variable, perceived distance is proportional to physical distance. But since the size-distance-invariance hypothesis holds in this hypothetical case, this implies that perceived size is proportional to physical size in each channel. The same conclusion follows from a consideration of Equation [9a].

Under these circumstances then, Equation [4] provides a valid test of Equation [2]. Similarly, it is only under these circumstances that Equation [3] provides a valid test of Equation [1]. There is evidence, however, that even when *O* is viewing a well structured visual field binocularly, perceived and physical distance are *not* proportional. Thus Gilinsky, and Weckowicz, Somner, and Hall all report that under such conditions, perceived distance is a negatively accelerating function of physical distance.[13] In general therefore, Equations [3] and [4] do not provide valid tests of the size-distance-invariance hypothesis.

Statistical considerations.

Consider Equation [9]; Since, in practice, an *O*'s judgments vary from trial to trial, the determinate values of the variables of Equation [9] must be replaced by statistical *expectations.* Thus, Equation [9] may be replaced by

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[12] Equation [2] is the special case of the power function $s_o'/d_o' = k(s_o/d_o)^a$ in which $a = 1$. It can easily be shown that if this relation held, Equations [9] and [9a] would hold irrespective of the value of *a*. The suggested technique therefore provides a test of this more general relationship.

[13] T. E. Weckowicz, R. Sommer, and R. Hall, Distance Constancy in Schizophrenic patients. *J. Ment. Sci.*, 104, 1958, 1174; A. S. Gilinsky, Perceived size and distance in visual space, *Psychol. Rev.*, 58, 1951, 460-482.

$$E(\bar{s}_{va(s)}) = (s_{va(d)}d_{st}/s_{st})\ [E(s_{va(s)})/E(d_{va(d)})] \qquad [14]$$

(The expected value of $\bar{s}_{va(s)}$ will be interpreted as the expectation of the size match *when the Distance Variable is set at the expectation of* $d_{va(d)}$).

A test of Equation [14] involves finding an unbiassed estimator for the ratio of expectations, $E(s_{va(s)})/E(d_{va(d)})$. It is more convenient to estimate a *product of* expectations and for this reason, to test the equivalent equation,

$$E(\bar{s}_{va(s)})E(d_{va(d)}) = s_{va(d)}d_{st}E(s_{va(s)})/s_{st}$$

or,

$$D = E(\bar{s}_{va(s)})E(d_{va(d)}) - s_{va(d)}d_{st}E(s_{va(s)})/s_{st} = 0 \qquad [15]$$

The mean $m_{s_{va(s)}}$, of a random sample of values of $s_{va(s)}$ is an unbiassed estimator of $E(s_{va(s)})$. Provided that any correlation between $\bar{s}_{\ va(s)}$ and $d_{va(d)}$ is at least approximately linear, an unbiassed estimator of the product of expectations, $E(\bar{s}_{va(s)})E(d_{va(d)})$ is obtained as follows. A value of $d_{va(d)}$ is obtained at random. Leaving the Distance Variables at this setting, a value of $\bar{s}_{va(s)}$ is obtained. This procedure is repeated n times. Where $m_{s_{va(s)}}$ is the mean value of $\bar{s}_{va(s)}$ and $m_{d_{va(d)}}$ is the mean value of $d_{va(d)}$, it can be shown that the required estimated is given by the expression $[m_{s_{va(s)}}m_{d_{va(d)}} - covar\ (\bar{s}_{va(s)}d_{va(d)})/n]$. It follows that an unbiassed estimator of D in Equation [15] is,

$$D = m_{s_{va(s)}}m_{d_{va(d)}} - covar\ (\bar{s}_{va(s)}d_{va(d)})/n \qquad [16]$$
$$-s_{va(d)}d_{st}m_{s_{va(s)}}/s_{st}$$

Mathematically, the unit of measurement of D is the square of the unit of spatial extent. Thus if $s_{va(s)}$ etc., are in inches, D is in square inches.

A corresponding modification of equation [9a] is required in any experimental application of the second suggested technique.

EXPERIMENT

The technique described in the preceding discussion may be illustrated by the following experiment. An attempt was made to establish whether the size-distance-invariance hypothesis holds in monocular visual space, by testing Equation [14]. The Standard Object and the Size Variable were both familiar objects viewed under reduced conditions and the Size Variable was an unfamiliar object viewed in a well structured field.

Apparatus. The experimental arrangement was that schematically illustrated in Fig. 1. Throughout the experiment, the Standard was placed to the left, the Distance Variable was placed centrally and the Size Variable, to the right. The Standard and Size Variables were separated by an angle of 25°. The Distance Variable lay on the bisector of this angle. The Size Variable was placed on a laboratory table. When looking at this object, O had an uninterrupted view of this table, the laboratory, and E.

The Standard and the Distance Variable were each placed in a reduction-tunnel. By a suitable combination of concealed lighting and screens it was possible to arrange that nothing else was visible in either tunnel. The tunnels were connected to a common darkened viewing chamber and could, without head movement, see both objects through a single aperature. All the O's reported that the two objects seemed to lie in a single, completely darkened, chamber.

In comparing the Size Variable with the objects in the tunnels, it was necessary for

O to move his head about 6 in. to the right. To ensure that the distances of the various objects did not alter during the course of the experiment, a pivoted headrest was placed against the back of the O's head. He was instructed to keep his head against this rest when moving.

The Standard object was the face of a matchbox cover. The longer side was 2.1 in. ($s_{va(d)} = 2.1$ in.). It was so placed that this side was vertical and its center lay in O's visual horizontal plane. The distance of the variable could be changed continuously between 50 and 210 in. The Distance Variable was a playing card. Its longest side was 3.5 in. ($s_{st} = 3.5$ in.). It was so positioned that the longest side was vertical and its center lay in O's visual horizontal plane.[4]

The Size-Variable was a flat black strip, 1 in. in width, mounted on the back of a small, white, vertical screen. The size of the visible portion of the strip could be increased by raising it from behind the screen. The length of this visible portion could be adjusted continuously between 0 and 12 in. To increase the visibility of the Size-Variable, a second white screen was mounted behind it. The front screen was so adjusted that the lower limit of the visible portion of the strip lay in O's visual horizontal plane. The distance of the Size-Variable from O was 60 in. ($d_{va(s)} = 60$ in.).

Observers. The Os were 11 undergraduate students, 7 women and 4 men. The age-range was 17-40 yr. and the modal age was 18. All were naïve as to the aim of the experiment.

Procedure. Monocular viewing conditions were used throughout the experiment. For each O, the experiment consisted of 10 trials. For each trial the procedure was as follows:

(a) Determination of $s_{va(s)}$. A two-category method of limits was employed. The Size-Variable was set markedly larger (or smaller) than the Standard (matchbox cover). O was asked to judge "which of the two would be the larger if they were placed face to face." The Size-Variable was then changed appropriately in steps of 0.2 in. until a change in judgment occurred. This was repeated once each for an ascending and a descending series. The value of $s_{va(s)}$ for the trial was taken as the mean of the limits for these two series.

(b) Determination of $d_{va(d)}$. A two-category method of limits was employed. At each judgment, O was asked to state whether the Distance-Variable (card) or the Standard (matchbox cover), was the more distant. Adjustments were made in 1 in. steps. One ascending and one descending series was given and the value of $d_{va(d)}$ for the trial was taken as the mean of the limits for the two series.

(c) Determination of $\bar{s}_{va(s)}$. The Distance-Variable was set at the distance $d_{va(d)}$ determined in Part (b) of the trial. The procedure was then identical to that of Part (a) except that O was asked to compare the sizes of the Size- and Distance-Variables. One ascending and one descending series was given and the value of $\bar{s}_{va(s)}$ for the trial was taken as the mean of the limits for the two series.

Over the 10 repetitions of this procedure, the order of Parts (b) and (c) was, of necessity, always the same. Part (a), however, was randomly placed either first, second, or third in the procedure.

Result. For each O, values of $m_{s_{va(s)}}$, $m_{s_{va(s)}}$, $m_{d_{va(d)}}$ and *covar* $(s_{va(s)} d_{va(d)})$ were calculated. In each case a value of D was calculated from Equation [16]. The mean and standard deviation of each of these variables is shown in Table I.

If the size-distance-invariance hypothesis held for any *O*, the expected value of *D* would be, for him, zero, Equation [15]. If the hypothesis held for all *O*s, the expected value of *D*, replicating over *O*s would also be zero; for 10 out of the 11 *O*s tested, *D* was less than zero. On a two tailed binomial test, this proportion differs significantly from 0.50 at the 0.05 level of significance ($p = 0.012$). This result implies that for at least some *O*s, Equation [15] does not hold. It follows that for these *O*s, Equation [2], the linear form of the size-distance-invariance hypothesis, does not hold over the four combinations of physical size and distance employed in this experiment.

While this result further eliminates the possibility that s_o'/d_o' is a power function of s_o/d_o, (see footnote 12) it does not in general eliminate the possibility that the weak form of the hypothesis expressed in Equation [1] does hold.

TABLE I

SUMMARY OF EXPERIMENTAL RESULTS

	$m_{s_{va(s)}}$	$m_{\bar{s}_{va(s)}}$	$m_{d_{va(d)}}$	$covar\ (s_{va(s)} d_{va(d)})$	D
mean	1.91 in.	2.97 in.	125.5 in.	−0.29	−52.92 sq. in.
SD	0.42 in.	0.59 in.	13.0 in.	0.73	43.37 sq. in.

SUMMARY

It has often been suggested that visual space is so structured, that the ratio of perceived size to perceived distance is proportional to the corresponding ratio of physical size to distance. A common method of testing this hypothesis employs size and distance matching techniques. The validity of this procedure as an exact test of the hypothesis, is dependant upon certain assumptions which are not empirically justified. A modification of the usual technique is suggested which enables the hypothesis to be tested without making these assumptions. An experiment employing this procedure, under monocular viewing conditions, failed to confirm the hypothesis.

COMPARISONS OF PREDICTIONS OF SINGLE RANDOM EVENTS WITH JUDGMENTS OF POPULATION BIAS

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*S*s who are required to judge the direction of bias of binary populations by samples from these populations tend to use smaller samples when the bias is favorable to them than when it is unfavorable.[1] Experiment I attemped to determine whether a similar result would be obtained when *S*'s task was to predict a single event in such a series rather than of a population parameter.

EXPERIMENT I

S's task was to observe and record cards exposed one at a time from a pack containing marked and blank cards, to stop the presentations at any point chosen by him, and then to predict whether the next card in the pack (the "critical" card) would be marked or blank. In each pack the occurrence of one of the two kinds of cards as the critical card was made preferable to *S* by its differential money value—"independent outcome-value" (*IO*). The hypothesis was that *S* would employ smaller samples when their bias was in the preferable direction than when it was in the less preferable direction. *S* also won or lost money—"dependent outcome-value" (*DO*)—according to whether or not his prediction was correct and he was charged a small amount for each card that he observed.

Subjects. The *S*s were undergraduates at the University of Pennsylvania, 30 men and 30 women. They were paid a minimum of $1.00 for their services.

Materials. Packs of 50 cards of standard playing-card size were used. Their faces were either blank or printed with a large bold-face X. Of the 12 experimental packs, 2 contained 70% marked cards, 4 had 60%, 4 had 40%, and 2 had 30%. Each pack was used twice, once with positive and once with negative *IO*. An additional pack with 50% was used for practice.

Procedure. The *S*s were seen individually. The procedure, materials, and pay-offs

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[1] H. C. Morlock, Jr., The effect of outcome desirability on information required for decisions, Doctoral dissertation, University of Pennsylvania, 1964.

were described fully and a practice trial was run. Each of the 24 experimental trials proceeded as follows: A pack of cards was displayed and *S* was told the value of the critical card in that pack (*IO*), which was always zero for a blank card and was +10, −10, +25, and −25 cents for a marked card in equal numbers of packs. *E* turned up cards from the pack at a rate of about one in 2 sec. *S* recorded the cards on a slip of paper as they were shown, stopped the series when he wished, and predicted the next card. This card was then turned up and *S*'s payoffs for its value (*IO*), a win or loss of 15¢ for correct or incorrect prediction (*DO*), and a cost of ⅕¢ per card shown, were recorded and made known to *S*. *S* was informed that the packs had been shuffled, that he could not predict perfectly, but that he could do better than chance if he observed the cards carefully. In addition, *E* shuffled the packs in the sight of *S* while reading the instructions. Six *S*s were assigned at random to each of 10 schedules of order of presentation of the packs. These orders were random under the restrictions that neither the same *IO* nor the same card frequency should occur more than twice in succession.

Results and discussion. The mean number of cards drawn when the samples were biased in the direction favorable to *S* was 7.9, $SD = 5.4$, and the mean for unfavorably biased samples was 8.2, $SD = 6.1$. The mean difference of 0.3 was in the predicted direction but was approximately equal to its standard error and therefore did not approach statistical significance. This failure to confirm an hypothesis similar to one for which Morlock had obtained positive results was associated with other differences in the *S*s' behavior in the two experiments. One of these was especially pronounced; namely, that whereas Morlock's *S*s judged the pack bias to be favorable in about half (51.0%) of the trials, our *S*s predicted favorable outcomes more than five times as often as unfavorable outcomes (84.4 vs 15.6%).

The hypothesis that fewer cards would be drawn from packs with a favorable bias than from those with an unfavorable bias supposed that the *S*s would base their predictions upon the relative frequencies of marked and blank cards in the samples, but would arrive at favorable predictions upon less evidence than unfavorable ones because of the well-established effect of *IO* upon predictions of uncertain outcomes.[2] The unexpected prevalence of favorable predictions led us to question this supposition, and it was easily shown to be false. Our *S*s did not in general base their predictions upon the relative frequencies in the samples:

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[2] R. W. Marks, The effect of probability, desirability, and "privilege" on the stated expectations of children, *J. Person.*, 19, 1951, 332-351; D. G. Pruitt and R. D. Hoge, Strength of the relationship between the value of an event and its subjective probability as a function of method of measurement, *J. exp. Psychol.*, 69, 1965, 483-489; F. W. Irwin and J. G. Snodgrass, Effects of independent and dependent outcome values upon bets, *ibid.*, 71, 1966, 282-285; and others cited in these papers.

of their 1440 predictions, only 43.1% agreed with the direction of the sample biases, whereas 40.1% disagreed with the samples, 12.4% were based upon evenly-divided samples, and 4.4% were made without observing any cards at all. This contrasted sharply with Morlock's results, in which the corresponding percentages were 97.7, 1.2, 0.9, and 0.3. It must also be noted that 96.3% of the judgments of the populational bias of Morlock's *S*s' were correct, while only 51.7% of the predictions of single events of our *S*s' were correct. This difference was largely due to the fact that the bias of a sample is a better predictor of the populational bias than of a single event; and, in addition, Morlock's *S*s were aided by the fact that his cards were randomized in blocks of 10, whereas our packs were shuffled as wholes. Nevertheless, if our *S*s had always predicted in accordance with the sample, their percentage correct (by actual count) would have risen to 59.7, and they would have won a mean amount per *S* of 58¢ instead of their actual mean of 10.5¢.

A major difference between the two experimental situations was in the tasks that confronted the *S*s. Morlock's *S*s were required to use information from a sample in order to judge a parameter of a population; namely, its direction of bias, whereas ours had to use such information to predict a single random event. Since our *S*s did not in general rely upon the sample bias, it seemed possible that they tried to solve their problem by using sequential properties of the cards in the samples, in spite of the instructions and the card-shuffling. Evidence that this was the case is given by Curve A of Fig. 1, which shows the percentage of predictions that the critical card would be the same as the last card of the sample plotted against the length of the terminal run of the sample, *i.e.* the run immediately after which *S* stopped and predicted the critical card. (The 64 predictions made without looking at any cards are omitted from Fig. 1.) It can be seen that for terminal run-lengths of 1 and of 6 or more the *S*s tended to predict that the run would continue (a "positive recency-effect"), while intermediate run-lengths of 3, 4, and 5 show a negative recency-effect. The observed relative frequencies differed significantly from 0.5 with $p < 0.05$ for all run-lengths except 2 (run-lengths greater than 5 being grouped together). It may be concluded that our *S*s based their predictions at least in part upon sequential information. As to Morlock's experiment, although direct evidence is not available, his *S*s' judgments agreed with the sample biases so well that little room existed for effects of sequences.

The possibility that the effects shown in Curve A of Fig. 1 might differ according to whether the terminal run was made up of the favorable or the unfavorable type of card was investigated by decomposing this curve into Curve B, in which the former was true, and Curve C, in which the latter was true. Effects of run-length are weaker in Curve C, as indicated by its relative flatness, but both curves show a descent followed at long run-lengths by an ascent. The fact that Curve B lies above Curve C reflects the strong tendency to predict favorable rather than unfavorable outcomes.

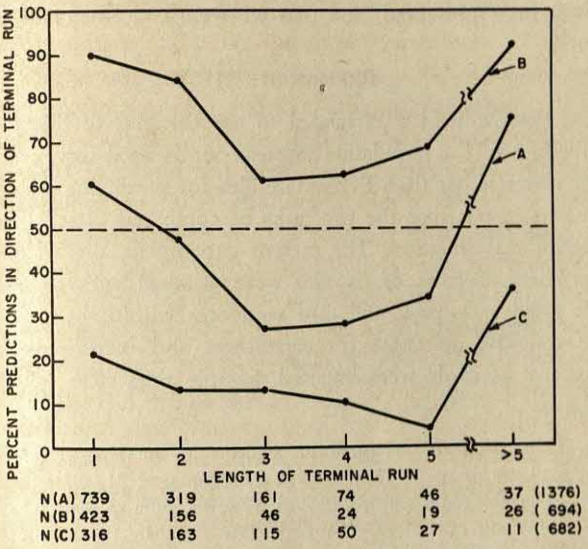

FIG. 1. PERCENTAGE OF PREDICTIONS OF THE CRITICAL CARD IN THE DIRECTION OF THE TERMINAL RUN (EXPERIMENT I)
A, all terminal runs; B, favorable terminal runs; C, unfavorable terminal runs

Studies of recency-effects as a function of run-length in binary-prediction experiments have produced quite diverse results, which throw little light upon our situation.[3] The relation shown in Fig. 1 seems to be unique, although both it and the procedure from which it was derived are perhaps most similar to Lepley's. That its general form is reproducible will be demonstrated by Experiments II and III. In comparisons with other experiments, however, it must be remembered that our *S*s made only one overt prediction in each series, that they saw much smaller samples than have usually been presented, and that we were concerned only with the terminal run.

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[3] M. E. Jarvik, Probability learning and a negative recency effect in the serial anticipation of alternative symbols, *J. exp. Psychol.*, 41, 1951, 291-297; Julian Feldman, On the negative recency hypothesis in the prediction of a series of binary symbols, this JOURNAL, 72, 1959, 597-599; D. C. Nicks, Prediction of sequential two-choice decisions from event runs, *J. exp. Psychol.*, 57, 1959, 105-114; N. H. Anderson and R. E. Whalen, Likelihood judgements and sequential effects in a two-choice probability learning situation, *ibid.*, 60, 1960, 111-120; Ward Edwards, Probability learning in 1000 trials, *ibid.*, 62, 1961, 381-390; Harold Lindman and Ward Edwards, Supplementary report: Unlearning the gambler's fallacy, *ibid.*, 62, 1961, 630; W. M. Lepley, "The maturity of the chances"; A gambler's fallacy, *J. Psychol.*, 56, 1963, 69-72.

EXPERIMENT II

The fact that Morlock's *S*s tended to use the bias of the sample for judging the bias of the population whereas our *S*s used sequential properties of the series rather than the sample bias for predicting a single event in the series suggests that the two tasks be compared directly within the same experimental situation. The present experiment allotted these tasks to the members of pairs of *S*s who were matched with respect to size of sample from each pack and card sequences within the samples. Payoffs were restricted to those for correctness and incorrectness; no *IO* values nor cost of cards were imposed. Sample sizes were determined in advance by *E*.

Subjects. Thirty women, undergraduate students at the University of Pennsylvania, were used in this study. They received a minimum of $1.00 for their services.

Procedure. The procedure and instructions were the same as those of Experiment I, with the following exceptions. (1) Each *S* was shown 12 sequences of cards instead of 24, two each of lengths 5, 7, 9, 11, 13, and 15 cards. (2) Identical sequences were shown to both members of each pair of *S*s, and the cards were shuffled thoroughly between pairs. (3) No written record of the sequences was kept by *S*. (4) One member of each pair was told that she was to predict the next *single card* following the presentation of the sample (Group S). The other member of the pair was told that no pack was evenly divided between marked and blank cards and that, after the sample was presented, she was to judge whether the *whole pack* contained more marked or more blank cards (Group P). Immediate feedback as to correctness was given after each decision in both groups, with pay-offs of ±15¢ as in Experiment I.

Results and discussion. As expected, judgments of the direction of bias of the pack (Group P) agreed with the direction of bias of the sample more frequently than did predictions of single cards (Group S). The mean numbers of such judgments out of a possible 12 were 10.0 and 7.8 in the two groups, respectively. This difference of 2.2 was significant ($t = 3.6, df = 14, p < 0.01$).

Fig. 2 indicates that the *S*s of Group S, like those of Experiment I, tended to show a positive recency-effect for long terminal runs and a negative recency-effect for runs of medium length (3 or 4). In sharp contrast, the *S*s of Group P showed a positive recency-effect for all run-lengths greater than one, and this effect increased with the length of the run as would be expected if they were judging the bias of the pack from that of the sample.

Thus, the two tasks led to quite different strategies. To predict single events, the *S*s mistakenly attempted to find predictive properties within the series, whereas to judge the population-bias they tended to make appropriate use of the sample-bias. That the former strategy was mistaken is shown, aside from *a priori* considerations, by the fact that only 53.3% of the predictions of Group S were correct, whereas 62.8% (by count) would have been correct had the predictions all conformed to the sample-bias. In the case of Group P, 70.0% of the predictions were correct, as compared with the 81.1% (by count) that would have been correct had the *S*s always judged from the sample-bias.

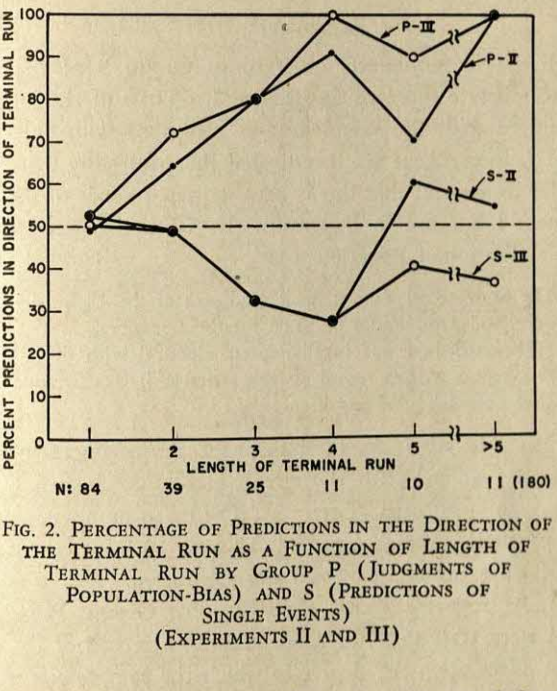

FIG. 2. PERCENTAGE OF PREDICTIONS IN THE DIRECTION OF THE TERMINAL RUN AS A FUNCTION OF LENGTH OF TERMINAL RUN BY GROUP P (JUDGMENTS OF POPULATION-BIAS) AND S (PREDICTIONS OF SINGLE EVENTS)
(EXPERIMENTS II AND III)

These findings are related to those of Galanter and Smith, who interspersed special trials, among the regular trials of an experiment in binary-prediction, in which *S*'s predictions held for the next five trials instead of the next single trial only.[4] On the first of these special trials the relative frequency of predictions of the more frequent event, which had stabilized at a value near 0.75, the probability of the event, rose to 1.00; thereafter, it leveled off at about 0.80. We suggest that the special trials set a task that resembled that of our Group P, and that the *S*s who had been using sequential properties of the series tended to shift to the use of the bias of the sample, thereby achieving a more nearly optimal solution.

EXPERIMENT III

Although the recency-effects observed in Group S of Experiment II were quite similar to those of Experiment I, 65.0% of the predictions of Group S agreed with the sample-bias as compared with only 51.4% in Experiment I. Experiment III investigated the possibility that this difference was due to the fact that the *S*s kept written records of the sequences in Experiment I but not in Experiment II. With this exception, it also provided a replication of Experiment II.

Subjects. The Ss were 30 women, undergraduates at the University of Pennsylvania who were paid a minimum of $1.00 for their services.

Procedure. The procedure was in all respects identical with that of Experiment II except that *S* kept a written record of each series as it was presented. The same packs, series, order of packs, and pay-offs were used.

Results and discussion. As in Experiment II, judgments of the pack-bias (Group P) agreed with the sample-bias more frequently than did predictions of the next card (Group S). The mean numbers of such judgments and predictions out of a possible 12 were 10.1 for Group P and 5.8 for Group S. The difference of 4.3 was significant ($t = 6.5$, $df = 14$, $p < 0.01$). As reported above, the means for Groups P and S in Experiment II were 10.0 and 7.8. Thus, keeping a record of the series had no apparent effect upon the degree to which the sample-bias was used in judging the population-bias, but it evidently weakened the tendency to use this information for predicting single events; the difference between 5.8 and 7.8 was significant, $t = 2.9$, $df = 14$, $p < 0.01$. The percentages of *correct* judgments were 43.3 for Group S and 73.9 for Group P; they would have been 62.8 and 81.1 (as in Experiment II) had the judgments always conformed to the sample bias.

As shown in Fig. 2, Group P showed positive recency-effects that increased with terminal run-length, whereas Group S showed generally negative recency-effects that were most pronounced for run-lengths of 3 and 4. The agreement of the curves with those in Experiment II was close; the largest discrepancies occurred at long run-lengths for which *N* was small, and none of the differences between corresponding points in the two experiments approached significance. In view of this, the difference between Experiments II and III in the use of sample-bias for predicting single events cannot be ascribed to differential recency-effects in the terminal run of the series.

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[4] E. H. Galanter and W. A. S. Smith, Some experiments on a simple thought problem, this JOURNAL, 71, 1958, 359-366. See also C. R. Peterson and Z. J. Ulehla, Sequential patterns and maximizing, *J. exp. Psychol.*, 69, 1965, 1-4.

Taken together, the results of the three experiments indicate that it may be fruitful to compare tasks that require predictions of single probabilistic events with tasks for which population parameters or long-run central tendencies are relevant. Both kinds of problems occur in the natural life of organisms; for the human being, it is not difficult to think of examples ranging from predictions of long and short-range events in the weather or the stock market to predictions of individual acts and average or trait-like properties of individuals and groups. Clearly, there are instances in which local sequential dependencies, if they can be found, are highly useful for decisions, while for other purposes such dependencies are nearly or altogether useless.

SUMMARY

After observing samples from shuffled binary packs, *S*s predicted the next card to be drawn or judged the direction of the bias of the packs. Predictions of single events were affected by the length and direction of the terminal runs of the samples (positive and negative recency-effects) and were not responsive to the direction of bias of the samples. Judgments of pack-bias tended to agree with the sample-bias and showed positive recency-effects that increased with the length of the terminal runs of the samples.

AFFECTIVITY IN THE AROUSAL OF ATTITUDES AS MEASURED BY GALVANIC SKIN RESPONSE

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Galvanic skin response (*GSR*) refers to the brief decrease in electrical resistance of the skin following stimulation. Although the exact physiological basis for this response is not known, it is generally thought to be related to the activity of the sweat glands. The usefulness of the *GSR* as an indicator of emotional response has been examined in many studies over the past 50 yr. It seems clear that *GSR* is evoked by, among other things, stimuli judged to have affective significance.

McCurdy, after reviewing a number of experiments, stated that *GSR* has a demonstrable relationship to the intensity of reported experience, whether 'emotional,' 'affective,' 'conative,' or otherwise.[1] Woodworth and Schlosberg concluded that the *GSR* is a good measure of emotion, if by emotion is meant a highly activated, tense, or energized condition and not a specific mental or behavioral state.[2]

That elevated *GSR*s occur in response to 'emotionally toned' words or expressions has been demonstrated repeatedly. McGinnies, for example, obtained higher *GSR*s from *S*s following tachistoscopic presentation of taboo words than following neutral words.[3] More recently, Cohen, Bondurant, and Silverman not only found that skin resistance showed a greater reduction following "charged" than "bland" expressions, but also discovered that this reaction was closely related to the personal meaning that the stimulus had for each *S*.[4]

In addition, a few studies relating the *GSR* to attitudes have been performed. Rankin and Campbell reported that when simulated adjustments were made to the *GSR* apparatus attached to the *S's* hand, higher *GSR*s were elicited from white *S*s in response to a Negro *E* than to a white *E*.[5] Cooper and his associates on the basis of three studies concluded that complimentary statements concerning groups toward which *S*s held negative attiudes elicited higher *GSR*s than did the same statements

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* Received for publication October 4, 1965. Based upon a master's dissertation submitted by the first author to the University of Maryland.

[1] H. G. McCurdy, Consciousness and the galvanometer, *Psychol. Rev.*, 57, 1950, 322-327.

[2] R. S. Woodworth and Harold Schlosberg, *Experimental Psychology*, 2nd ed., 1954, 158-159.

[3] Elliott McGinnies, Emotionality and perceptual defense, *Psychol. Rev.*, 56, 1949, 244-251.

[4] S. I. Cohen, Stuart Bondurant, and A. J. Silverman, Psychophysiological influences on peripheral venous tone, *Psychosom. Med.*, 22, 1960, 106-107.

[5] R. E. Rankin and D. T. Campbell, Galvanic skin response to Negro and white experimenters, *J. abnorm. soc. Psychol.*, 51, 1955, 30-33.

about neutral groups.[6] McGinnies and Aiba presented Japanese students with two opposed persuasive communications on a political issue. Their *Ss* responded with higher *GSRs* when listening to a communication with which they disagreed than when listening to one with which they agreed.[7]

Although the studies cited indicate that heightened *GSRs* accompany certain attitudes, clear-cut evidence is lacking concerning the relationship between positive, or favorable, attitudes and emotionality. Cooper and Singer's investigation of this did not yield conclusive results.[8]

The present experiment examined the relationship between positive, negative, and neutral attitudes and autonomic arousal. It was assumed that measurable autonomic reactions would occur when *Ss* having either extremes of attitude toward the church listened to statements expressing conflicting opinions. Specifically, we predicted that higher *GSRs* would be elicited from *Ss* favorable toward the church by derogatory statements and from *Ss* antagonistic toward the church by laudatory statements. The same *Ss* were expected to react less strongly to statements supportive of their attitudes. Neutral *Ss* were not expected to respond significantly to either type of statement.

Method: Subjects. A 23-item Likert-type scale based on Thurstone and Chave's statements measuring attitudes toward the church was used in selecting the *Ss*.[9] Five response-alternatives ranging from "strongly agree" to "strongly disagree" were provided for each statement on the scale. The items were so scored that they ranged from 23 (most favorable toward the church) to 115 (most unfavorable toward the church).

The scale was administered to 486 students in introductory psychology who yielded a distribution ranging from 27 to 106. From these students, 60 *Ss* were chosen, 20 from each of three positions on the attitude-continuum. These three groups were defined as pro-church (mean attitiude score 36.1), neutral-church (mean attitude score 68.3), and anti-church (mean attitude score 94.0).

Apparatus and materials. The apparatus employed was a Lafayette Model 601 A Psychogalvanometer, utilizing the circuit of a modified Wheatstone bridge. Twelve statements adapted from Thurstone and Chave's list of opinions about the church were used as stimuli.[10] Four of these statements were laudatory of the church (pro-church statements), four were critical of the church (anti-church statements), and four were neutral towards the church. Two additional statements, one pro-church and one anti-church, were selected for use in adjusting the apparatus and allowing *S*'s *GSR* to stabilize.

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[6] J. B. Cooper and D. N. Singer, The role of emotion in prejudice, *J. soc. Psychol.*, 44, 1956, 241-247; J. B. Cooper and H. E. Siegel, The galvanic skin response as a measure of emotion in prejudice, *J. Psychol.*, 42, 1956, 149-155; J. B. Cooper and David Pollock, The identification of prejudicial attitudes by the galvanic skin response, *J. soc. Psychol.*, 50, 1959, 241-245.

[7] Elliot McGinnies and Hitoshi Aiba, Persuasion and emotional response: A cross cultural study, *Psychol. Rep.*, 16, 1965, 503-510.

[8] J. B. Cooper and D. N. Singer, The role of emotion in prejudice, *J. soc. Psychol.*, 44, 1956, 241-247.

[9] L. L. Thurstone and E. J. Chave, *The Measurement of Attitude*, 1929, 22-66.

[10] Thurstone and Chave, *op. cit.*, 1929, 22-66.

The 12 statements were listed in two different, counterbalanced orders with the two "warm-up" statements presented first in both lists. The two lists of statements were tape-recorded, with 30-sec. intervals separating adjacent items. Ten *Ss* in each of the three groups were presented with one order of statements, and the remaining 10 were exposed to the second order.

Procedure. The *Ss* were tested individually by the first *E*. Upon entering the experimental room, *S* was seated and the basic principles of the apparatus were explained.

Electrodes were then fastened to the palm of the *S*'s right hand, and his right arm was strapped to the arm of the desk in order to minimize movement. *S* was also told that his name had been drawn at random from Psychology 1 students who early in the semester had completed a questionary concerning the church. He was then informed that the purpose of the experiment was to investigate galvanic skin responses to different statements concerning the church.

The following instructions were then given:

> A number of statements concerning the church will be read to you from the tape recorder. As you hear each statement, try to think, as best you can, how you feel about the statement. You might try to imagine how you would respond if you were with a group of friends and someone made this statement to you. Do not, however, say anything during the experiment; just concentrate on the statements and how you feel about them.
>
> Before we begin the actual experimental statements, I am going to ask you some questions I wish you to answer in order to adjust the equipment.

S was then blindfolded and told to sit as still as possible during the experiment, as any physical movement might interfere with the results.

E then asked *S* three questions concerning his name, age, and major subject which were answered aloud. Neither the answers nor the galvanic skin responses were recorded, since this procedure was followed merely to enable *E* to balance *S* into the circuit. During the experimental trials, the largest needle deflection on the psychogalvanometer in a 30-sec. period following the end of each statement was recorded. If it did not make a complete recovery following a deflection, the needle was reset at zero.

Results. Amplitude of *GSR* by the *Ss* to each of the experimental statements was recorded in microamperes, and the mean response for each category of statements was determined for each *S* who was, therefore, represented by three scores: mean *GSR* to anti-church statements; mean *GSR* to neutral statements, and mean *GSR* to pro-church statements.

The data were treated according to a Lindquist Type I Analysis of Variance, in which each of several *A* treatments in combination with any one *B* treatment is administered to the same *Ss*, with each *B* treatment administered to a different group of *Ss*. In this case, the *A* factor was the attitude of the related statements and the *B* factor was the attitudinal score of the *Ss*.

The appropriate analysis of variance was computed. The *B*-effect (between the groups attitude toward the church) was found to be non-significant ($F = 0.7543$ with 2 and 57 *df*). That is, considered independently of the categories of stimuli, there were no differences in *GSR*s between the three groups of *S*s.

The *A*-effect (between statements representing different attitudes toward the church) attained statistical significance at less than the 0.05 level ($F = 3.1823$ with 2 and 114 *df*). Independent of the attitude scores of the *S*s, the statements elicited different levels of *GSR*. The mean *GSR* to each category of statements was then averaged over all *S*s. The smallest mean *GSR* occurred in response to the neutral statements (31.9), a larger mean *GSR* to the anti-church statements (36.0), and the largest mean *GSR* to the pro-church statements (40.2). This effect can also be observed in Fig. 1. The pro-church and neutral *S*s showed the highest mean *GSR*s to the anti-church statements, while the anti-church *S*s responded most strongly to the pro-church statements. The fact that the pro-church statements elicited the highest over-all level of *GSR* can be attributed to reactions by the anti-church *S*s.

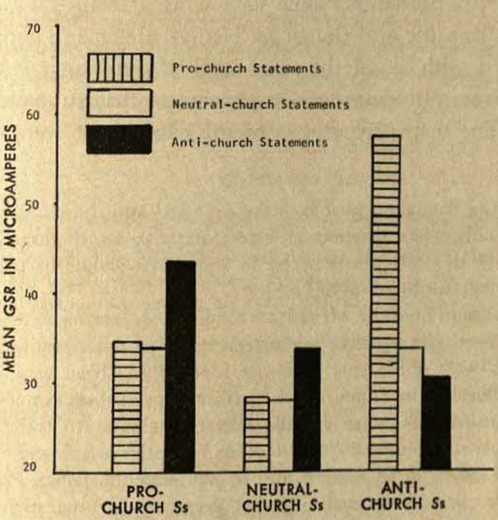

FIG. 1. MEAN *GSR* FOR EACH GROUP FOR EACH STATEMENT CATEGORY

The dependence of *GSR* upon initial attitude of the *S*s toward the church was evaluated by examining the interaction of group x treatment. This interaction-effect was significant at less than the 0.001 level ($F = 6.3922$ with 4 and 114 *df*) and is shown in Fig. 1. The pro-church *S*s responded most strongly to the anti-church statements and showed lower mean *GSR*s to the pro-church and neutral items. The pattern of the neutral *Ss* was similar, with the highest mean *GSR* occurring in response to anti-church statements and almost identical mean *GSR*s to pro-church and neutral statements. The anti-church *S*s showed an opposite pattern, responding most actively to the pro-church statements and less to the neutral statements and the anti-church statements.

To summarize, statements critical of the church elicited a higher level of *GSR* from pro-church *S*s, while statements praising the church elicited higher *GSR*s from the anti-church *S*s. Neutral statements about the church were associated with about the same level of autonomic reaction among the two extreme attitude-groups as statements that supported their predispositions. The major hypotheses of the experiment were thus substantiated.

Discussion. The data show that both the pro- and anti-church *S*s responded with elevated *GSR*s only when listening to statements with which they presumably disagreed. Why did they not also respond to statements about the church that were consistent with their attitudes on this subject?

The verbal stimuli may be viewed as conditioned reinforcers. Confirmation of one's attitude constitutes positive reinforcement, whereas derogation of one's point of view is negatively reinforcing. Evidence for this has been recently provided by Golightly and Byrne, who found that subjects improved their performance in a discrimination learning task when attitude-statements similar to their opinions were used as positive reinforcement.[11] In addition, negative reinforcement, or punishment, has been shown to be accompanied by autonomic response. For example, not only does shock elicit heightened *GSR*, but previously neutral stimuli paired with shock will also elicit an autonomic reaction.[12]

Similar data on autonomic responses to positive reinforcement are not available. Since verbal materials have been shown to have reinforcing qualities, it follows, however, that different verbal stimuli should have the capacity to elicit autonomic responses appropriate to the reinforcement history of the individual.[13] In the present experiment, statements that challenged the *S*s' attitudes elicited autonomic behavior in a manner consistent with what we know about autonomic responses to negative reinforcement in general. Statements about the church that confirmed an existing attitude failed to elicit a corresponding autonomic response. It seems, as a general principle, that less affect is elicited by positively reinforcing than by negatively reinforcing verbal material.

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[11] Carol Golightly and Donn Byrne, Attitude statements as positive and negative reinforcements, *Science*, 146, 1964, 789-799.

[12] A. W. Staats, C. K. Staats, and H. L. Crawford, First order conditioning of meaning and the parallel conditioning of a *GSR*, Tech. Rep. No. 6, Contract No. Nonr-2305 (00) between Office of Naval Research and Arizona State University, 1958.

[13] Leonard Krasner, Studies of the conditioning of verbal behavior, *Psychol. Bull.*, 55, 1958, 148-171; J. H. Williams, Conditioning of verbalization: A review, *Psychol. Bull.*, 1964, 62, 383-393.

Consistent with this interpretation are the data for the neutral *S*s. It can be assumed that the attitudes of these *S*s were not reinforced by either type of statement. In fact, the neutral *S*s showed less differential response to the different categories of stimuli than did either the pro- or anti-church *S*s.

The patterns of *GSR* in the two extreme experimental groups also suggest that contradiction of what is essentially a negative, or hostile, attitude may arouse more affect than contradiction of a positive, or favorable attitude. That is, the anti-church *S*s reacted more strongly to pro-church statements than the pro-church *S*s did to statements criticizing the church. This is consistent with the findings of Cooper and Singer.[14] By analogy, one might contrast the behavior of a militant segregationist when confronted with an argument for desegregation with that of an integrationist when listening to an argument for segregation. Both may be emotionally aroused, but the segregationist is apt to react more dramatically when challenged. Perhaps negative attitudes enjoy stronger affective support than positive attitudes due to essential differences in the acquisitional process.

SUMMARY

This experiment investigated the assumption that attitudes are emotionally supported. On the basis of scores on an attitude scale, 20 pro-church, 20 anti-church, and 20 neutral *S*s were chosen. They were presented with 12 tape-recorded statements, 4 favorable, 4 neutral, and 4 critical with respect to the church, and their *GSR*s to each statement were recorded. It was found that both the pro- and anti-church *S*s demonstrated proportionately higher *GSR*s to those statements which contradicated their measured attitudes. The results support the hypothesis that both extremes on an attitudinal continuum have an affective component, and that autonomic response is elicited more strongly when the attitude is challenged than when it is reinforced.

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[14] J. B. Cooper and D. N. Singer, The role of emotion in prejudice, *J. soc. Psychol.*, 1956, 44, 241-247.

A COMPARISON OF HAPTIC AND VISUAL JUDGMENTS OF SOME ILLUSIONS

By RAY OVER, University of Otago

Rudel and Teuber have shown that haptic (tactile-kinesthetic) illusion of the same magnitude and direction as a visual illusion is obtained when blindfolded *Ss* are required to judge size by tracing the raised contours of the Muller-Lyer figure.[1] They found that the illusion decreased over trials for both visual and haptic judgments and that the decrement effect transferred from modality to modality. These data have important implications for theories of illusions. They pose problems for those theories which offer explanations in terms of processes specific to vision. The perspective theory, for example, argues that the presence of surrounding contours modifies judgments of apparent size-relationships by changing apparent distance-relationships.[2] This type of analysis cannot, however, be applied to the data on haptic illusions provided by Rudel and Teuber It is possible that different kinds of processes underlie haptic and visual illusions. If, however, it is shown that visual and haptic judgments of illusory figures are affected by similar sets of variables it is most probable that attempts to explain illusions in terms of processes which are modality-specific are invalid.

The aim of the present study is to provide further data on the relationship between haptic and visual illusions. It is asked whether stimulus-features which are known to affect visual judgments similarly affect haptic judgments. Experiment I examines the effect of angle of the arrowheads on haptic and visual judgments of the Müller-Lyer figure. For visual judgments, the magnitude of the illusion has been found to be proportional to the cosine of angle.[3] Experiment II compares haptic and visual judgments of the bisection (inverted T) and horizontal-ver-

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[1] R. G. Rudel and H.-L. Teuber, Decrement of visual and haptic Müller-Lyer illusion on repeated trials: A study of crossmodal transfer, *Quart. J. exp. Psychol.*, 15, 1963, 125-131.

[2] Reinhard Tausch, Optische Tauschungen als artifizelle Effecte der Gestaltungsprozesse von Grossen- und formenkonstanz in der natürlichen Raumwahrnehmung, *Psychol. Forschung.*, 24, 1954, 299-348; R. L. Gregory, Distortions of visual space as inappropriate constancy scaling, *Nature*, 199, 1963, 678-680.

[3] Gerardus Heymans, Quantitative Untersuchungen uber das "optische Paradoxon," *Z. Psychol.*, 9, 1896, 221-255.

tical illusory figures. For visual judgments, a significantly greater illusion has been found for the bisection figure.[4]

EXPERIMENT I

Apparatus. Figures were constructed by cementing lengths of chemically polished casein plastic of 0.15-in. diameter to 12-in. squares of hardboard. The same figures were used to obtain haptic and visual judgments. There were four test-stimuli (*TS*s), each of which consisted of a 3-in. length with arrowheads at either end. For two *TS*s, the arrows pointed inward (one at 30° and one at 60°) and for the other two, outward (one at 120° and one at 150°). Each of the two lengths which made up an arrowhead was 1 in. There was a set of nine comparison-stimuli (*CS*s), each of which was a contour without arrowheads. The set ranged from 2.20 in. to 3.80 in. in 0.2-in. steps.

Subjects. There were two groups, each of 36 *S*s. The *S*s in one group made haptic judgments and in the other visual judgments.

Procedure. S was initially presented with one of *TS*s and with *CS* of 3-in. and required to judge which was longer. The figures were placed side by side on a table and were within comfortable reach of *S*. For half of the *S*s in each group, *TS* was on the right, and for half on the left. The *S*s making visual judgments were not required to fixate; those making haptic judgments were blindfolded. They were required to move the index finger of the right hand along the contours and were allowed to go from *TS* to *CS* as often as they wished. Whenever they touched *TS*, however, they were required to trace along the arrowheads as well as the contours to be judged. *S* was instructed to judge which figure was longer; he was not allowed to use an 'equal' category. No information was given to *S* as to the accuracy of judgments.

The up-and-down method was used to obtain judgments.[5] If *S* judged *TS* to be longer than *CS* of 3-in. he was presented with successively longer *CS*s until he reported *CS* to be longer; if *CS* was initially judged to be longer, *S* was presented with successively shorter *CS*s until *TS* was judged to be longer. Each *S* made judgments with each of the four *TS*s; sequences were counterbalanced within each group.

Results. The mean length of *CS* which was judged equal to each *TS* is shown in Table I for both haptic and visual conditions. Each mean is significantly different ($p < 0.05$) from 3 in.; that is, both haptic and visual illusions have been demonstrated for each *TS*. The effect of the orientation of the arrowheads (inward vs. outward pointing) on the direction of the illusion can be examined by subtracting, for each group, the mean equality-judgment found for a given 'in' figure from the mean equality-judgment found for the 'out' figure of corresponding angle. The mean difference of $+ 0.61$ in. ($SD = 0.18$) is significantly different from zero ($t = 5.81$, $p < 0.02$). Thus *TS*s with arrowheads pointing outward were judged to be longer than 3 in. and with arrowheads pointing inward to be shorter than 3 in. The influence of the angle of the arrowheads on the magnitude of the illusion can be examined. The mean equality-judgment found for *TS*s with 'small' angles (30°, 120°) is + 0.18 in. greater than that found for *TS*s with large angles (60°, 150°), the standard deviation being 0.51. This mean difference is significantly different from zero ($t = 6.11$, $p < 0.01$). The important comparison is of the magnitude of illusion for haptic and visual judgments of the same *TS*. There is a mean difference of +0.05 in. ($SD = 0.04$) between haptic and visual judgments of the same *TS*. This difference is not significantly different from zero ($t = 2.04$, $p > 0.05$).

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[4] F. W. Finger and D. K. Spelt, The illustration of the horizontal-vertical illusion, *J. exp. Psychol.*, 37, 1947, 243-250

[5] W. J. Dixon and A. M. Mood, A method for obtaining and analysing sensitivity data, *Amer. Stat. Assoc. J*, 43, 1948, 109-126

TABLE I

MEAN LENGTHS JUDGED EQUAL TO THE ILLUSORY FIGURES (3 IN.) UNDER HAPTIC AND VISUAL CONDITIONS

Angle of Arrowheads		Haptic	Visual
30	Mean	2.59	2.47
	SD	0.42	0.17
60	Mean	2.75	2.73
	SD	0.79	0.24
120	Mean	3.20	3.14
	SD	0.53	0.25
150	Mean	3.32	3.31
	SD	0.29	0.17

The results show that the haptic illusion, as well as the visual illusion, found with the Müller-Lyer figure is influenced by the angle of the arrowheads. Haptic and visual illusions of equivalent direction and magnitude were found for each of the four *TS*s. The data thus are in agreement with, and extend to a further variable, the finding by Rudel and Teuber that there is a basic similarity between haptic and visual judgments of the Müller-Lyer figure.

EXPERIMENT II

This experiment examines the relationships between haptic and visual judgments of the bisection (inverted T) and the vertical-horizontal illusion figures.

Apparatus. For each illusion, a set of 10 figures was constructed by cementing lengths of casein plastic to hardboard. For each figure, the vertical line was 3 in. in length; within the set, the horizontal line varied in lengths from 2.2 to 3.8 in.

in 0.2-in. steps. Figures were presented singly on a stand 18 in. in front of *S*, the horizontal contour being at eye-level.

Subjects. There were four groups, each of 16 *Ss*. Every *S* made two sets of either haptic or visual judgments of one of the illusion-figures.

Procedure. The up-an-down method was again used to obtain judgments. *S* was initially presented with a figure in which both contours were 3 in. and was instructed to judge which was longer. As in Experiment I, figures were presented in serial order until *S* reversed his judgment. The *Ss* making haptic judgments were blindfolded; those making visual judgments were not required to fixate.

Results. Table II shows. for haptic and visual judgments of the two illusory figures, the mean length of the horizontal line that was judged to be equal to a vertical line of 3 in. Each mean is significantly greater than 3 in. ($p < 0.01$); that is, for both haptic and visual judgments the horizontal line must be greater than 3 in. to be judged as the same size as the vertical line. There is greater overestimation of the vertical line with the bisection figure than the horizontal-vertical figure for both haptic ($t = 2.37$, $p < 0.05$) and visual ($t = 4.30$, $p < 0.01$) judgments. The difference between mean haptic and mean visual judgments is not significantly different for either the bisected figure ($t = 0.68$, $p > 0.05$) or the horizontal-vertical figure ($t = 0.57$, $p > 0.05$). The results thus show that haptic illusions of the same direction and magnitude as visual illusions are found for the bisected and horizontal-vertical figures.

TABLE II

MEAN HORIZONTAL LENGTHS JUDGED EQUAL TO VERTICAL LENGTH OF 3 IN.

Judgments		Bisection-figure	Horizontal-vertical figure
Haptic	Mean	3.59	3.27
	SD	0.58	0.50
Visual	Mean	3.67	3.33
	SD	0.31	0.32

DISCUSSION

Haptic and visual illusions of equivalent direction and magnitude have been found for the illusion-figures studied. The present data, together with the results obtained by Rudel and Teuber, suggest that an adequate account of spatial illusions cannot be given in terms of processes which are specific to vision.[6] The status of some theories of illusions can be considered in these terms.[7]

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[6] Rudel and Teuber, *op. cit.*, 128-129.

[7] H. A. Carr, *An Introduction to Space Perception*, 1935, 368-405.

The theory of retinal sensitivity argues that, because perceived size varies with the retinal position of the stimulus, differences in perceived size are due to the differential sensitivity of the retina. The horizontal-vertical illusion, for example, is accounted for in terms of a relative macropsia in vertical coding of the visual field. Data involving haptic inspection suggest that this is not an adequate explanation. Whereas the visual inspection of the illusory figure leads to stimulation being distributed across the receptor surface at a given period of time, haptical inspection of the figure involves differential stimulation of one part of the receptor surface over a period of time. The two conditions produce illusions of the same magnitude.

The theory of illusions based on eye-movement has been discredited by the observation that illusions are found with tachistoscopic exposure[8] and with stabilization of the retinal image.[9] The fact that tactile illusions are found, in which the figure in relief is pressed onto the skin, suggests that movement is not the determining component of haptical stimulation.[10]

The perspective theory treats illusions as misapplied constancies. It argues that visual inferences are learned in dealing with an objective environment and that these habits of interpretation are wrongly applied to illusion-figures. The perspective theory particularly argues that two-dimensional illusory figures are perceived as representations of three-dimensional space; for example, the vertical line is seen as longer because it is seen as extending away from the horizontal. The claim is that the presence of the inducing figure changes the apparent size of the length to be judged by modifying its apparent distance. This theory has to face the problem that haptic and visual illusions are equivalent in direction and magnitude. It seems meaningless to speak of apparent distance in connection with the haptical inspection of an illusory figure.

The perspective theory is one form of a relational stimulatory theory. In its general terms, this theory states that judgments of the size of an object are relative to the spatial properties of the surrounds. It makes no statements involving processes which are specific to one modality. The major weakness of various relational or context theories is that they have used such terms as contrast and confluence as though they were explanatory principles. Both terms do nothing more than state that judgments are relational. There is a need to determine for each class of illusion the stimulus-features compared and the processes involved in the comparison.

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[8] E. G. Boring, *Sensation and Perception in the History of Experimental Psychology*, 1942, 244.

[9] R. M. Pritchard, Visual illusions viewed as stabilised retinal images, *Quart. J. exp. Psychol.*, 10, 1958, 77-81.

[10] F. E. Craig, Variations in the illusion of filled and unfilled tactual space, this JOURNAL, 43, 1931, 112-114.

It is, of course, possible that the processes underlying haptic and visual illusions are similar in principle but different in operation; for example, that both haptic and visual judgments are based on relational stimulation but that the visual interpretation of relationships is in terms of *S*'s past experience with three-dimensional spatial arrangements. In these terms, however, it is unlikely that practice would modify haptic and visual illusions at the same rate or that there would be crossmodal transfer of the decrement effect.[11] A more probable hypothesis is that similar kinds of relational responding underlie haptic and visual illusions. The problem becomes one of determining the processes involved.

SUMMARY

In the present experiments, it has been asked whether variables which have been shown to influence the magnitude of visual illusions similarly influence haptic (tactile-kinesthetic) illusions. Both visual and haptic judgments made of the Müller-Lyer figure were found to be a function of the angle of the arrowheads. For both modes of judgment, a larger illusion was found for the bisection (inverted *T*) than the horizontal-vertical figure. The results suggest that theories which attempt to explain illusions in terms of processes which are specific to vision are invalid.

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[11] Rudel and Teuber, *op. cit.*, 128-129.

CHOICE REACTION-TIME AND SIZE OF STIMULUS-SET WHEN TRANSMITTED INFORMATION IS HELD CONSTANT

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In a previous study, it was shown that if response-information was held constant in a disjunctive reaction, varying the stimulus-information by introducing different *relative frequencies* of stimuli affected the reaction-time.[1] The question was then suggested, whether this effect of stimulus-information could be generalized to cases in which it was varied by increasing the *number of alternative* stimuli.

In general, the degree of similarity among stimuli increases with their number. When this is the case, our hypothesis states nothing more than that similarity and, hence, 'confusability' increases with the number of alternative stimuli, which is the finding of studies by Crossman and by Rabbitt.[2] We suggest that the approach of information-theory to our problem will prove to be fruitful only when stimulus-information not confounded with an independently defined variable of discriminability can be shown to affect response-times. Previous studies have failed to keep these variables unconfounded.[3]

The problem of isolating stimulus-information can be solved in the following way. *S* is instructed to discriminate between two sets of stimuli: the size of one set is known to him and the other is—from his point of view—indefinitely large. Two sets of stimuli are used in the experiment. *S* is first thoroughly familiarized with the stimuli of one of the sets. Then he is presented with a stimulus which belongs either to this *familiar* set or to the other *unfamiliar* set, and is instructed to indicate whether the stimulus is one of those of the familiar set or not. Since *S* has no information about the size of the set of unfamiliar stimuli, this set is assumed

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* Received for publication January 14, 1966. The writers thank Dr. D. Kahneman for suggestions and criticism, and Dr. C. W. Greenbaum for comments on the drafts of this paper.

[1] I. M. Schlesinger and Rachel Melkman, The effect on choice reaction-time of stimulus information varied independently of transmitted information, *J. gen. Psychol.*, 74, 1966, 165-172.

[2] E. R. F. Crossman, The measurement of discriminability, *Quart. J. exp. Psychol.*, 7, 1956, 176-195; P. M. A. Rabbitt, Effects of independent variations in stimulus and response probability, *Nature*, 183, 1959, 1212.

[3] Crossman, *op. cit.*, 176-195; Rabbibt, *op. cit.*, 1212; R. E. Morin, Bert Forrin, and Wayne Archer, Information processing behavior: The role of irrelevant stimulus-information, *J. exp. Psychol.*, 61, 1961, 89-96.

to be subjectively indefinitely large. Hence, it is predicted that responses to unfamiliar stimuli will take longer than those to familiar-stimuli. Discriminability will not be relevant to the results, since it can differ only between *sets* of stimuli; it makes no sense to say that A is more discriminable from B than B from A.

No specific prediction can be made as to the size of the difference in reaction-times, because *S* is not informed of the actual size of the set of unfamiliar stimuli. Moreover, the relationship between reaction-time and amount of information has previously not been found to hold for individual stimuli.[4]

The predicted difference between responses to familiar and unfamiliar stimuli may be accounted for in terms of a 'matching model,' such as the one proposed by Welford.[5] Assume that the incoming stimulus is matched against the set of n familiar stimuli, one after another. If the incoming stimulus is an unfamiliar one, n matchings will be required in order to decide which response is to be made. If, however, the incoming stimulus belongs to the set of familiar stimuli, it will often not be necessary to go through the whole set in order to identify it as being familiar; with equiprobable occurrence of n stimuli the mean number of matchings required will be $(n + 1)/2$. Hence less time will be required to respond to a familiar stimulus than to an unfamiliar one.

This matching model thus describes a possible process whereby the number of stimulus alternatives affects reaction-times. It enables us to make a specific prediction regarding the effect of the size of the set of familiar stimuli. A response to an unfamiliar stimulus will require n matchings, to a familiar one, $(n + 1)/2$ matchings. One may infer that the response to the unfamiliar will take longer, up to twice the time for the familiar. Moreover, the ratio will be greater for a larger n.

It should be noted that prediction concerning the effect of size of set cannot be derived from the main hypothesis of this paper. The effect of stimulus-information on response-time is uncertain because it is not known how large, subjectively, is the ensemble of unfamiliar stimuli. Our hypothesis is therefore weaker than the matching model. In the present study, it was intended to test the more general hypothesis, that is, that stimulus-information varied independently of transmitted information will affect reaction-time, and, in addition, to test the specific matching model described above.

Method: (1) Apparatus. On a vertical display board, nine rows of 6-w. lights were mounted, each row containing nine such bulbs. The square of 9 × 9 lights was 25 cm. square. The apparatus was so constructed that different light-patterns could be presented. Two telegraph keys were built into the circuit in such a way that depressing either key would break the circuit and extinguish the light-pattern. An electric clock was switched on simultaneously with the light patterns and was stopped by depressing either one of the telegraph keys.

Stimulus-patterns. Eight different stimulus-patterns could be presented by the above apparatus. Four of these were nine-light patterns and four others were seven-light patterns. Heenceforward these will be referred to as 9-l and 7-l respectively

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[4] Ray Hyman, Stimulus information as a determinant of reaction-time, *J. exp. Psychol.*, 45, 1953, 188-196.

[5] A. T. Welford, The measurement of sensory-motor performance: survey, and reappraisal of twelve years' progress, *Ergonomics*, 3, 1960, 189-299.

These patterns were constructed for use in a previous experiment and have been described in more detail elsewhere.[6]

Each pattern was presented, in addition, on a key-card (13.8 × 8.6 cm.) by small dark-blue circles on a white background.

(2) Subjects. Twenty students studying at the Hebrew University—5 men and 15 women—served as *S*s. They were between 19 and 30 yr. old.

(3) Procedure. The experimental procedure, in general outline, was as follows. The first phase of the experiment presented four of the stimulus-patterns—two 9-1 and two 7-1—with the purpose of familiarizing *S* thoroughly with them. Next, *S*'s recognition of the patterns was tested: he was told to respond to any one of the familiar patterns by depressing one of the telegraph keys and to an unfamiliar pattern by depressing the other one.

After a certain criterion was met, the experiment proper began. This consisted of 32 presentations, half of which were familiar patterns, *i.e.* those learned by *S*s in the first phase, and the other half unfamiliar patterns. *S* was instructed to discriminate between familiar and unfamiliar patterns by depressing one of the two keys. He was not informed of the number of unfamiliar patterns.

To find out to what extent experience with the set of unfamiliar stimuli in the discrimination-task made these familiar to *S*, he was presented with each of these stimuli after the end of the experiment proper, and asked which ones he had seen before. Of the four stimuli, *S*s recognized mostly only two or three. When asked what was their estimate of the number of unfamiliar stimuli, most *S*s gave numbers larger than four. The familiar set thus remained actually more familiar and appeared smaller in size, even at the end of the experiment.

Before half of the presentations, *E* informed *S* whether a 9-1 or a 7-1 pattern would appear; in these presentations the number of possible familiar patterns was, accordingly, two instead of four. (Although the same was, of course, also true of the number of possible unfamiliarized patterns, *S* could not have known this.)

It has been shown that negative sentences take longer to respond to than positive sentences, both in English and in Hebrew, which is the language in which the instructions were given in this experiment.[7] Hence, it was conceivable that instructions to respond in a certain way to unfamiliar stimuli (in Hebrew one says "not familiar") would result in subvocal responses lengthening the reaction-time to these stimuli compared with the familiar stimuli. To counterbalance this effect, unfamiliar stimuli were called "new" and familiar stimuli, "not new" for half of the *S*s.

Time taken for each response and errors was recorded by *E*.

(a) Training trials. Key-cards representing four different stimulus-patterns—two 7-1 and two 9-1—chosen at random were placed in front of *S* from left to right. *S* was instructed to acquaint himself with these patterns so that he would be able to discriminate them from other stimuli later in the experiment. His attention was drawn to the fact that two patterns consist of 7 circles and the other two of 9 circles. He was then told to compare the patterns on the key-cards with those appearing on the display board. The light-pattern corresponding to the key-card to *S*'s

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[6] I. M. Schlesinger, Discriminability as a function of figure complexity, *J. gen. Psychol.*, 73, 1965, 21-29.

[7] P. C. Wason, Response to affirmative and negative binary statements, *Brit. J. Psychol.*, 52, 1962, 133-142; R. R. Eifermann, Negation: A linguistic variable, *Acta Psychol.*, 17, 1960, 258-273.

left, a 7-l card, was then switched on for 5 sec., and *E* proceeded to present each of the other three patterns, a second 7-l then two 9-ls from left to right in this manner. Next, *S* was shown the four light-patterns again for 3 sec. each and in the same sequence.

(b) Test-trials. The key-cards were turned face downward and *S* was told that he would be shown either a "not familiar" ("new") or a "familiar" ("not new") pattern. *S* was instructed to respond to familiar patterns by depressing the key to his right, and to unfamiliar patterns by depressing the key to his left, using the same finger for both responses and reacting as quickly as possible. For half the *S*s, chosen randomly, the assignment of keys to type of stimulus was reversed. *E* then switched on two familiar (one 7-l and one 9-l) and two unfamiliar (one 7-l and one 9-l) patterns. *S* was informed of any error he made; if he made an incorrect response to a familiar pattern, he was shown the same pattern again on the display board, so that he would recognize it better in the following. Next, he was given four test-trials with the remaining four patterns.

A criterion was set up for the correctness of response in the test trials. This was one incorrect response in the series of four trials. If he made more than one error, he was given another series of training trials in which each familiar pattern was exposed for 3 sec., and could be compared to the patterns on the key-cards. After this, he was given the test-trials again. None of the *S*s failed to meet the criterion in this second series of test-trials.

(c) Measuring response-times to familiar and to unfamiliar stimuli. Instructions for the main phase of the experiment were the same as for the test-trials. In addition, *S* was told that on *some* of the trials he would be told beforehand whether a 7-l or a 9-l pattern would appear. Actually, before 16 of the 32 trials, such announcement was made, and before another 16, none was made. An *A B B A* sequence was used.

Each of the eight stimulus-patterns—four familiar and four unfamiliar—appeared four times: twice preceded by such an announcement, and twice without announcement. The sequence of the patterns was determined for each *S* separately by referring to a table of random numbers.

In this phase of the experiment, *S* was not informed of errors.

RESULTS AND DISCUSSION

Response-times were measured for the two kinds of stimuli—familiar and unfamiliar—under two conditions: (a) *S* was informed beforehand whether a 7-l or 9-l pattern would appear, *i.e.* there were two alternative familiar patterns; (b) no such information was given, *i.e.* there were four alternative familiar patterns. Mean response-times are shown in Table I. As predicted, unfamiliar patterns took longer to respond to, and the difference was found to be significant at the 1% level by a Wilcoxon test ($T = 28$). This finding confirms the main hypothesis of this study concerning the effect of stimulus-information on response-times.

Restricting the number of alternative familiar patterns was effective in reducing response-times to *familiar* patterns from a mean of 2.07 to a mean of 1.93 sec. This difference was significant at the 5% level by the above test ($T = 50.5$). For unfamiliar patterns, however, restricting the number of alternatives reduced response-times by only 0.02 sec. and this difference was not significant ($T = 72$). These results fail to bear out the prediction derived from the matching model, according to which the effect of the number of alternative stimuli would be more pronounced for unfamiliar than for familiar stimuli. In this connection, it is of interest that in a previous experiment in which stimulus-information was varied by frequency-imbalance, results were obtained which also failed to fit predictions made from the matching model.[8]

Welford has proposed a three-stage central mechanism to account for results of previous studies of sensorimotor tasks.[9] The present findings, in conjunction with those of the experiment in which stimulus-information was varied by frequency-imbalance, give additional evidence for the 'perceptual mechanism' which constitutes the first stage of Welford's model. One of us has suggested that it might be fruitful to conceive of each of these three stages as a channel with limited capacity for transmitting information.[10] Some support for this notion has been obtained in the experiment on frequency-imbalance. Unfortunately, the experimental situation dealt with in the present paper does not lend itself to testing such an hypothesis, since, as stated above, no specific predictions can be made regarding the difference between response-times to familiar and to unfamiliar stimuli.

TABLE I

MEAN RESPONSE TIMES IN SECONDS

$N=20$

Stated number of familiar patterns	Stimulus	
	Familiar	Unfamiliar
2	1.93	2.22
4	2.07	2.24
Mean	2.00	2.23

In a study concerned with a different theoretical problem, a single response was called for, whereas the number of stimuli was varied.[11] *Ss* responded by saying "recognize" whenever they saw the stimulus-figure, which was either a square or a circle. When they were informed beforehand which figure would be presented, response-times decreased significantly, though only very slightly. Garner quotes this experiment as being relevant to the problem of the effect of stimulus-uncertainty.[12] There seems indeed to be no question of 'confusability' in the Bevan and Dukes experiment, since no discrimination was called for. It is suggested, however, that their experimental situation is a rather special case, and that no conclusions can be drawn from it concerning the issue dealt with in the present paper. It seems safe to assume, that the instruction to respond by saying "recognize" is taken by *S* to mean that he must first decide whether a circle or a square is shown and only then announce that he has recognized the figure. Hence, when no advance information is given he performs, in effect, a discriminative reaction;[13] whereas when he is informed beforehand which figure will appear, he performs a simple reaction. Obviously, a discriminative reaction can be expected to be similar to a disjunctive reaction in that it requires more time than a simple reaction. Our experiment differs from the Bevan and Dukes experiment in that *S* was not required to recognize *which* stimulus is shown but only to perform a two-choice reaction, and the results obtained seem unequivocal in demonstrating the effect of the number of stimulus-alternatives when transmitted information is constant.

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[8] Schlesinger and Melkman, *op. cit.*, 165-172.

[9] Welford, *op. cit.*, 192.

[10] Schlesinger, Response uncertainty as a determinant of reaction-time, *Acta Psychol.*, 22, 1964, 52-59.

[11] William Bevan and W. P. Dukes, Preparatory set (expectancy)—an experimental consideration of its "central" locus, *Brit. J. Psychol.*, 44, 1956, 52-56.

[12] W. R. Garner, *Uncertainty and Structure as Psychological Concepts*, 1962, 45-46.

SUMMARY

It is generally accepted that choice-reactions are a function of the amount of transmitted information. This paper advances the hypothesis that reaction-times are dependent also on the amount of stimulus-information when the latter is varied independently of transmitted information.

In the present study, stimulus-information was varied by varying the number of alternative stimuli. In doing this it is necessary to keep stimulus-information and similarity unconfounded. This was done by letting the *S*s discriminate between a set of familiar and a (subjectively) indefinitely larger set of unfamiliar stimuli. In such a two-choice task, responses of 20 *S*s to familiar patterns required significantly less time than responses to the unfamiliar patterns. Thus, the hypothesis was corroborated.

A 'matching model' which was advanced to account for the effect of stimulus-information was tested by comparing response-times in two experimental conditions: (a) when there were four alternative familiar patterns; and (b) when the number of these patterns was only two. Contrary to predictions derived from the model, the smaller number of alternatives was effective in reducing response-times only to familiar patterns.

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[13] This is essentially the task required in Julius Merkel, Die zeitlichen Verhältnisse der Willenstätigkeit, *Philos. Studien*, 2, 1885, 73-122.

FLUCTUATION OF AMBIGUOUS FIGURES AND INTELLECTUAL FLEXIBILITY

By ANTE FULGOSI, University of Zagreb, and
J. P. GUILFORD, University of Southern California

The immediate reason for this study was the fact that the second author had hypothsized that Thurstone's[1] factor described as the "rate of alternation of ambiguous figures" is possibility identifiable with the ability defined as "divergent production of figural classes" in the second author's structure of intellect.[2]

Thurstone's factor was determined by substantial to high correlations among his tests of Retinal Rivalry, Windmill Illusion, and Necker Cube. In each of these tests, under steady fixation the observed figure alternates between two different perceptual organizations or visual structures, more rapidly for some individuals than for others. Before Thurstone demonstrated that the rate of fluctuation reflects a constant personal quality and that his three tests could all be used to measure the same trait in individuals, Porter had shown that as many as 15 different ambiguous figures gave similar evaluations of individuals in this respect.[3]

The factor of divergent production of figural classes (*DFC*) has been defined as the ability to shift readily from one class of visually perceived objects to another. Having grouped a collection of objects in one class, *e.g.* as to color, the individual can readily regroup them with respect to some other attribute, *e.g.* as to shape, or other attributes to form other classes. It was suggested that the flexibility found in connection with shifting alternative figures might belong in the same category as shifting from class to class where figures are concerned.

The essential strategy of this study involved administering three selected tests of rates of fluctuation together with three tests designed for

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* Received for publication April 26, 1966. A study supported by Contract Nonr-228(20) with the Office of Naval Research, Personnel and Training Branch.

[1] J. P. Guilford, *Personality*, 1959, 383-384; L. L. Thurstone, A factorial study of perception, *Psychometric Monogr.*, 4, 1944, 109-110.

[2] Guilford, Three faces of intellect, *Amer. Psychologist*, 14, 1959, 469-479. This factor will often be designated by its structure-of-intellect trigram symbol *DFC* in the remainder of this paper.

[3] E. L. H. Porter, Factors in the fluctuation of fifteen ambiguous phenomena, *Psychol. Rec.*, 2, 1938, 231-253.

the intellectual aptitude factor *DFC*. It did not appear necessary in a preliminary investigation to determine whether the rate of fluctuation would also be related to divergent production of classes where the information is either symbolic (composed of literal and numerical items) or semantic (composed of meaningful verbal items), for relationships to the parallel abilities in the symbolic and semantic areas of information are less likely. If the results of this study came out with a positive relationship, studies of the rate of fluctuation in connection with the parallel symbolic and semantic abilities[4] (*DSC* and *DMC*, where "M" stands for semantic) would be more reasonably considered.

A secondary aspect to the problem was to see whether rate of fluctuation would possibly correlate with tests of any other intellectual factors that have to do with flexibility. In addition to the type of flexibility that pertains to the ready shifting from class to class, which has been known as "spontaneous flexibility," there are two other kinds that have been recognized in connection with structure-of-intellect (*SI*) abilities.[4] One type is known as adaptive flexibility, which was first recognized in connection with a factor for which a Match Problems test is a consistent indicator. In each item of a Match Problems test is a set of continuous squares (triangles), the sides of which are composed of match sticks (represented in line drawings). The examinee is told to remove a specified number of matches to leave a specified number of squares (triangles) and to solve each problem in different ways. The kind of flexibility involved was first identified as "adaptive flexibility," because it was necessary to be flexible in order to solve a problem. The factor has more recently been identified as the divergent production of transformations (with the trigram symbol *DFT*), where transformations are any kinds of change. It was recognized that there are also potential *DST* and *DMT* abilities or functions, of which *DMT* has been demonstrated many times. Again, it was thought that it was most likely that if rate of fluctuation is related to any adaptive-flexibility factors at all, it would be with *DFT*, the figural factor. Consequently a form of Match Problems was used in this study.

The third type of flexibility has been recognized as a kind of redefinition, a low degree of which has been associated logically with functional fixedness. Such an intellectual factor was first found in the semantic area.[5] Parallel factors in the figural and symbolic areas were hypothesized on the basis of the *SI*-theory and have since been demonstrated.[6] With the thought that only the figural-redefinition factor might possibly be related to rate of fluctuation, only a test to represent factor *NFT* was included in this study. That test is Hidden Figures, a multiple-choice test based upon the Gottschaldt figures. Gottschaldt's five basic figures are the alternative answers for every item, each of which is one of his more complex figures, within which one of the basic figures is hidden.

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[4] J. W. Frick, J. P. Guilford, P. R. Christensen, and P. R Merrifield, A factor-analytic study of flexibility of thinking, *Educ. psychol. Measmt.*, 19, 1959, 469-496.

[5] R. C. Wilson, J. P. Guilford, P. R. Christensen, and D. J. Lewis, A factor-analytic study of creative-thinking abilities, *Psychometrika*, 19, 1954 p. 306.

Test-material. Five printed tests, administered to small groups, of *Ss*, were used to represent three intellectual abilities having something to do with flexibility of thinking. The tests of Match Problems and Hidden Figures have just been described. Three tests designed for Factor *DFC* were included for several reasons. One reason was that this factor had been less strongly demonstrated previously in a ninth-grade sample but not in an adult sample, to which the same test-battery had been administered. One test designed for *DFC* previously had been revised and a new one had been written. Three tests were selected for use in the study to represent Factor *DFC*, anticipating a small factor analysis of these three tests with the three fluctuation-rate scores.[7]

The three *DFC* tests were:

Multiple Grouping of Figures presents in each problem nine quasi-geometrical line drawings, each figure with several attributes so that it is capable of being classified in more than one way. The examinee is to form as many classes as he can, each with at least three figures.

In each problem of *Multiple Figural Similarities,* a set of three line figures is given, forming a class because they have several attributes in common. The examinee is to select from a set of 10 figures, each of those that could be classified with the set of three by reason of at least one common attribute.

In an item of *Alternate Letter Groups,* a set of eight letters of the alphabet, in capital form, is given. The examinee is to form as many classes as he can, each composed of at least three letters, *e.g.* those having only straight lines or those having closed figures in them.

Apparatus for fluctuations. The fluctuation "tests" had to be administered individually, since a monitored count of each subject's fluctuations in a given time-period has to be recorded. *S* reported each fluctuation the moment it occurred by pressing a lever, which registered on an impulse-counter in another room. The room was practically free from distractions, being designed as "sound-proof." All stimuli for ambiguous figures were seen on a large gray background. In every case, *S* was given printed instructions and was given a short learning session in which he became acquainted with the two views to be seen in each fluctuation-test and with the procedure for reporting changes. A passive attitude was emphasized for all the fluctuation-tests, with *S* told not to hold back changes or to try to bring them about. In each test, two 3-min. obbservation-periods were given, with a 1-min. rest period between. The score was the sum of the two part scores, and from the latter reliability estimates could be readily made, applying the Spearman-Brown formula.

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[6] We are most concerned here with Factor NFT, which was most recently verified by Maureen O'Sullivan, J. P. Guilford, and Richard de Mille, The measurement of social intelligence, *Rep. psychol. Lab.*, University of Southern California, No. 34, p. 23.

[7] We are indebted to Mr. Jack Dunham, who was preparing the same tests for use in a much larger factor analysis of classifying tests, for the use of these tests.

(1) Retinal rivalry. In an ordinary stereoscope, two gray backgrounds were shown, with a square of blue and a corresponding square of yellow, the patches being 2 cm. on each side. Adjustments were made to give each *S* completely overlapping squares. *S* was instructed to press the lever only when a new color had completely taken over.

(2)Necker cube. The outline cube was drawn with each square's sides 9 cm. long, in black ink on a white cardboard. A fixation-point was provided in the center of the total figure.

(3) Rubin vase. The Rubin vase figure was made of black profiles of a face on a white background, the total figure measuring 7.5 × 7.5 cm. Both Necker cube and Rubin vase were presented directly in front of *S* at a distance of about 90 cm.

TABLE I

INTERCORRELATIONS, MEANS, AND STANDARD DEVIATIONS FOR THE FLUCTUATION-RATES AND TEST-SCORES

Variables	1	2	3	4	5	6	7	8	9	Mean	S.D.
(1) Multiple grouping of figures	—	.14	.28	.05	.12	.24	.13	.42	.15	13.7	3.14
(2) Multiple figural similarities	.14	—	.38	.00	.08	.09	.10	.36	.29	14.0	4.98
(3) Alternate letter groups	.28	.38	—	.04	−.16	−.06	−.04	.42	.45	16.9	4.83
(4) Retinal rivalry	.05	.00	.04	—	.36	.42	.55	.11	.06	52.0	9.74
(5) Rubin vase	.12	.08	−.16	.36	—	.82	*	.10	.00	88.7	64.63
(6) Necker cube	.24	.09	−.06	.42	.82	—	*	.13	.14	83.0	51.24
(7) Vase-cube	.13	.10	−.04	.55	*	*	—	.18	.19	105.2	16.26
(8) Hidden figures	.42	.36	.42	.11	.10	.13	.18	—	.45	9.4	2.69
(9) Match problems	.15	.29	.45	.06	.00	.14	.19	.45	—	24.7	6.50

* Correlations in these cells are part-whole and irrelevant.

Between the completion of one test and the first presentation of the next, there was an interval of 1 min.

The printed tests were administered in small groups from 20 to 30 days following the administration of the fluctuation-tests.

Subjects. The *S*s were students in beginning psychology who volunteered for the experiment. As a partial requirement in the course, every student was required to volunteer for service as an experimental *S*. The intention was to test 100 *S*s, and 101 did receive the individual tests, but only 83 could be tested later with the group tests, largely owing to irregular schedules at the end of the semester.

Results. The major part of the relevant data is presented in Table I.[8] Estimates of reliability are not all given, since they were not made for all variables used in this study. The alternate-form reliabilities for the three fluctuation-rate scores were very high, ranging from 0.94 to 0.96. In other studies the reliabilities of the printed tests have been much lower than those values, commonly in the range of 0.60 to 0.70. Since the distributions of the scores for the fluctuation-tests were strongly positively skewed, they were normalized before computation of the intercorrelations.

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[8] For the computations involved we are indebted to the Biomedical Data Center at the Universtiy of California at Los Angeles.

Examination of the intercorrelations shows that the correlations among the three fluctuation-tests were moderate to high, and those among the three tests intended for Factor *DFC* were low but all positive. On the other hand, correlations between the two kinds of tests were essentially zero. This suggests that there were two distinct factors, one for the fluctuation-tests and one for the *DFC*-tests. But because two of the fluctuation-rate tests (Necker cube and Rubin vase) correlated 0.82, which was distinctly higher than either of these tests correlated with retinal rivalry, a factor analysis would very likely show a third doublet factor for those two tests. For the factor analysis, therefore, the scores for Necker cube and Rubin vase were combined.

TABLE II

ROTATED FACTOR MATRIX

Tests	Factors		Communalities
	A	B	
Multiple grouping of figures	.35	.14	.14
Multiple figural similarities	.45	.11	.25
Alternate letter groups	.70	.00	.49
Retinal rivalry	.00	.70	.49
Vase-cube	.05	.74	.52

The factor analysis of the five test variables was carried out by hand calculation and axes were rotated graphically to give the rotated factor matrix shown in Table II. The results show a very complete separation of the two groups of tests, giving a decisive answer to the main question of this study. The conclusion is clear; the fluctuation-rate factor and the factor for divergent production of figural classes are totally different and unrelated psychological variables.

Nor were the correlations of the scores of the fluctuation-rate with Match Problems and Hidden Figures any higher than with the three *DFC* tests, suggesting that the fluctuation-rate factor is identifiable with neither the intellectual ability of *DFT* (divergent production of figural transformations) nor that of *NFT* (convergent production of figural transformations.

The finding with respect to the test Hidden Figures is consistent with Thurstone's result in his analysis of perception.[9] Thurstone had in his battery more than one test of the type of Hidden Figures, which together determined a factor that was distinct from that for his fluctuation-rate tests.

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[9] Thurstone, *op. cit.*, 94-95.

Intercorrelations between the two sets of tests were essentially zero, some of them being on the negative side.

Discussion. Fluctuation-rate scores do not correlate with tests for three of the intellectual-ability factors that pertain to flexibility in dealing with figural information. Will they correlate with any other intellectual abilities? Although this seems highly unlikely, there are a few reports of such correlations.

Craine reported a correlation (epsilon coefficient) of .64 between rate of retinal rivalry (vertical vs. horizontal bars) and score on the *Ohio State Psychological Examination.*[10] Further study showed a correlation of .42 with Raven's Progressive Matrices, with an N of 41.[11] It also revealed a correlation of only .24 with the WAIS Full-scale IQ ($N = 40$) but a significant correlation (coefficient not given) with Performance IQ.[12] One should not expect such correlations to be replicated in larger samples, although it is possible that the retinal-rivalry score involves some basic variable or variables not shared with other fluctuation-rate tests.

Results from Thurstone's report of his analysis of perception tell a quite different story. Thurstone had in his battery of tests some that represented five of his PMA factors—verbal, numerical, word fluency, reasoning, and space. Scores for these five factors correlated close to zero with all three of his fluctuation-rate scores, for his Windmill illusion, Retinal rivalry, and Necker cube tests.[13] It is likely that Thurstone's results are more representative; they were derived from a rather large sample ($N = 170$).

SUMMARY

The hypothesis tested was that Thurstone's factor of rate of spontaneous fluctuation of ambiguous visual phenomena is identifiable with the intellectual-aptitude factor of divergent production of figural classes (*DFC*). Three tests for each factor were administered to a sample of students and the intercorrelations were factor analyzed. The analysis showed the two factors to be clearly orthogonal.

No relation was found between fluctuation-rate and scores for tests representing two other intellectual abilities that pertain to flexibility in thinking. The factor, fluctuation-rate, is probably a purely perceptual phenomena and apparently has nothing to offer toward the understanding of flexibility in thinking.

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[10] Kenneth Craine, Binocular rivalry: Its relation to intelligence and general theory of its nature and physiological correlates, *J. gen. Psychol.*, 64, 1960, 261. N equalled only 40, however.

[11] Craine, *op. cit.*, 262.

[12] *Op. cit.*, 262.

[13] Thurstone, *op. cit.*, 121.

PERCEPTION OF THE VISUAL HORIZONTAL IN NORMAL AND LABYRINTHINE DEFECTIVE OBSERVERS DURING PROLONGED ROTATION

By Brant Clark, San Jose State College, and Ashton Graybiel, U.S. Naval School of Aviation Medicine

It is well known that both normal and labyrinthine defective (*L-D*) *O*s can set a luminous line to the horizontal in darkness with small error when they are seated erect and stationary.[1] It is also well known that when normal *O*s are quickly accelerated to a constant angular velocity at some distance from the center of rotation of a rotating room, two effects occur: (1) There is a *slow* change in the perceived visual horizontal so that it tends to become close to the horizontal which is established by the resultant of gravity and centripetal force. This gradual change in visual orientation has been termed the lag-effect of the oculogravic illusion, and its duration is between 1-2 min.[2] (2) After the lag-effect terminates, there is little or no systematic change in the perceived visual horizontal at least up to 4 hr.[3] The lag-effect has also been reported for *L-D O*s, but it was much less clearly defined.[4] No data are available on the perception of the visual horizontal over a prolonged period of constant rotation in *L-D O*s. Furthermore, the observations of the perception of the visual horizontal by normal *O*s made during the 4-hr. exposure to constant centripetal acceleration were made only every 10 min. It was the purpose of the present study to make frequent observations of the perceived visual horizontal (oculogravic illusion) in normal and *L-D* men over a period of 1 hr. of constant rotation, including both the 2-min. period of the lag-effect and subsequent period of constant rotation, to determine any systematic changes in the lag effect under these conditions.

Method: (1) Apparatus. All of the observations were made in the Pensacola Slow

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* Received for publication August 6, 1965. This study was conducted under the sponsorship of the Offices of Life Sciences Programs, National Aeronautics and Space Administration, Grant R-93.

[1] Ashton Graybiel, The oculogravic illusion, *Arch. Ophthal.*, 48, 1952, 605-615.

[2] Graybiel, *op. cit.*, 605-615; Ashton Graybiel and Brant Clark, The validity of the oculogravic illusion as a specific indicator of otolith function, BuMed Proj. MR005. 13-6001 Subtask 1, Rep. No. 67, Pensacola, Florida; Naval School of Aviation Medicine, 17 Feb. 1962, 1-28.

[3] Clark and Graybiel, Visual perception of the horizontal during prolonged exposure to radial acceleration on a centrifuge, *J. exp. Psychol.*, 63, 1962, 294-301.

[4] Graybiel and Clark, *op. cit.*, 1-28.

Rotation Room which has been described in detail elsewhere.[5] The velocity used in this experiment was 14.0 r.p.m. in a counterclockwise direction which could be produced in from 3–4 sec. and maintained within an error of 4%. *O* was seated in a special chair located within a light proof compartment at the periphery of the room so that his head was 5.5 ft. from the center of rotation which produced a change in the direction of resultant force (*phi*) of 20° at his head. A Fiberglas head holder bolted to the chair, a seat-belt, and other supports maintained him in a fixed though comfortable position, facing the direction of rotation. *O* observed a luminous line broken in the center with a spot. The luminous line was within a collimator mounted rigidly directly in front of *O*. Either *O* or *E* could control the position of the luminous line by turning a knob, and its position was recorded on a large dial in a second compartment. *E* reported the readings to a recorder in the control room through a special communication system so *O* could not hear them. Thus, *O* could indicate his perception of the visual horizontal as a function of time following a rapid change in resultant force and subsequent constant rotation.

(2) Procedure. Each series of observations began with the room stationary. The luminous line was offset from horizontal in a clockwise direction, and then *O* set it to the horizontal. Five such readings were made while the room was stationary. The room was then accelerated quickly to produce a change in *phi of* 20°. The line was offset, and *O* immediately set it to horizontal and maintained it in this position. At 15 sec. following the beginning of rotation and every 15 sec. up to 2 min., *E* made a reading and offset the line as before. The luminous line was turned off after the 2 min. reading and remained off for 45 sec. During this period it was offset to a prearranged position and when the luminous line was turned on *O* set it to horizontal. *E* made a reading at 3 min. and turned it off again for 45 sec. Subsequently, readings were made every minute for 60 min. The offsets were prearranged to make *O*'s task fairly difficult. The reference point was always his last setting. For the 2 min. setting it was offset from the previous setting clockwise +10°; at 3 min. it was offset counterclockwise —10°. Succeeding offsets were +20°, —20°, +30° and —30°. This sequence was then repeated until the 58 additional settings were made. The oculogravic illusion was measured by determining the deviation of *O*'s settings from his static settings. This procedure obtained a record of his perception of the horizontal as a function of time following a change in the magnitude and direction of resultant force acting on him or in other words gave a measure of the oculogravic illusion.[6]

(3) Observers. Five normal men, including one of the authors, were studied. All had normal nystagmus responses to caloric stimulation, normal responses to rotation, and experienced the oculogravic illusion in other experiments. Nine labyrinthine defective (*L-D*) men were also studied. All were deaf and had acquired their deafness in childhood as a sequela of meningitis. All of them had abnormal vestibular responses to caloric stimulation and rotation.

Results. The results showing the change in setting to the horizontal as a function of time for the five normal and and the nine *L-D* men are presented in Fig. 1. The same data combined into blocks of five 1-min. settings are to be found in Table I. The lag-effect was clearly present for both groups (Fig. 1). There was a sharp increase in the oculogravic illusion for about 60 sec. and then a slight reduction with a leveling off thereafter. On the other hand, the normal and *L-D O*s differed from each other in two significant ways: First, at the termination of the lag-effect, the normal group made settings to the horizontal which were closely in accord with the change in the direction of the resultant force, *i.e.* the oculogravic illusion was approximately 20° (Fig. 1, Table I) while the illusion was only about 12° for the *L-D* group; secondly, the settings of the normals did not show a systematic change following the lag-effect where the *L-D Os* showed a continuous increase in the illusion throughout the 1-hr. period of the observations. These differences are clear from an examination of the data

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[5] Ashton Graybiel, Brant Clark, and J. J. Zarriello, Observations on human subjects living in a 'slow rotation room' for periods of two days, *Arch. Neurol.*, 3, 1960, 55-73.

[6] Graybiel, *op. cit.*, 605-615.

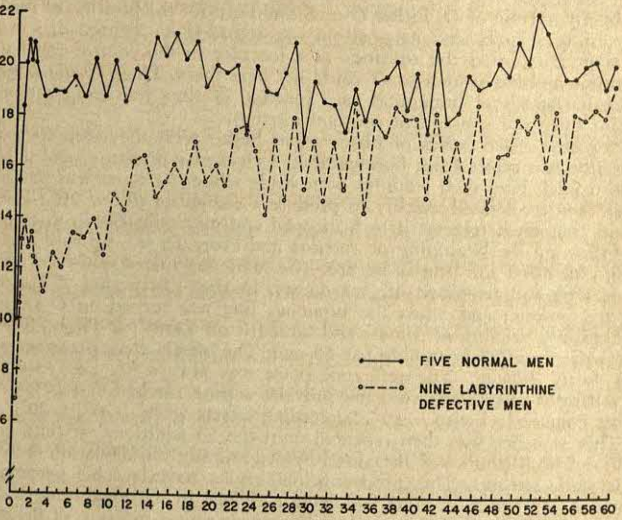

FIG. 1. CHANGE IN THE OCULOGRAVIC ILLUSION IN DEGREES DURING 1 HR. OF CONSTANT ROTATION

TABLE I

MEAN OCULOGRAVIC ILLUSION IN DEGREES FOR BLOCKS OF FIVE TRIALS FOLLOWING ABRUPT ACCELERATION TO CONSTANT VELOCITY IN A ROTATING ROOM

	Blocks of trials											
Os	1–5	6–10	11–15	16–20	21–25	26–30	31–35	36–40	41–45	46–50	51–55	56–60
Normal	19.3	19.5	19.8	20.3	19.2	19.6	18.3	19.4	18.8	19.9	20.8	19.8
L-D	12.3	13.6	15.4	16.1	16.3	16.4	16.0	17.9	16.2	16.9	17.1	18.4
Diff.	7.0	5.9	4.4	4.2	2.9	3.2	2.3	1.5	2.6	3.0	3.7	1.4

(Fig. 1, Table I). A trend analysis was performed for the two groups independently combining the data in blocks of five one-minute trials.[7] The test for trend for the normals was not significant ($p > 0.10$), while the *L-D Os* exhibited a significant trend in the direction of an increase in the oculogravic illusion throughout the hour of testing ($p < 0.01$). Over the first block of five settings (Table I) the median test showed that the normal group had a significantly greater illusion ($p = 0.03$), but over the settings from 56-60 minutes the difference between the two groups was not significant ($p = 0.37$).[8] Thus, although the normals showed a consistently larger mean oculogravic illusion throughout the hour of settings to the horizontal during constant rotation, at the end of the period the difference was only 1.4° and not statistically significant. It should be noted that nonparametric analyses have been used here because the variance in performance was substantially greater for the *L-D* than for the normal group. This very large variance of the *L-D* group was a crucial factor in the failure to find a statistically significant difference between the two groups during the final trials.

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[7] E. Kurnow, G. J. Glasser, and F. R. Ottman, Statistics for business decisions, 1959, 111-116.

DISCUSSION

The results of this experiment support the results of earlier experiments for normal *O*s in that a lag was present and also in that for frequent measurements following this initial period, there was no systematic change in the effect.[9] The data showing a greater oculogravic illusion in normal than in *L-D O*s for short periods of observation have also been reported.[10] On the other hand, the finding that, after prolonged exposure to a change in magnitude and direction of resultant force, the *L-D O*s closely approximated the settings of the normals has not been found elsewhere in the literature. Consequently, this prolonged lag-effect requires special comment with regard to the possible sensory processes involved.

It has been widely assumed that the otolithic organs play a crucial role in the perception of the postural vertical in normals, but the experimental evidence shows that although *L-D O*s make greater errors, the differences between their average settings and those of the normals *O*s are small, particularly after practice.[11] This makes it clear that other gravitational receptors are involved in this task. It has also been shown that *L-D O*s are more affected by prolonged bodily tilt than are normals.[12] There is extensive evidence that the otoliths play a key role in the perception of the oculogravic illusion.[13] The *L-D O*s differ significantly from normals in both the static and the dynamic phases of the illusion for observations made over a period of 2–3 min. The fact that the oculogravic illusion increases throughout 1 hr. of constant rotation for the *L-D O*s suggests that information from other sensory mechanisms becomes crucial in the perception of the visual horizontal.

In normals, the lag appears to be due to a gradual change in the relative weighting of visual and vestibular information over time.[14] It is suggested that in normals the information from the otoliths is more heavily weighted than data from tactile and proprioceptive receptors. On the other hand, in *L-D Os*, the information from the tactile and proprioceptive receptors must predominate. It is further suggested that for *L-D Os* the rapid change followed by a gradual change in the oculogravic illusion may be associated with differential weighting of information from the highly complex sensory mechanisms involved in tactile and proprioceptive perception.[15] It has been shown that for both of these sensory processes there are individual fibers which adapt slowly and others which adapt rapidly. For example, Lowenstein has found that fibers from certain stretch mechanoreceptors in the skin of the frog "show no signs of adapting to zero over the period of 40 minutes of observation,"[16] whereas Gray and Matthews report that fibers from the Pacinian corpuscles in the cat's toe adapt in about 2 min.[17] Thus, it would appear that immediately following a change in the direction and magnitude of the resultant force acting on an *L-D O,* there would be a relatively large amount of synergistic information from several tactile and proprioceptive receptors in support of a new spatial orientation and opposed to the visual information. This would be expected to produce an initial rather rapid change in orientation, but as the adaptative process progressed (*e.g.* in Pacinian corpuscles), information from the slowly adapting receptors would become more heavily weighted and, over time, would lead to an orientation to the resultant force similar to that of normals. It should be kept in mind that for *Os* with normal vestibular information available, the otolithic information receives the heavier weighting, and the lag effect is relatively short. Thus, it would appear that adaptative processes associated with differential weighting of sensory information over time will lead to the lag-effect in both normal and *L-D Os* but with different sensory processes involved in the two groups.

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[8] Sidney Siegel, *Nonparametric Statistics*, 1956, 4-58.

[9] Clark and Graybiel, *op. cit.*, 294-301; Ashton Graybiel, and R. H. Brown, The delay in visual reorientation following exposure to a change in direction of resultant force on a centrifuge, *J. gen. Psychol.*, 45, 1951, 143-150.

[10] Graybiel and Clark, *op. cit.*, 1962, 1-28.

[11] Clark and Graybiel, Perception of the postural vertical in normals and subjects with labyrinthine defects, *J. exp. Psychol.*, 65, 1963, 490-494.

[12] Clark and Graybiel, Perception of the postural vertical following prolonged bodily tilt in normals and subjects with labyrinthine defects, *Acta Oto-laryng.*, 58, 1965, 143-148.

[13] Graybiel and Clark, *op. cit.*, 1962, 1-28.

SUMMARY

Five normal and nine labyrinthine defective men were studied in a slow rotation room which produced a change in resultant force of 20° on them. The men faced in the direction of rotation and at 1-min. intervals set a luminous line to the perceived horizontal in darkness for 1 hr. The results for the normal men confirmed an earlier study showing no systematic change in the perception of the visual horizontal after an initial lag. In contrast the labyrinthine defective (*L-D*) men showed a smaller, rapid, and then gradual change in the perception of the visual horizontal throughout the 1 hr. of constant rotation. At the end of that period there was no significant difference between the two groups. These results are discussed in terms of a differential weighting of the synergistic information available to the two groups.

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[14] Harry Helson, *Adaptation-Level Theory,* 1964, 89-94.

[15] J. E. Rose and V. B. Mountcastle, Touch and kinesthesis, in J. Field, Handbook of physiology, Section 1: Neurophysiology, Vol. 1, 1959, 387-429.

[16] W. R. Lowenstein, Excitation and change in adaptation by stretch mechanoreceptors, *J. Physiol.,* 133, 1956, 588-602, esp. 596.

[17] J. A. B. Gray and P. B. C. Matthews, Responses of Pacinian corpuscles in the cat's toe, *J. Physiol.,* 113, 1951, 475-482.

FACILITATION OF MEMORY BY EXPERIENTIAL RESTRICTION AFTER LEARNING

By Robert J. Grissom, Princeton University

The classic studies showing that retention was aided by sleep were significant in detracting from the theory of disuse to explain the losses in long-term memory, thereby leading psychology to search for other mechanisms in forgetting.[1] These early studies did not completely dispel the conception of disuse, however, inasmuch as some forgetting occurred during sleep. Undoubtedly, much of the decrement in any study of retention is attributable to proactive interference from the lists learned earlier before the test material.

The decrement in retention in the sleeping condition may also be due in part to the difficulty in insuring that sleep follows immediately upon learning and that the retention-test immediately follows sleep. Some minutes after learning were spent in leaving the learning station, preparing for sleep, and in being roused from sleep and returned to the learning station for the retention-test. Events that produce retroactive interference are possible during the period between learning and sleeping, and the return to the learning station after interrupted (and sometimes brief) sleep may fail to reinstate the set of the *S* and the full response to the original context.

Some of the procedural difficulties using sleep may be eliminated by substituting a condition of minimal activity which extends without interruption from the time of learning to the moment of the retention-test. It was hypothesized that such a period of quiescence would facilitate retention by affording little opportunity for the formation of competing responses.

Method. The hypothesis was tested by means of environmental restriction of the *Ss* by confining them to soundproof and dark chambers. Confinement was the inter-

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[1] J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this Journal, 35, 1924, 605-612; E. B. van Ormer, Retention after intervals of sleep and of waking, *Arch. Psychol.*, 21, 1932, (No. 137) 5-49; Hiroshi Minami and K. M. Dallenbach, The effect of activity upon learning and retention in the cockroach, this Journal, 59, 1946, 1-58.

vening condition during the intervals of retention. The chambers have been described elsewhere.[2]

Subjects. Eighty men, paid volunteers who contracted to serve "from 8–24 hr. in isolated confinement," were drawn from the area of Princeton University. They were randomly divided into groups of 10 which underwent 8, 16, 20, or 24 hr. of complete isolation, *i.e.* in which their activities were controlled and restricted. To insure that they were alert at the time of learning, they were instructed to sleep for normal periods prior to the day spent in isolation.

Procedure. The *Ss* were isolated individually and were restricted to a bed in one of the chambers. They were subjected to no encumbrances other than a urinal belt and tube and were told to remain quietly in bed throughout the period of isolation. Sitting up, exercising, and vocalizing were expressly forbidden, but occasional turning and rolling to maintain comfort were permitted. The *Ss* were further informed that they would be monitored through a microphone in the chamber for violations of these restrictions and that *Ss* violating them would be dismissed from the experiments without reimbursement. Liquid food, water, and the urinal jar were accessible by merely turning to one side of the bed. An emergency, chemical commode was at the foot of the bed. Throughout isolation the chamber was dark and silent. The *Ss* were presented with no tests or communications other than those reported here. They were free to sleep at will.

This study was mainly concerned with the over-all differences in retention between the restricted and the control groups of *Ss*. Retention as a function of time was of little interest to us. The time of day the experiments were conducted was, therefore, the same for the different groups. Thus, all the *Ss*, restricted and control in a given condition, learned at the same time of day and were tested for retention at the same time of day. Such procedure also makes it more likely that *ad lib* sleep will not vary much between restricted and control groups. To avoid learning or testing at late night or early morning hours, all the *Ss* in the 8-hr. groups began their isolation at approximately 9:00 A.M. All other *Ss* began isolation at approximately 5:00 P.M.

After 1 min. of confinement the first of *S*'s taped instructions was played through a speaker within the chamber. He was told to listen carefully to a passage which was to be played to him and then immediately thereafter to try to repeat the passage verbatim (immediate recall test). The passage, 182 words long, was adapted from Tolstoy's *War and Peace.* The *S*'s recall was tape recorded.

Upon completion of the immediate recall-test, control *Ss* were released from the chamber, told to go about their daily affairs, and asked to return to the laboratory for some "further work" in 8, 16, 20 or 24 hr. (depending on their random assignment of retention-interval). These control *Ss* were given no further information. After the immediate recall-test, the experimental *Ss* were left undisturbed in the chamber for the same periods of 8 to 24 hr.

Upon termination of the retention-period and before release from the chamber and its restrictions, confined *Ss* were again asked to try to repeat the passage *verbatim.* Control *Ss* were returned to confinement and the restrictions of the chamber for the purpose of this terminal recall.

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[2] Peter Suedfeld, R. J. Grissom, and Jack Vernon, The effects of sensory deprivation and social isolation on the performance of an unstructured cognitive task, this JOURNAL, 77, 1964, 112 f.

After the terminal test, each *S* was questioned as to the extent of his thoughts about the passage during the interval and as to his anticipation of the terminal test. To counter any reservations an *S* might have had about admitting to having practiced the material or having expected a retest, such reports were earnestly solicited and encouraged. *E* told *S* that anticipation of a retest and practice were common and expected in such experiments and that his interest was in determining the degree of such reflections for each *S*. In an attempt to assess the role of rehearsal with care, a cautious criterion for classifying an *S* as a rehearser was adopted. Any *S* who indicated that he had thought about the passage, even if only once, fleetingly, or had considered a retest as a possibility was classified as a rehearser.

Responses were scored for the number of words from the passage which were correctly reproduced, irrespective of correspondence of sequence, each word being valued as one point. This scoring method was adapted from that of King, who found it to correlate highly with counts of retained ideas, scaled scores based on judges' rankings, and with other measures of retention.[3] Identification of each *S*'s taped recall as to treatment-group was unknown to *E* during scoring. The difference between immediate recall and terminal recall ($T_1 - T_2$) was computed for each *S* as the measure of forgetting.

Results and discussion. A two-way analysis of variance revealed significantly superior retention in experientially restricted as compared with the control groups ($p < 0.05$). The difference between mean immediate recall scores of pooled experimental and pooled control groups was not significant. The difference between mean loss in the experimental and control groups was in the predicted direction for each retention-interval, as can be seen in Fig. 1, but was significant in a *t*-test only at 24 hr. ($p < 0.02$, two-tailed).[4] Moreover, the only group to have declined significantly in retention from the immediate to the terminal recall test was the 24-hr control group ($p < 0.01$ in a two-tailed *t*-test for correlated samples). The difference in immediate recall scores of the 24-hr. groups was not significant.

Consideration of retention as a function of interval in this study is not wholly legitimate because neither learning nor testing times were equated for the various intervals, for reasons already stated. Furthermore, *ad lib* sleep was more likely in the 16-, 20-, and 24-hr. intervals since the isolation began at approximately 5:00 P.M.

Objections notwithstanding, the manifest divergence of the lines in Fig. 1 compel consideration of the trends, which proved to be insignificantly different as indicated by a lack of interaction between variables in the analysis of variance. Thus, the apparent reminiscence effect in Fig. 1 was not real. This result was confirmed by a test of slope, which revealed that the slope of the trend for the experimental groups departed insignificantly from zero.[5] In short, relative to variability the divergence of the trend lines is small. The *SD*s in the 8-, 16-, 20-, and 24-hr. groups were

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[3] D. J. King, On the accuracy of written recall; A scaling and factor analytic study, *Psychol. Rec.*, 10, 1960, 113-122.

[4] Results from the 24-hr. groups have been presented in a brief report, R. J. Grissom, Peter Suedfeld, and Jack Vernon, Memory for verbal material: Effects of sensory deprivation, *Science*, 138, 1962, (No. 3538), 429-430.

[5] Don Lewis, *Quantitative Methods in Psychology*, 1960, 122 ff., 395 ff.

7.9, 7.5, 5.1, and 6.5; and 10.3, 12.5, 10.8, and 7.3 for the experimental and control groups, respectively. Variances were not significantly heterogeneous.

The slight extent of forgetting obtained over-all in this experiment and the small differences in forgetting by the control and experimental groups were not surprising inasmuch as the study compared experimental groups, which were predicted to yield high retention, with control groups, which could not have been expected to undergo much forgetting. Furthermore, although comparison between experimental and control groups is proper, no determination can be made of absolute loss in retention because the level immediately after T_1 is not known. It is quite possible that retention on T_2 profits from contextual cues gained from T_1, especially if there is facilitating rehearsal.

As to rehearsal, no *S* reported to have done more than to think of the passage occasionally and few had expected a retest. By the strict criteria already described, however, 28% of the control and 25% of the experimental *Ss* were classified as rehearsers, frequencies which differed insignificantly in a Chi-squared test. This finding of no greater rehearsal in the experimental than in the control groups is consistent with the frequent observation in the Princeton Sensory Deprivation Laboratory and elsewhere that restricted *Ss* report difficulty in sustaining thought on any topic.[6] Furthermore, it is likely that the use of an immediate recall test minimized rehearsal by leading many *Ss* to believe that they would not encounter the originally learned passage again. Many of the *Ss* who reported that they had not expected a terminal test said that after immediate recall they had expected to be tested later on *new* tasks.

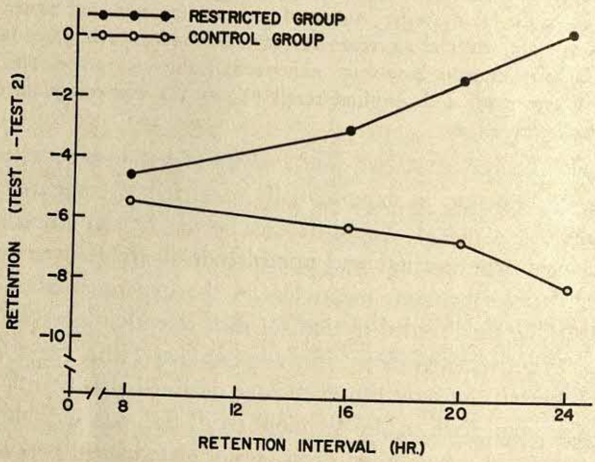

FIG. 1. EFFECT OF INTERVENING ACTIVITY ON RETENTION

The mean loss of the experimental *Ss* who were classified as rehearsers was +2.63 and that of the experimental *Ss* who were not so classified was −2.87. The difference in scores between rehearsing and non-rehearsing experimental *Ss* falls just short of significance at the $p < 0.05$ level in a two-tailed t-test, whereas the difference in loss scores between rehearsing and non-rehearsing control *S*s did not approach significance. The significance of the difference in scores of the experimental and control groups at 24 hr. is maintained, however, when the datum from the one experimental *S* who was classified as a rehearser in these groups is excluded from the t-test ($p < 0.05$, two-tailed). Finally, it should be noted that the reasonable assumption that rehearsal generally facilitates retention does not have firm empirical support from other studies, although the problem of rehearsal would seem to be especially critical with contextual material scored by a word-count.[7]

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[6] G. E. Ruff, E. Z. Levy, and V. H. Thaler, Factors influencing the reaction to reduced sensory input, in P. Solomon, P. E. Kubzansky, P. H. Leiderman, J. H. Mendelson, R. Trumbull, and D. Wexler (eds.), *Sensory Deprivation,* 1961, 72-90.

There is the possibility that retention may be depressed by stress induced by experiential restriction, and certainly 'sensory deprivation' and its variants are frequently associated with stress, hallucinations, and psychotic states.[8] Nonetheless, confined *S*s in the present study volunteered no reports of hallucinations, observed that the period of isolation seemed to terminate very quickly, and rarely reported having had any more stressful experiences than occasional minor physical discomforts. This apparent innocuousness of experiential restriction, and the low 7% attrition-rate of experimental *S*s, is consistent with past observations at the Princeton Laboratory that when *E* plays a reassuring, informative, and supportive role prior to isolation, experiential restriction is not stressful.

The retention-data in this study are in obvious accord with the interference-theory, but do not decisively disprove the notion of spontaneous decay in that there may have been loss during the period between T_1 and T_2. As has already been noted, recall might have been greatest immediately following T_1 due to arousal of contexual cues by that test.

It is not possible to identify more closely just what aspect of experiential restriction is responsible for aiding retention, but the results of this experiment may be related to the similar effect of intervening darkness in animal studies.[9]

SUMMARY

It was predicted that a period of quiescence interpolated between learning and a retention test would facilitate memory.

*S*s who were confined to a bed within a soundproof and dark chamber for varying intervals after hearing a prose passage retained the material significantly better than did control *S*s, who went about their daily affairs during the retention intervals. The differences were not readily attributable to rehearsal, so that an interpretation in terms of interference is favored. It is argued, however, that decay as a process influencing retention cannot be ruled out by the results.

The treatment of the experimental and control *S*s differed in no other respect than in experience during the retention interval. Steps taken to avoid stressing the restricted *S*s were successful.

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[7] B. J. Underwood and Geoffrey Keppel, An evaluation of two problems of method in the study of retention, this JOURNAL, 75, 1962, 1-17.

[8] Ruff *et al.*, 72-90.

[9] R. B. Malmo, Interference factors in delayed response in monkeys after removal of frontal lobes, *J. Neurophysiol.*, 5, 1942, 295-308; Robert Thompson, Retroactive effect of interpolated visual stimulation, *Psychol. Rep.*, 3, 1957, 183-188.

DIFFERENTIAL EFFECTS OF CENTRAL AND LATERAL FIXATION ON AFTER-EFFECTS OF EXPANSION AND CONTRACTION

By HERMAN H. SPITZ, Bordentown, New Jersey

If one fixates a rotating, contracting, Archimedes spiral, an apparent movement in the opposite direction—an after-effect of expansion—will be observed when the spiral is stopped. Similarly, when a rotating, expanding, Archimedes spiral is stopped, an after-effect of contraction will occur. There is ample evidence that the after-effect of expansion of a centrally fixated rotating spiral is stronger than the after-effect of contraction.[1] Explanations for these differences include the suggestion that the expanding inspection-spiral dissipates some of its neural effects in the surrounding field, resulting in a contractive after-effect of less than full strength. On the other hand, with the contracting inspection-spiral there is no place for these effects to dissipate, and the after-effects of expansion is full-blown.[2] In fact, there may even be an added effect of "rebound."

Recent findings with a spinning fragmented circle, however, appear to make the above explanation inadequate. When the fragmented circle is used as an inspection-figure there is no movement of lines across the entire disk to its edge or center and yet, with an appropriate stationary test-figure, after-effects of expansion are obtained but after-effects of contraction are not.[3]

Another proposal to account for the difference between after-effects of expansion and contraction has recently been introduced.[4] This proposal is based on the permanent adaptation of small contractive after-effects posited to occur in our normal environment. When we move toward a specific point in our environment, there is a radial expansion of the visual field surrounding this point. When we stop, there is a small, perhaps

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* Received for publication January 24, 1966. From the Edward R. Johnstone Training and Research Center.

[1] Paul Bakan and Kiyoe Mizusawa, Effect of insepection time and direction of rotation on a generalized form of the spiral aftereffect, *J. exp. Psych.*, 65, 1963, 583-586; G. A. Pinckney, Reliability of duration as a measure of the spiral aftereffect, *Percept. mot. Skills*, 18, 1964, 375-376; W. C. Roehrig and Jacques Rutschmann, Procedural considerations of the spiral aftereffect test, *ibid.*, 17, 1963, 551-557; H. H. Spitz and R. S. Lipman, Some parameters in the perception of the spiral after-effect, *ibid.*, 9, 1959, 81.

[2] C. G. Costello, Massed practice and spiral after-effect, *Percept. mot. Skills*, 12, 1961, 11-14; H. H. Spitz, Neural satiation in the spiral aftereffect and similar movements aftereffects, *ibid.*, 8, 1958, 207-213.

[3] Spitz, Expansion aftereffects without perceived contraction of the inspection-figure, this JOURNAL, 78, 1965, 582-588.

[4] Spitz, *op. cit.*, 587.

subliminal, after-effect of contraction, much as an expanding inspection-figure becoming its own stationary test-figure. Since we have moved forward, or been moved forward, frequently since birth, there have been many opportunities for such a process to have occurred.

Conversely, we move backward infrequently, and when we do we rarely fixate on any single point from which we recede. Therefore, there has been much less radial contraction of the visual field in our natural environment, and the occurrence of small after-effects of expansion should have been extremely rare.

If one grants the possible existence of long-term, permanent adaptation of after-effects of movement, similar to short-term, temporary adaptation reported by some investigators,[5] then it follows that we would have built up relatively more adaptation to after-effects of contraction than to expansion, and that this greater adaptation will have raised the threshold of the after-effects of contraction. Consequently, after-effects of expansion should be, and are, stronger than after-effects of contraction when fixation is at the center of the spiral. On the other hand, and this is the major prediction stemming from the adaptation-hypothesis, after-effects should not differ when fixation is lateral to the spiral. With lateral fixation, the spiral is projected onto a cortical area asynchronous with the frequent radial motion occurring from birth. To test this deduction, the following experiment was performed.

EXPERIMENT

Subjects. The *Ss* were 86 women students who volunteered to serve in the experiment. Their average age was 19.18 ± 0.4 yr.[6] In addition to these *Ss*, 19 served in the experiment but were dropped because they misunderstood the instructions and signaled when the objective movement was interrupted (see Results).

Apparatus. An 8-in. glazed print of an Archimedes spiral, made up of 3¼ turns of a 0.5-in. wide black line on a white field, was prepared and glued to an 8-in. cardboard disk. This was mounted on a shaft which, at a point 8.44 in. above the table top, projected through the horizontal center of a 48-in. wide by 24.25-in. high, gray background. The two lateral fixation-points were round, metal disks, 0.56 in. in diameter, the center of each of which was placed 0.44 in. to the right or left of the rim of the spiral, on a level with its center. These metal disks were held by smaller cylinder magnets which protruded slightly from the screen. One side of each metal disk was painted the same gray as the background; the other side was an unpainted metallic white. When not used as a fixation-point, either or both disks could be turned to the painted side to avoid distracting *Ss*.

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[5] Costello, *op. cit.*, 12; H. J. Eysenck and S. B. G. Eysenck, Reminiscence on the spiral after effect as a function of length of rest and number of present trials, *Percept mot. Skills*, 10, 1960, 93-94.

[6] The author is most grateful to Marshall P. Smith, Chairman of the Psychology Department at Trenton State College, for securing subjects and providing space for this experiment.

The major source of light was a 150-w. flood-light set 6 ft. above the floor and slightly to the right and rear of the seated *S*. As measured from *S*'s eye-level, the spiral reflected about 25 ft.-L. of light.

Procedure. S sat 6 ft. from the spiral which was rotated at 100 r.p.m., clockwise for after-effects of contraction and counerclockwise for after-effects of expansion. The rate of which the spiral approached a stop, and stopped, was the same in both directions. After having been given a brief explanation of a movement after-effect, and told that *E* was interested in how long the after-effect lasted, *S* was given a button to hold in her lap and told to continue to fixate the fixation-point until the after-effect stopped, at which point she was to press the button. The fixation-point was either the small black bolt-head in the center of the spiral or one of the two lateral disks.

Duration of the objective rotation was controlled by a timer which, after 45 sec., automatically stopped the spiral and activated a 0.001-min. timer. A press on the button held by *S* stopped the 0.001-min. timer, registering the duration of the after-effect.

TABLE I

CONDITION AND ORDER OF PRESENTATION FOR THE ORIGINAL SIX GROUPS

N	Group	Fixation	Trial 1	Trial 2
10	1	Left of spiral	Expansion A–E	Contraction A–E
10	2	Left of spiral	Contraction A–E	Expansion A–E
10	3	Right of spiral	Expansion A–E	Contraction A–E
10	4	Right of spiral	Contraction A–E	Expansion A–E
23	5	Center of spiral	Expansion A–E	Contraction A–E
23	6	Center of spiral	Contraction A–E	Expansion A–E

The *S*s were randomly placed into one of the six groups shown in Table I. No practice trial was given. Each *S* served for two trials only, half the *S*s receiving the expansive after-effect first and the contractive after-effect second, half receiving these conditions in reverse order. There was an 80-sec. rest from the end of the after-effect on Trial 1 to the start of the spiral for Trial 2. During most of this 80-sec. rest-period the spiral was covered with white cardboard.

Results. A number of *S*s (19) pressed the button almost immediately after the spiral stopped spinning. Most of these *S*s remarked, or admitted when questioned, that the after-effect continued to occur after they had responded. Since the *S*s were not given any practice trial, some attrition was to be expected. Therefore, it was arbitrarily decided to exclude all *S*s who responded within 2 sec. after the spiral stopped, or who remarked that the after-effect continued after they pressed the button, and 19 *S*s were dropped for this reason.

Since no significant differences were found between fixation to the left (Groups 1 and 2) and fixation to the right (Groups 3 and 4), for the statistical analysis Groups 1 and 3 were combined into a single Lateral, Expansion First Group, and Groups 2 and 4 into a single Lateral, Contraction First Group.

A Lindquist Type III analysis of variance was performed on the data. Significant main effects and interactions were: Fixation (centrally fixated after-effects persist longer than laterally fixated after-effects) $F = 6.75$, $P < 0.02$; Trials (Trial 1 after-effects persist longer than Trial 2 after-effects) $F = 32.50$, $P < 0.001$; After-effects (expansion after-effects persist longer than contraction after-effects) $F = 7.00$, $P < 0.01$; and the After-effects by Fixation interaction (expansion after-effects persist longer than contraction after-effects during central but not during lateral fixation) $F = 8.00$, $P < 0.01$, These results are shown graphically in Fig. 1.

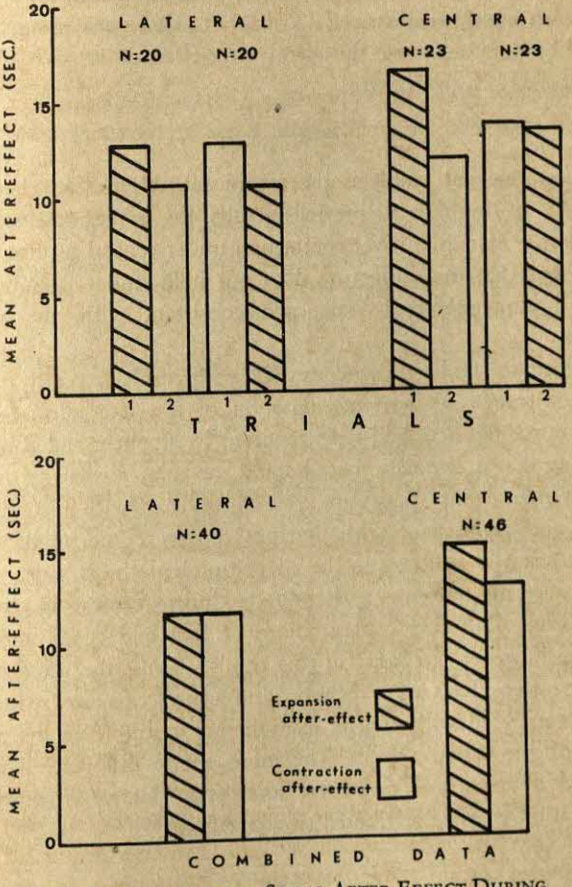

FIG. 1. DURATION OF THE SPIRAL AFTER-EFFECT DURING LATERAL AND CENTRAL FIXATION

Reading from Table I, in the upper left portion of this figure, Groups 1 and 3 (expansion after-effect first) have been combined; and Groups 2 and 4 (contraction after-effect first) have been combined. In the lower portion, the lateral condition combines Groups 1 through 4, and the Central condition combines Groups 5 and 6.

The between *Ss* error term was more than four times larger than the within *S*s error term, indicating rather large individual differences, while intra-individual scores are fairly stable. In fact, the intercorrelation of each individual's score on the first and second trials, regardless of which after-effect was given first, was 0.56 for central fixation and 0.73 for lateral fixation, both highly significant ($P < 0.001$). These results suggest that in studying the parameters of the spiral after-effect it is advisable to have each *S* serve as his own control.

DISCUSSION

The most relevant result is the significant After-effects by Fixation-interaction. On the basis of previous results, the longer persistence of the after-effects of expansion over contraction under central fixation was to be expected, but that this difference does not hold under lateral fixation is a novel finding predicted from, and consistent with, the adaptation-hypothesis.

Supplementary findings are consistent with results recently reported by other investigators. A shortened duration of the after-effect following a spiral of opposite effect had been reported by Roehrig and Rutschmann.[7] The finding that a centrally fixated spiral, no matter what its direction of spin, will induce more persistent after-effects than will a laterally fixated spiral, is not inconsistent with previous reports.[8] One might argue that this latter result is contrary to the adaptation-hypothesis, since adaptation should reduce all movement after-effects during central as compared to lateral fixation. Differing concentrations of rods and cones, and of nerve fibers, may however, account for this result.[9] Thus, the gross process by which the after-effects of expansion and contraction are reduced under lateral as compared with central fixation may result from the anatomical structure of the visual system. The more subtle difference *between* the after-effects of expansion and contraction under lateral as compared with central fixation may be attributed to adaptation.

SUMMARY

After-effects of expansion of a centrally fixated spinning spiral are known to persist longer than after-effects of contraction. It was hypothesized that this difference results from adaptation to the frequent natural occurrence of small, perhaps subliminal, after-effects of contraction. The finding that the after-effects of expansion and contraction do not differ under lateral fixation is congruent with this hypothesis.

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[7] Roehrig and Rutschmann, *op. cit.*, 554.

[8] S. L. Freud, Duration of spiral after-effect as a function of retinal size, retinal place, and hemiretinal transfer, *Percept. mot. Skills*, 18, 1964, 47-53.

[9] Freud, *op. cit.*, 52.

SUPPRESSION IN BINOCULAR VISION

By JAMES ROLAND LAW, Duke University, and
HERBERT F. CROVITZ, VA Hospital, Durham, North Carolina

Three facts of stereoscopic viewing have been known for a long time: (1) When a figure is presented to one eye and a homogeneous ground to the other, the figure will be seen most of the time.[1] (2) When two figures which differ in direction or color are separately presented to the two eyes, the figures alternate in their visibility.[2] (3) When a figure prevails over a homogeneous ground or another figure, it carries some of its background into the binocular percept.[3] Recently, Hochberg has called attention to the suppressive potential of this accompanying background and referred to it as a "contralateral suppressive field."[4] Kaufman has measured the extent of this suppressive zone and found that it is greater in the horizontal than the vertical direction, arguing that this difference stems from an added wake of suppression caused by the apparent movement of the contours over each other during vergence eye-movements.[5]

The present study was designed to answer two further questions. First, is the prevalence of suppressive zones diminished in stereograms that give common contours that can be superposed? Secondly, is the prevalence of suppressive zones diminished in stereograms which have identical superposed contours as compared to disparate superposed contours?

Fig. 1 shows the four stereograms used in this study. In each case the perceived figure is a cross. In Stereogram 1 there is a single black vertical bar to the left eye and a single black horizontal bar to the right eye: the two eyes do not have a com-

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* Received for publication April 18, 1966.

[1] Herman Helmholtz, *Handbuch der physiologischen Optik*, trans. J. P. C. Southall, 3, 1925, 496; B. B. Breese, On inhibition, *Psychol. Monogr.*, 3, 1899, (No. 11), 1-66.

[2] Charles Wheatstone, *Lond. R. Soc. Philos. Trans.*, 371, 1838; Helmholtz, *op. cit.*, 494.

[3] P. L. Panum, *Physiologische Untersuchunger uber das Sehen mit zwei Augen*, Kiel, 1858, as cited by G. Roenne, *Acta Ophthal.*, 34, 1965, 1 ff.

[4] Julian Hochberg, The suppression and imitation of binocular stereopsis, paper read at the Fifth Annual Scientific Meeting of the Psychonomic Society, October, 1964.

[5] Lloyd Kaufman, On the spread of suppression and binocular rivalry, *Vision Res.*, 3, 1963, 401-415.

mon superposable contour. In Stereogram 2 the eyes are given a common superposable black vertical bar. Stereograms 3 and 4 similarly have a common superposable vertical bar, but now the bars are disparate with regard to color. Suppression-time when the vertical bars are identical can be compared to suppression-time when they are disparate. Further, color coding in Stereograms 3 and 4 permits a study of suppressions in the vertical bars in relation to suppressions in the horizontal bar.

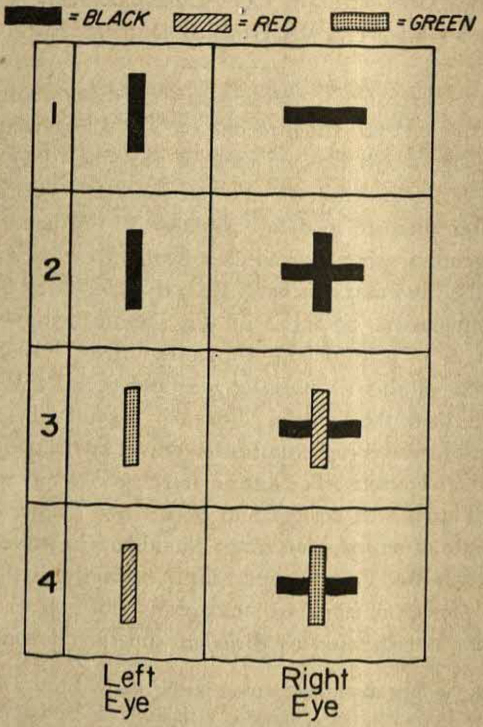

Fig. 1. Stereograms Used in this Study

All bars were 3.2 mm. by 25.4 mm. and were shown on a white background. The viewing apparatus was an Abrams desk stereoscope which gives magnification of about × 1.5 at the viewing distance of 14 cm. The *S*s viewed the stereograms in consecutive periods of 30-sec. duration and were instructed to press a key whenever any part of the black horizontal bar disappeared, releasing it only when the black fully returned. The key was connected to a Standard electric timer which automatically added the seconds of suppression. For each of the chromatic stereograms (3 and 4) two separate viewing periods were given. *S* pressed the key in one 30-sec. period only when the vertical bar was seen as green; and in the other only when the vertical bar was seen as red (and concomitantly there was disappearance of the horizontal bar). This resulted in a total of six viewing periods; the order of periods was separately randomized for each *S*. There were 16 *S*s; 14 of them undergraduates without previous experience in experiments on binocular vision.

Results. Table I shows the means and standard deviations for the number of seconds there were disappearances in the black horizontal bar in the six conditions. For Stereograms 3 and 4, the "a" conditions refer to disappearances when the color of the left-eye vertical bar is seen, and the "b" conditions refer to disappearances when the color of the right-eye vertical bar is seen.

Suppression-times varied to a highly significant degree over the six conditions ($F = 31.43$, $df = 5, 75$; $p < 0.001$). Stereogram 1 had the highest suppression-time and differed significantly from all other conditions. The "b" conditions for Stereograms 3 and 4 had the lowest suppression-times and were the lowest for every *S*. There was no significant difference among Conditions 2, 3a, and 4a ($F = 2.19$, $df = 2, 30$).

TABLE I

MEANS AND SD OF SUPPRESSED TIME IN THE SIX CONDITIONS

	Conditions					
	81	2	3a	3b	4a	4b
Mean	18.24	12.38	9.25	1.44	9.88	1.88
SD	7.66	5.23	3.75	1.77	5.92	2.17

Discussion. The fact that there was significantly more suppression in Stereogram 1 than in any stereogram with superposable vertical bars might be traced to more convergence-slippage in viewing Stereogram 1, consistent with Kaufman's claim that the spread of suppression is greater when there is phenomenal movement of one contour over another.[6] Breese long ago argued that such movement is one of the factors controlling visual suppression.[7] This factor might be maximally involved in Stereogram 1 where there is no common contour available for the alignment of the optic axes. In the other stereograms, the suppression of one of the vertical contours could allow some slippage of convergence,[8] but doubleness of the vertical bars consequent to it would reëstablish alignment to rid the perceived field of noticeable doubleness.[9] Informal observation, however, shows that when Stereogram 1 and Stereogram 2 are surrounded by frames so that convergence-slippage appears equal and minimal, there is still more suppression in Stereogram 1 than Stereogram 2. Clearly, superposed contours reduce suppression; whether or not differences in convergence and slippage is the correct explanation of this fact must await a study of sensitive recording of vergent eye-movements.

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[6] Kaufman, *ibid.*, 401-415.

[7] Breese, Binocular rivalry, *Psychol. Rev.*, 16, 1909, 410-415.

[8] Such slippage may account for the occasional disappearances associated with the color of the right-eye vertical bar in Conditions 3b and 4b. An alternative is that these reports were due to error in discriminating the rivalrous colors or in key presses prompted by color changes.

[9] G. L. Walls, Is vision ever binocular?, *Optic. J. & Rev. Optom.*, 1948, *85*, 33-46; Hochberg, *op. cit.*; Kaufman, *op. cit.*

A second finding of the present investigation is that suppression-time was not different among Conditions 2, 3a, and 4a. One possible interpretation of this result is that the identical mechanisms are at work in each of these cases. In Stereograms 3 and 4 the color coding made it obvious that disappearances in the horizontal bar are strongly linked to appearences of the left-eye vertical bar. In Stereogram 2, where two identical vertical bars are superposed, matters are different. There is no visual warrant for saying that the left eye vertical bar is seen and is responsible for the disappearances in the right-eye vertical bar and in the right-eye horizontal bar. But when it is remembered that each bar is surrounded by a suppressive zone which is part of its background, these relationships fall into place. Seeing disappearances in the horizontal bar is actually seeing the surround of the left-eye vertical bar; and seeing the right-eye horizontal bar entirely black is seeing the surround of the right-eye vertical bar. Moreover, when a particular surround is seen, the figure encompassed by it must be seen also; otherwise a surround would be seen without its figure. It seems, then, that suppressions in the horizontal bar entail visibility of the left vertical bar and concommitant suppression of the right vertical bar. This supports the interpretation that the mechanisms at work in Stereograms 3 and 4 operate also in Stereogram 2.[10]

Immediately involved in such an implication are "Suppression-Theories" of binocular vision. These theories have a long history and are again receiving careful attention.[11] As the claim may be put, the two eyes do not see any part of the visual field at the same place at the same time. The difficulty in showing that this is so (or not so) involves principally the fact that, given the superposition of identical figures, the suppression of either one or of neither yields the same percept.

One possible way of reducing this difficulty is suggested by the interaction between the stimulus-materials of this study and the idea that visible contours are accompanied by suppressive zones. Thinking about a figure as dressed in a suppressive cloak of its background increases the saliency of such easily ignored observations as these: (1) Most often, existing suppressions are not detected because backgrounds are not discriminably different. This is true even in rivalry-studies when, say, a green bar and a red bar, both with white backgrounds, are superposed by stereoscopic viewing. The rivalry is seen to take place only in the bars. The present study, however, strongly suggests that the white surrounding the bar when it is red is not the same white which surrounds it when it is green; (2) Rivalrous contours, because of differences in their backgrounds, serve the function of making suppression noticeable. The importance of this way of viewing the matter for a suppressive theory of binocular vision is the resulting accessibility of a technique for detecting suppression in those parts of the visual field where the two eyes have identical stimuli: whenever a rivalrous monocular contour is placed adjacent to superposed identical figures, and is intruded upon by the contralateral background, it may be taken as an indication that the figure which carries its background has suppressed the other figure, totally or partially.

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[10] The same mechanisms are at work in Stereogram 1 also. The visible left-eye bar is responsible for disappearances in the right-eye horizontal bar and for suppressions of the background projected to the same place by the right eye.

[11] Hochberg, *op. cit.;* Harry Asher, *The Seeing Eye,* 1961, 216; F. H. Verhoeff, Panum's areas and some other prevailing misconceptions concerning binocular vision, *Amer. ophth. Soc.,* 57, 1959, 37.

The results of this experiment do not, of course, establish any suppressive theory of binocular vision. Indeed, it still seems impossible to test such theories by direct observation.[12] The method outlined here moves in the direction of the next best alternative—providing a demonstration in which the length of the inference-chain is minimal. The question is whether this method can further clarify the role of suppression in binocular vision.

SUMMARY

*S*s viewed Helmholz's crossed-bar stereogram and monocular-binocular variants of it. Duration of suppression in the horizontal bar was measured in 30-sec. observational periods. Suppression-time was significantly reduced when a binocular superposable contour was available and it was not significantly changed when the superposable contours differed in color as compared to when they were identical in color—so long as the color seen was the color of the superposable vertical bar in the half-view not containing the horizontal bar. When that vertical bar was suppressed, little suppression of the horizontal bar occurred. The results imply a method for detecting suppression in those parts of the visual field containing identical contours.

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[12] Walls, *op. cit.*, 40.

THE INFLUENCE OF STIMULUS-MODALITY AND DURATION ON CHANGES IN TEMPORAL JUDGMENTS OVER TRIALS

By JOEL S. WARM and EMERSON FOULKE, University of Louisville,
and MICHEL LOEB, US Army Medical Research Laboratory

Several investigations of time-estimation have reported that judgments given to a constant interval tend to grow longer over a series of trials.[1] This effect of repetition has been termed the "serial position-gradient."[2] To date, little effort has been made to establish the dependence of this effect on the duration of the stimuli estimated or the sensory modality in which they are presented.[3] Accordingly, the present study was undertaken to assess the effects of stimulus-modality and duration on the tendency for temporal estimates to grow longer with repetition.

Method: (1) Subjects. One hundred twenty students, 57 men and 63 women, served as *O*s. All were naïve as to the nature of the study.

(2) The production method. The method of production was used to obtain temporal judgments. As described by Bindra and Waksberg, this technique requires *O* to delimit an interval of time specified by *E*.[4] In the present study, *O* marked off the time by pressing and releasing a key. It should be noted that this procedure necessarily provided *O* with tactual kinesthetic stimulation during each response.

(3) Experimental design. O was asked to produce four intervals, viz., 0.5, 3.0,

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* Received for publication November 29, 1965. The authors are indebted to Miss Claudia Monroe and Mr. Kent TeVault for technical assistance. This research was facilitated by a contract from the Office of The Surgeon General, US Army, and by a grant from NASA.

[1] R. K. S. DeWolfe and C. P. Duncan, Time estimation as a function of level of behavior of successive tasks, *J. exp. Psychol.*, 58, 1959, 153-158; M. E. Eason and J. S. Kafka, Diagnostic implications of a study in time perception, *J. gen. Psychol.*, 46, 1952, 169-183; J. R. Kleiser, The effect of habit formation, mental activity and gross muscle movements on the reproduction of time. Unpublished Doctoral dissertation, University of Pennsylvania, 1953; Ellen Robinson, The effect of degree of knowledge of results on time estimation, *Psychol. Rec.*, 13, 1963, 355-360; J. S. Warm, J. R. Morris, and J. K. Kew, Temporal judgment as a function of nosological classification and experimental method, *J. Psychol.*, 55, 1963, 287-297; H. L. White, Comment on the "Positive Feedback" method of time estimation, *Percept. mot. Skills*, 19, 1964, 851-854.

[2] J. L. Falk and Dalbir Bindra, Judgment of time as a function of serial position and stress, *J. exp. Psychol.*, 47, 1959, 279-282.

[3] Sanford Goldstone and J. L. Goldfarb, Judgment of filled and unfilled durations: Intersensory factors, *Percept. mot. Skills*, 17, 1963, 763-774; G. R. Hawkes, R. W. Bailey, and J. S. Warm, Method and modality in judgments of brief stimulus duration, *J. aud. Res.*, 1, 1961, 133-144.

[4] Dalbir Bindra and Hélène Waksberg, Methods and terminology in studies of time estimation, *Psychol. Bull.*, 53, 1956, 155-159.

7.0, and 15.0 sec. These intervals were filled in the following ways: auditory/tactual-kinesthetic (A/TK); electrocutaneous/tactual-kinesthetic (E/TK); and tactual-kinesthetic sensations with no auditory or electrocutaneous components (TK). Ten *O*s were assigned at random to each of the 12 possible combinations of stimulation and intervals. Each *O* produced 30 intervals which were divided into 6 blocks of 5 judgments each.

(4) Apparatus. The *O*s were seated in a sound-deadened chamber having an ambient noise-level of about 40 db. *SPL*. A telegraph key was mounted on a stand in front of *O*.

The auditory stimulus consisted of a 1000 ~ tone. It was presented binaurally to *O* via a headset. Electrocutaneous stimulation consisted of the same signal fed to a stainless steel electrode, 12 mm. in diameter, applied to the volar surface of *O*'s index finger. *O*'s palm rested on a 2-in. square stainless steel passive electrode; no electrode paste was employed. A Ballantine AC voltmeter Model 320, connected across a 100 Ω resistor in series with *O*, was used to measure the current.

Auditory or electrocutaneous stimulation was controlled by *O*'s response-key. During the *TK* sessions, the oscillator was turned off. *O*'s productive judgments were recorded to the nearest 1/100 sec. by means of a Standard Electric Model S-1 precision-timer.

(5) Procedure. At the beginning of each session, the electrocutaneous threshold was determined for each *O*. A stimulus-intensity of 150% of the threshold current midway between the *RL* and the tolerance-threshold was then used.[5] For the *A/TK* sessions, the *O*s were asked to equate the subjective intensity of the auditory signal to that of the electrocutaneous stimuli. This procedure avoided differential treatment of the *O*s in the preliminary portion of the experimental session.

Instructions for making the judgments were read to *O*. Individual operating under the *A/TK or E/TK* conditions were instructed to turn on the signal for the stated intervals by depressing the response-key. The *O*s in the *TK* condition were simply instructed to depress the response-key. Each trial was signaled by *E*'s announcement of "ready." A delay of 15 sec. separated the successive trials. The *O*s were asked not to count or tap in making their judgments and time-pieces were removed at the start of each session. *O* was not told about the accuracy of his performance.

Results. For each *O*, median judgments were calculated for each block of trials. Means of median judgments obtained at each duration are plotted in Fig. 1 as a function of modality and trials. The figure shows that the intervals produced in response to the 0.5 and 3.0 sec. instruction generally tended to fall below the standard while at 7.0 and 15.0 sec. the intervals produced tended to approach or lie above the standard. Fig. 1 further reveals that there was an increase in the interval over trials more pronounced at 7.0 and 15.0 sec. than at 3.0 sec. At 0.5 sec., the interval decreased over trials. For all intervals, there does not appear to be any consistent tendency for the absolute magnitude of response to vary systematically with differences in the sensory cues filling the interval.

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[5] G. R. Hawkes and J. S. Warm, The sensory range of electrical stimulation of the skin, this JOURNAL, 73, 1960, 485-487. The mean value used in the present study was 0.79 ma.

Data for each duration were treated by analysis of variance. No significant effect for modality and no significant modality x interaction of trials were obtained at any of the four intervals. Additionally, real differences in the magnitude of the responses were not observed at 0.5 and 3.0 sec. Significant differences between trials were found, however, at 7.0 and at 15.0 sec. ($p < 0.05$). The over-all trend of the trial means for judgments of 7 sec. revealed both a significant linear component ($p < 0.01$) and a significant quadratic component ($p < 0.05$). At 15.0 sec., the over-all trend of the trial means was linear ($p < 0.05$), and there was no significant curvature.

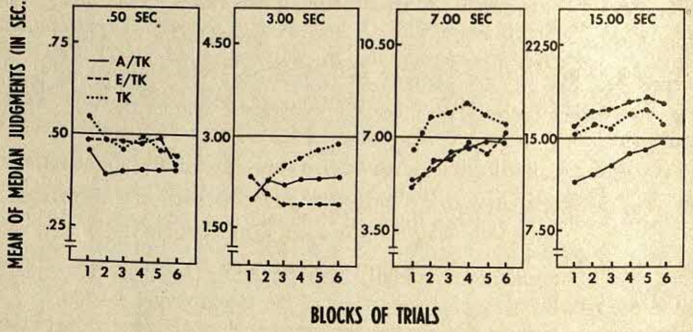

FIG. 1. MEANS OF MEDIAN PRODUCTION JUDGMENTS (IN SEC.)

It has been suggested that observed lengthening of temporal judgments with successive repetitions might reflect overestimation of elapsed time due to the boredom involved in the performance of a repetitive task.[6] Clearly, the drift toward underestimation (lengthening of produced intervals) in the present study does not accord with such an explanation. Further, the general tendency found here for response magnitude to increase beyond the standard at 15.0 sec. is consistent with the findings of Falk and Bindra and Robinson and supports the former's contention that the 'serial position gradient' does not represent improvement in judgment accuracy with practice.[7] Woodrow has suggested that changes in the direction of time-order errors in temporal discriminations may be attributed to the effects of the stimulus-range employed upon the *O*'s frame of reference.[8] It is unlikely that such a phenomenon was operative in the present study, however, since *O*s were exposed to only one duration level in any condition. All in all, it appears that an explanatory framework that could adequately account for the 'serial position gradient' remains to be identified.

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[6] H. J. Jerison and Jules Arginteanu, Time judgments, acoustic noise, and judgment drift, *WADC tech. Rep.*, 57-459, 1958, 20-23; Warm, Morris, and Kew, *op. cit.*, 295.

[7] Falk and Bindra, *op. cit.*, 280-281; Robinson, *op. cit.*, 357.

SUMMARY

The present study assessed the effects of variations in sensory content and nominal durations of intervals delimited by *Ss* on the tendency for these intervals to increase in magnitude over trials. The intervals to be produced were 0.5 sec., 3.0 sec., 7.0 sec., and 15.0 sec. The pressing of the response-key resulted in the following sensory inputs: (a) auditory and tactual-kinesthetic, (b) electrocutaneous and tactual-kinesthetic, (c) tactual-kinesthetic alone. A significant linear increase over trials was noted at 15.0 sec. and an increase with significant linear and quadratic components was observed at 7.0 sec.; at 0.5 and 3.0 sec., no significant trend over trials was apparent. The shape and slope constants of the gradient as well as absolute response magnitude showed no relation to the sensory stimulation employed.

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[8] Herbert Woodrow, The effect of practice upon time-order errors in the comparison of temporal intervals, *Psychol. Rev.*, 42, 1935, 127-152.

MONOCULAR AND BINOCULAR PERCEPTION OF VERTICALITY AND THE RELATIONSHIP OF OCULAR DOMINANCE

By CARL W. SCHNEIDER, Michigan State University

Certain organismic states may be altered through experimental manipulation of such factors as body-tilt (supported or unsupported), sidewise head-tilt, body-rotation, or electrical or mechanical stimulation of the neck muscles.[1]

It has been suggested that these manipulations, which induce concurrent asymmetrical responses in the proprioceptive, tactile, and vestibular feedback mechanisms, contribute to error in the perception of verticality,[2] and it is with this induced asymmetry that most investigators have concerned themselves. Little consideration has been given to an analysis of the possible relationship which may exist between natural bilateral asymmetry and the perception of verticality.

Walls has described in great detail the nature of bilateral asymmetry in the visual system.[3] He clearly distinguishes between two types of ocular dominance; rivalry-dominance, which is purely sensory, and sighting dominance, which is motor in character. The relationship between visual asymmetry and the perception of verticality has yet to be described.

The present investigation represents an attempt to describe the relationship between the left-eye, right-eye, and binocular perception of the vertical, and the possible involvement of ocular dominance.

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* Received for publication May 4, 1966. This study was supported by a Public Health Service fellowship 5-F1-MH-17, 006-02 from the National Institute of Mental Health.

[1] Heinz Werner, Seymour Wapner, and K. A. Chandler, Experiments on sensory-tonic field theory of perception: I. Effect of extraneous stimulation on the visual perception of verticality, *J. exp. Psychol.*, 42, 1951, 341-345; II. Effects of supported and unsupported tilt of the body on the visual perception of verticality, *idem,* 346-350; Seymour Wapner, Heinz Werner, and R. B. Morant, III. Effect of body rotation on the visual perception of verticality, *idem,* 351-357; Heinz Werner and Seymour Wapner, IV. Effect of initial position of a rod on apparent verticality, *ibid.,* 43, 1953, 68-74; Seymour Wapner and Heinz Werner, V. Effect of body status on the kinesthetic perception of verticality, *ibid.*, 44, 1952, 126-131; C. W. Schneider and S. H. Bartley, A study of the effects of mechanically induced tension of the neck muscles on the perception of verticality, *J. Psychol.,* 54, 1962, 245-248; Martin Bauermeister, Effect of body tilt on apparent verticality, apparent body position, and their relation, *J. exp. Psychol.*, 67, 1964, 142-147.

[2] Werner and Wapner, Sensory-tonic field theory of perception, *J. Pers.,* 18, 1949, 88-107.

[3] G. L. Walls, A theory of ocular dominance, *Arch. Ophthal.,* 45, 1951, 387-412.

Observers. Twenty-four men from an experimental course in vision served as *O*s in this investigation. Twelve of the *O*s were left-eye dominant and 12 were right-eye dominant as determined by a sighting test prior to the experiment. All had served as *O*s in perceptual experiments prior to this one, but none had prior experience in a study of verticality.

Apparatus. The visual target, which was placed 13 ft. from *O*, was a luminescent rod 1 in. wide and 24 in. long, and it revolved around its midpoint in the frontal plane. A metal shaft 3/8 in. in diameter extended horizontally from the midpoint of the rod through a 2-in. thick backboard, which was 36 in. square, to a gear arrangement and synchronous motor which could drive the rod in either a clockwise or counterclockwise direction. A simple circuit made it possible for *E* to adjust the rod freely to some initial position and then activate the motor drive by pressing a button. The rod then moved at a constant rate (2°/sec.) toward the vertical position until *O* pushed a button which activated a solenoid brake while simultaneously deactivating the motor drive. This system provided a precise method of stopping and starting the movement of the rod. All rod settings could be read to an accuracy of less than 1/4° on a protractor with a 12-in. radius. The entire apparatus was bolted to a heavy table which placed the midpoint of the rod at eye-level for *O* who was seated during the experiment. The study was conducted in a dark room in which all visual cues to the vertical had been eliminated.

Procedure. The data were obtained from each *O* in three 45-min. sessions which were separated by three-day intervals. At the beginning of the first session *O* was given a test to determine eye-dominance. The test consisted of sighting the rod under binocular conditions through a 3/8-in. hole in the center of a 4 × 8 in. card held at arm's length with both hands. *O* then closes one eye or the other; if he is left-eyed, the rod will remain in sight when viewing with the left eye and will disappear when viewing with the right eye. If he is right-eyed, the situation will be reversed. Every *O* received the test again at the end of the study.

Three viewing conditions were used in the experiment: right-eye, left-eye, and binocular. When *O* viewed the target with one eye, the other eye was covered with an eye-patch. In any particular session *O* made either all binocular observations, or half with the left eye followed by half with the right eye, or half with the right eye followed by half with the left eye. The order in which these three conditions followed each other in the three sessions was randomized for each *O*.

The six initial positions of the rod were ±5°, ±30°, ±45°. *O* made four observations under each viewing condition and initial position of the rod. The order of initial positions of the rod was randomized for every *O* and each session. During the sessions, *O* was instructed to keep his eyes closed while *E* placed the rod in the initial position. After placement of the rod in position, *E* gave a verbal signal followed approximately 5 sec. later by the activation of movement, which *O* stopped by pressing a push-button switch when he thought the rod was vertical.

Measures. The measures were degrees of deviation of *O*'s setting from true (plumb line) vertical. Clockwise deviations from true vertical were designated positive (+) and counterclockwise deviations negative (−).

Results. Fig. 1 shows the results obtained from the right-eye and left-eye dominant groups. Under monocular conditions, the mean error for the right eye is clockwise from the true vertical and for the left eye it is counterclockwise. Under binocular viewing conditions, the mean error is clockwise from the vertical for the group with right-eye dominant, and counterclockwise for the group with left-eye dominant.

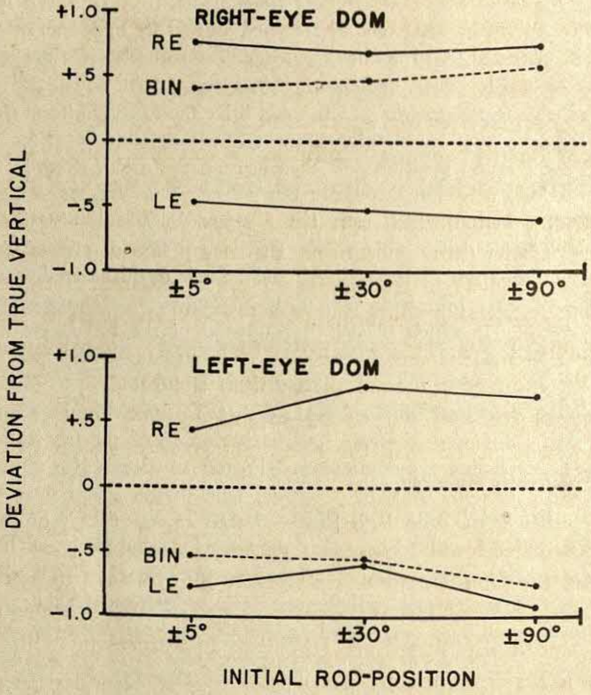

FIG. 1. DEGREES OF DEVIATION FROM TRUE VERTICAL
RE, right eye; LE, left eye; BIN, binocular vision; +, clockwise from true vertical; —, counterclockwise from true vertical.

T-tests of the differences in mean error obtained by the right- and left-eye dominant groups were performed and the following was found.

(1) Right-eye dominant group—right-eye vs. left-eye differences were significant ($p < 0.005$) for all initial positions of the rod.

(2) Left-eye dominant group—right-eye vs. left-eye differences were significant ($p < 0.005$) for all initial positions.

(3) Binocular viewing—left-eye dominant vs. the right-eye dominant: The differences were significant ($p < 0.005$) for all initial positions.

(4) Comparison of the differences between right-eye and binocular

viewing for the right-eye dominant group and the left-eye and binocular viewing for the left-eye dominant group proved to be non-significant.

Discussion. The findings of the present study suggest the existence of a well-defined relationship between the perception of verticality, in the absence of systematic extraneous stimulation, and ocular dominance. For the right-eye dominant group, the apparent vertical is displaced to the right of true vertical, while the apparent vertical for the left-eye dominant group is displaced to the left of true vertical.

Under monocular viewing conditions, the displacement of the vertical is dependent on the viewing condition. When observing with the right eye, the apparent vertical is displaced clockwise from true vertical, and when observing with the left eye, the apparent vertical is displaced counterclockwise. Under these conditions, the magnitude of the error does not tend to be smaller when the dominant eye is utilized. On the contrary, under certain initial rod positions (see Fig. 1), the error is smaller with the non-dominant eye. This finding is particularly interesting in light of the fact that most people tend to use their dominant eye when required to sight an object, and we would expect a greater degree of accuracy than might be achieved with the non-dominant eye. In the present study, however, this is not the case.

The magnitude of the error for the right-eye dominant group tends to be slightly smaller for the binocular viewing than when only the right eye is used, and the same is true when the left-eye error is compared with the binocular error of the left-eye dominant group. While these differences were not statistically significant, they are in agreement with the recent findings that the error in the perception of verticality is greater for monocular than binocular viewing.[4]

Werner and Wapner have concluded that in the absence of extraneous stimulation and in an erect position "true vertical is seen by most people as tilted somewhat to the right."[5] In subsequent investigations, there have been many instances in which the true vertical was seen as tilted to the left.[6] On the basis of the findings of this study, it may be concluded, however, that Werner and Wapner are correct if we assume that the majority of people are right-eye dominant.

In the preceding discussion, certain relationships have been defined, and no attempt has been made to identify causative factors. Since the elements involved in visual asymmetry and bilateral asymmetrical distribution are not well understood, it would be premature at this point to suggest that the perception of verticality is influenced primarily by ocular dominance. It may well be that there are other factors, yet to be identified, which are related to ocular dominance and play a more important role in determining the perceptual end result.

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[4] Peng Tan-ling, The influence of the direction of convergence on judgment of visual vertical, *Acta psychol. Sinica,* 2, 1964, 152-160.

[5] Werner and Wapner, *op. cit., J. Pers.,* 18, 1949, 100.

[6] J. H. McFarland, Seymour Wapner, and Heinz Werner, Relation between perceived location of objects and perceived location of one's own body, *Percept. mot. Skills,* 15, 1962, 331-341.

SUMMARY

The perception of verticality was determined for 12 right-eyed and 12 left-eyed *O*s using the right eye, the left eye, and binocular vision. The apparent vertical was displaced to the right of true vertical when the right eye was used and to the left of true vertical when the left eye was used by all *O*s. Under the binocular viewing condition, the apparent vertical was displaced clockwise of true vertical by the right-eye dominant *O*s and counterclockwise of true vertical by the left-eye dominant *O*s.

APPARATUS

A PSYCHOPHYSIOLOGIC STIMULATOR: FLOATING CONSTANT-POWER

By Roy J. Krusberg and Herbert Zimmer, University of Georgia

Various investigators have on repeated occasions expressed concern over the possibility that wattage, rather than voltage or amperage traditionally used, may be the current-characteristic most relevant to electrodermal stimulation.[1] Definitive experimentation on this point has been hampered by the unavailability of a constant-power stimulator. Instead, the wattage dissipated through *S* has had to be calculated from the measurements of *S*'s resistance and amperage or voltage. The instrument described here is intended to remedy these deficiencies and to arm the experimenter with means of establishing positive control over these independent variables.

Until recently a simple, reliable circuit capable of driving a load of varying-impedance with constant power has been virtually impossible to design, since no suitable exponential device has been available. Previous efforts to construct a constant-power instrument around a nonlinear functional generator were abandoned after it was found necessary to employ an excessive number of operational amplifiers to achieve the desired stability and accuracy. The development of the Hall effect through thin-film devices has yielded a solution of this problem. The constant-power stimulator described in this paper owes its effectiveness to the Hall effect used as a voltage-current multiplier. In 1879 E. H. Hall made detailed observations regarding the effects of perpendicular magnetic fields on the bending of electrical field-lines within very thin sheets of various metals.[2] More than 70 yr. elapsed before advances in the manufacture of semiconductors enabled industry to produce the Hall effect devices of wide utility outside the physics laboratory.

The identical considerations which governed the design of the constant-current and constant-voltage stimulators described by Krusberg and Zimmer provided the

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* This study was conducted in the Bioelectronic Computer Laboratory, and supported by the Air Force Office of Scientific Research, Grant AF-AFOSR-257-65, and the Rome Air Development Center, Contract AF 30(602)-3380.

[1] H. E. Hill, H. G. Flanary, C. H. Kornetsky, and A. Abraham Wikler, Relationship of electrically induced pain to the amperage and the wattage of shock stimuli, *J. clin. Invest.*, 31, 1952, 464-472; R. T. Green, The absolute threshold of electric shock, *Brit. J. Psychol.*, 53, 1962, 107-116; Bernard Tursky and P. D. Watson, Controlled physical and subjective intensities of electric shock, *Psychophysiol.*, 1, 1964, 151-162.

[2] E. H. Hall, On a new action of the magnet on electric currents, *Phil. Mag.*, 9, 1880, 225-230; On the new action of magnetism on a permanent electric current, *ibid.*, 10, 1880, 301-328.

guidelines for the present instrument.[3] The output of this stimulator is isolated from ground: (1) to avoid the introduction of ground-loops or noise which might interfere with psychophysiologic recording; and (2) to be safe for use with human *Ss*. In addition, the stimulator was designed: (3) to cover the resistance range usually encountered with human *Ss;* (4) to produce acceptable power regulation within this range; (5) to yield direct readings of power output to the *S;* (6) to be controlled by an external pulse or time-interval generator; and (7) produce an output of square-wave power-pulses of an upper frequency determined by the response-characteristics of the control-circuitry. The stimulator-circuit is completely isolated from ground- and power-lines by using isolation-transformers and mounting all components on phenolic terminal boards. *S* is further protected by two 1/100 amp. fuses in series with the output jacks.

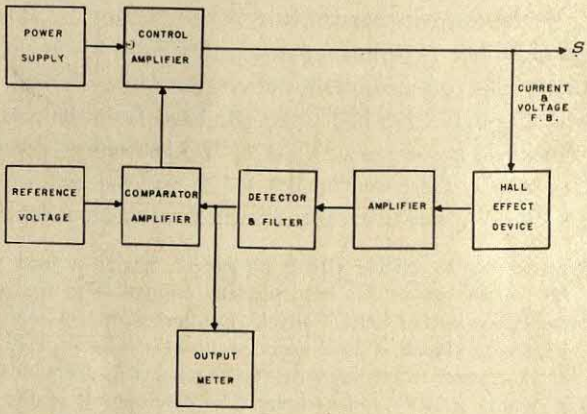

FIG. 1. INTERRELATION OF FUNCTIONAL UNITS OF CONSTANT-POWER STIMULATOR

The instrument is capable of regulating power output within 1% from 10–500 mw. output into load resistances varying from 13,000–100,000 Ω and will hold to within 2% down to 600 Ω from 10–500 mw. The maximal power-output capability of the instrument is 500 mw., bounded by current limiting at 5 m.amp., and voltage limiting at 200 v., in the present circuit design. These parameters can be readily modified, whenever this might become desirable. The precision of the prototype was limited by the deliberate use of inexpensive components, with a total cost of about $130. This figure is predicated on one's willingness to assemble the Hall generator oneself, as was done in this instance. By doubling the cost, a more accurate version of this instrument can achieve the regulation of the power output within 2% or better over its entire range of operation. The precision of the constant-power stimulator, as given above, is achieved by substituting (a) a Scientific Columbus M250 Hall multiplier for the laboratory assembled unit, (b) an operational amplifier, similar to Nexus SQ-10, in place of the prototype two-stage triode voltage-amplifier, and (c) a Microtran M8025 chopper-transformer, or possibly a transistor chopper input, in place of the present transistor-transformer.

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[3] R. J. Krusberg and Herbert Zimmer, Floating constant-current and constant-voltage squarewave physiologic stimulators, this JOURNAL, 78, 1965, 124-129.

The block-diagram of Fig. 1 provides an overview of the prototype model of the constant-power circuit. A series-control amplifier regulates current flow to *S*. A fraction of the voltage across *S*, plus the entire current flow through *S*, are multiplied by the Hall device. The output voltage of the Hall generator is chopped and amplified to an appropriate level. The amplified output is rectified, filtered, and fed to a comparator amplifier. Since the output of the detector is proportional to the power across *S*, its output is shown on the power output indicating meter, as its equivalent in mw. A reference voltage is supplied to the comparator amplifier, and its output is coupled to the grid of the control amplifier.

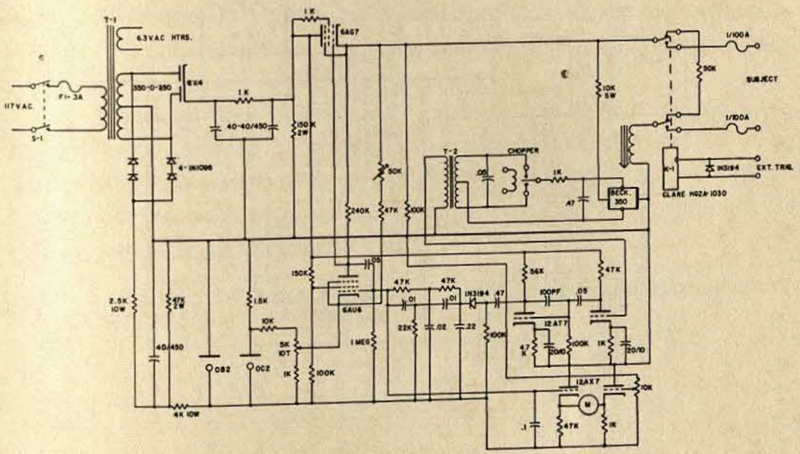

FIG. 2. CIRCUIT-DIAGRAM OF CONSTANT-POWER STIMULATOR

A schematic diagram of the regulator-circuit appears in Fig. 2. The Hall multiplier consists of a Beckman model 350 thin-film Hall generator, in a Beckman 34-138-1 core, with two coils containing 5,000 turns each of number 40 wire, mounted on the core. The entire assembly is potted in a triple mu-metal can with Hysol R8-2038. The voltage input leads of the Hall generator are connected through a 10,000 Ω resistor to the output voltage terminals of the control amplifier circuit. The series-connected current coils are lined directly to *S*. The Hall voltage is proportional to the product of the current flow through the device and the magnetic flux at right angles to the current flow. The output of the Hall generator is thus a function of the product of voltage across *S* and the current through

S, and can be expressed as $V_H = f(kV_sI_s)$. Since by Ohm's and Kirchoff's law $P = VI$, and $V_H = kP$, the Hall voltage can be seen to be a function of the power dissipated by S.

The Hall voltage, a D.C. voltage, is converted to A.C. by an Airpax M-4 chopper feeding a Stancor TA-31 transformer. The 400 ~ power for the chopper drive is supplied by a small transistor multivibrator, using the chopper coil in a resonant load circuit, as shown in Fig. 3. The A.C. voltage is amplified by a two-stage 12AT7 triode resistance-coupled amplifier, the output of which is converted to a high-level D.C. signal by a half-wave diode rectifier. The detector-stage serves the additional function of shifting the level of the D.C. signal to that required by the comparator amplifier. A twin-tee rejection-filter on the output of the detector removed most of remaining 400 ~ component of the detector output. A 400 ~ chopper frequency was chosen: (a) to give improved frequency response; (b) to eliminate any power line frequency components from the output stage; and (c) to simplify amplifier and detector design requirements.

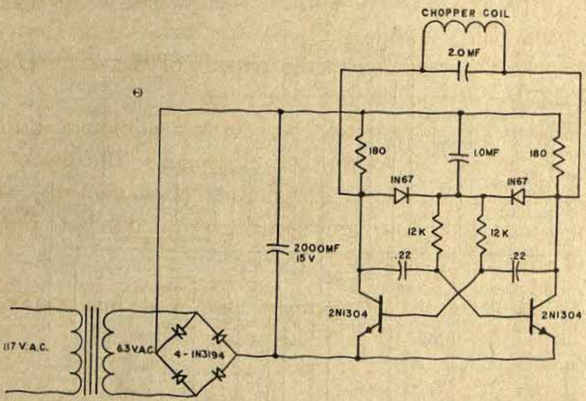

FIG. 3. 400~ CHOPPER-OSCILLATOR

The D.C. output of the detector appears on the grid of a 6AU6 sharp cut-off pentode comparator amplifier. The reference voltage, which determines the power output, is applied to the cathode of the 6AU6 amplifier. The plate of the 6AU6 amplifier is (a) fed from the output of the control amplifier through a plate load resistor, and (b) is also connected directly to the grid of the control amplifier tube. The control amplifier is a 6550 high transconductance power amplifier pentode, ideally suited for the high gain requirements of this application.

In operation, the difference between the cathode and grid voltages in the 6AU6 determines its plate current flow, and thereby the control grid voltage of the 6550. For any given load impedance, a particular setting of the 6AU6 cathode voltage will permit voltage to increase across *S* until the detector output voltage brings the 6AU6 plate voltage and 6AU7 grid voltage to a level of equilibrium. A decrease in load resistance will cause a lower grid voltage at the 6AU6 comparator amplifier, causing its plate current to decrease and to produce a corresponding decrease on the negative grid bias of the 6550 control amplifier. Current output from the 6550 will then increase until an equilibrium is once again established. The converse effect is created by an increase in load resistance. The power output of the control amplifier is sensed continuously, and corrections applied at the response rate of the system. This prototype instrument requires additional feedback, in the form of a fraction of the output voltage fed to the cathode of the comparator section, to produce the desired linearity in power output.

It is mandatory that polarization be held to a minimum during experimentation by the use of appropriate electrodes.[4] The extent to which polarization occurs and distorts experimental measurements, despite this precaution, can be determined through the use of an electrometer before, during, and after the experiment.

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[4] D. T. Lykken, Properties of electrodes used in electrodermal measurement, *J. comp. physiol. Psychol.*, 52, 1959, 629-634.

AN APPARATUS FOR STUDYING VISUAL REINFORCEMENT IN FISH

By THOMAS STURM, JOHN GRABOWSKI, and TRAVIS THOMPSON,
University of Minnesota

Several devices for studying operant conditioning in fish have been reported.[1] These methods have either employed photoelectric cells or lever-deflection transducers to detect the responses of the fish. Several problems are encountered when these devices are used with small fish.

The two primary difficulties of lever-deflection devices are the relatively large force required to detect the response, and the pretraining required to maintain an adequate response. Although Longo and Bitterman state that their lever-deflection apparatus can be adjusted to any desired sensitivity, experiments by the authors of this paper revealed that when the lever is sufficiently sensitive to be triggered by the response of relatively small fish (2-3 in. long *Bettas*), small disturbance in the water also operates the device.[2] The difficulty in training fish to perform the required lever deflecting response is discussed in the apparatus paper, some experimenters were not able to train a single fish to make the required response.

Photocell devices are easily operated by small fish, but present other difficulties. For example, an apparatus described by Thompson for studying visual reinforcement in Siamese Fighting Fish did not differentiate the animal's orientation as it swam through a response ring suspended in the aquarium.[3] Controlling this variable is particularly important when a moving visual reinforcer is used. This problem has been evercome by requiring that the light falling on three widely spaced photocells is interrupted in a specified order.[4] Unfortunately, this response sequence requires a long time for conditioning, and does not facilitate immediate recurrence of the response.

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[*] This research was supported in part by research grant GB-4518 from the National Science Foundation.

[1] W. A. Deterline, An automatic device for the investigation of operant behavior in fish, this JOURNAL, 69, 1956, 291-295; Nicholas Longo and M. E. Bitterman, Improved apparatus for the study of learning in fish, this JOURNAL, 72, 1959, 616-620; T. I. Thompson, Visual reinforcement in Siamese fighting fish, *Science,* 141, 1963, 85-87; T. I. Thompson and Thomas Sturm, Visual-reinforcer color and operant behavior in Siamese fighting fish, *J. exp. Anal. Behav.,* 5, 1965, 341-344.

[2] Longo and Bitterman, *op. cit.,* 617 f.

[3] Thompson, *op. cit., Science,* 1963, 86 f.

[4] Thompson and Sturm, *op. cit., J. exp. Anal. Behav.,* 340 v. 1965, 340 f.

The apparatus described in this paper was designed to overcome the difficulties of the lever operandum and the three photocell response-system. It was specifically designed for the operant conditioning of *Betta splendens* using visual reinforcement. It could, however, be used with any small fish to deliver other reinforcers. The system requires a simple swimming response similar to that described in the paper by Thompson,[5] but still insures that the fish will be oriented in a predetermined direction when the reinforcer is presented. After the response has been made, a model of a male *Betta splendens* is presented, exhibiting two components of the complex aggressive behavior sequence characteristically elicited by the visual image of another male Betta. The model approaches the fish frontally until it reaches a distance of approximately 2 in. from the fish and then stops for 4 sec. (frontal approach). After the 4-sec. delay, the model turns and moves laterally across the front of the tank (lateral display) and then disappears behind an opaque screen. Six seconds are required for the model to complete this lateral display. The fish is able to view the model for a total time of 11 sec., including both frontal and lateral display.

The apparatus may be divided into two parts: one part for the detection of the responses; and the second part for the reinforcement of the presentations. The detector of the responses of the photo-conductor is suspended at one end of a standard 4¼ × 4¾ × 9¼ in. 2-gal. glass aquarium. The illumination inside the aquarium is primarily provided by two external sources: a 15-w. fluorescent bulb extending the entire length along the back wall; and an incandescent light fixture (two 15-w bulbs) suspended 7 in. above the tank. The entire top of the tank has a loose-fitting lucite cover which supports the system detecting the responses.

The response-system consists of three Clairex CL5M3 photo-conductors cemented in a rectangular opaque block, 1 × 2 × 4 in. The photo-conductors are arranged in a row along a 1 × 2 in. top of the lucite block (Fig. 1 B). This forms the top of a response chamber 2 in. long, ¾ in. wide, and 7 in. deep, framed by clear, sheet-lucite (Fig. 1 C). An incandescent lamp situated below the aquarium allows light to travel through the lower portion of the response chamber, illuminating the three photo-conductors.

Each of the three CL5M3 photo-conductors is connected in series with a Dunco Reed Relay RR 1A (1440-ohm coil), to the 28-v. power supply used for circuitry of the relay-timer. The relay is normally closed with light on the photo-conductors and opens when the light beam is interrupted. This beam-breaking photo-relay system has the advantage of requiring no amplification.

A response is defined as the successive interruption of the light falling on the three photo-conductors in the following sequence: first, the fish interrupts the light falling on the photo-conductor nearest the back wall; secondly, interruption of the light falling on the middle photo-conductor; and finally, interruption of the light falling on the photo-conductor nearest the front wall of the tank. This sequence is followed by the presentation of the visual reinforcer. Interrupting the light beams in any other order will not lead to reinforcement, as arranged by external relay logic. This assures that *S* will be oriented toward the front wall after responding and hence the area in which the presentation of the reinforcer (Fig.1 E) occurs.

The visual reinforcer of the presentation system consists of a 40 r.p.m., 115-v. A.C.

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[5] Thompson, *op. cit., Science,* 1963, 85-87.

Dayton Motor D4552-F; 800-27; 70B (Fig. 1 K) which drives a ladder-type sprocket-chain (Fig. 1 I) directly from the drive gear (Boston Sprocket Gear CBA20B). The chain is supported on a pentagonal pathway by four additional sprocket gears

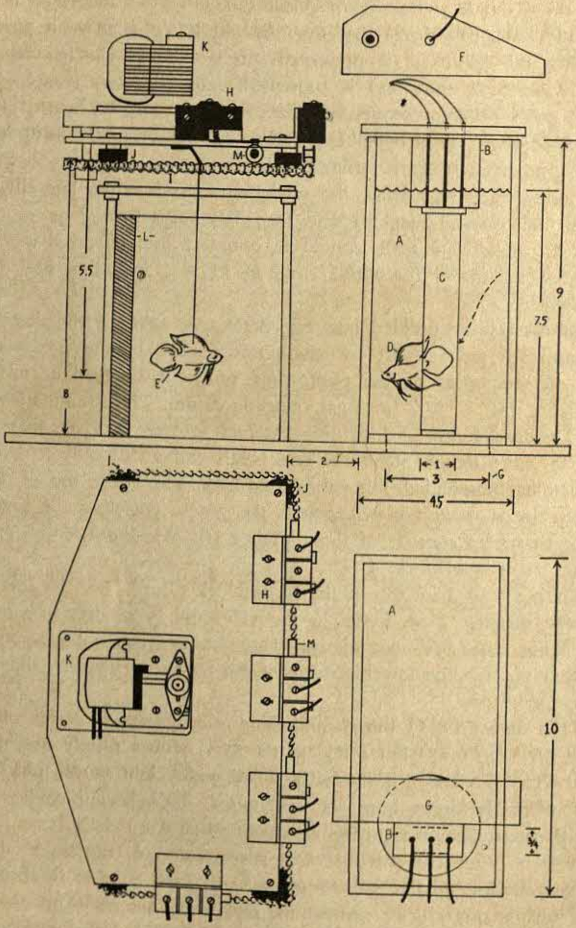

FIG. 1. APPARATUS FOR STUDYING VISUAL REINFORCEMENT IN FIGHTING FISH (ALL DIMENSIONS IN INCHES.)

A. Aquarium
B. Opaque lucite block
C. Response chamber
D. (male *Betta*)
E. Visual reinforcing model
F. Overhead lamp
G. Lucite strip attaching response-detector to aquarium lid
H. Microswitch
I. Sprocket chain
J. Gear
K. Motor
L. Opaque Screen
M. Microswitch roller

(Fig. 1 J) located at the corners of a heavy aluminum frame. The models of the fish are suspended by stiff wires soldered to the sprocket-chain. Four microswitches (Fig. 1 H) are situated above the chain, supported by L-brackets attached to the aluminum frame. The microswitch rollers (Fig. 1 M) are tripped by small stiff wire ramps which are soldered to the sprocket chain immediately in front of the models. Through the use of external logic and timers, it is possible to use microswitch closures to interrupt the power activating the drive motor, thereby stopping the model for a predetermined interval in frontal or lateral display.

Two models are soldered to the sprocket chain opposite each other to make it possible to present the models with a minimal delay between occurrence of the response and presentation of the model. While one model is being presented, the other is traversing the rear of the chain carrier behind an opaque screen (L), stopping in position to be presented, as soon as the first model has disappeared from the end of the tank.

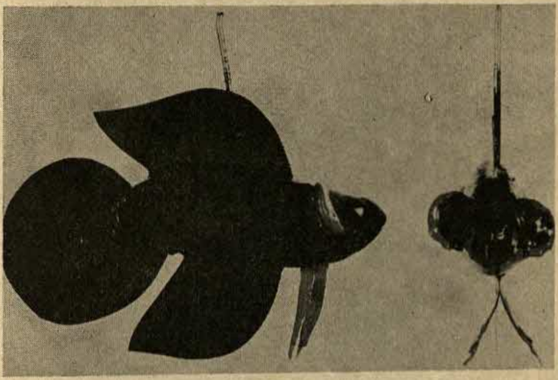

FIG. 2. LATERAL AND FRONTAL VIEWS OF A PLASTER OF PARIS MODEL OF A MALE *Betta* IN AGGRESSIVE DISPLAY

The model is 1¼ in. from the top of the dorsal fin to the bottom of the ventral fin and 2 in. long. This model is blue with black eyes and red gills.

The bodies of the models are plaster of paris casts of a male fish used for making a rubber mold. Fins were copied from a photograph of the fish in the maximal degree of fin erection and then cut from 0.002-in. thick Shim-stock sheet steel. The halves of plaster of paris molds were then glued together enclosing the sheet steel fins, and a small piece of No. 10 solid wire was soldered to the dorsal fin for attachment to the carrier. Then the entire model was painted a solid color. The eyes were then painted black, and the gills (which are exposed because the opercula are erected) red (Fig. 2).

The timer-switching circuitry controlling reinforcement contingencies involves the microswitches situated above the ladder chain, external clocks and external relays. Standardized commercially available 28-v. D.C. relay-timer equipment has been used in our laboratory for this purpose.

The model can be presented in frontal display, lateral display, frontal followed by lateral display, with or without pauses of specified duration in either or both display positions thus permitting the analysis of a variety of aspects of the display behavior.

Fig. 3 presents a cumulative record of the acquisition of this operant reinforced by a model of a male *Betta splendens* for one subject. In this case the model stopped for 4 sec. in frontal display, but did not stop in lateral display. The curves are generally similar to those obtained with other reinforcers on a continuous reinforcement schedule.[6] It shows somewhat more grain, as is characteristic of relatively weak reinforcers, or low deprivation conditions.

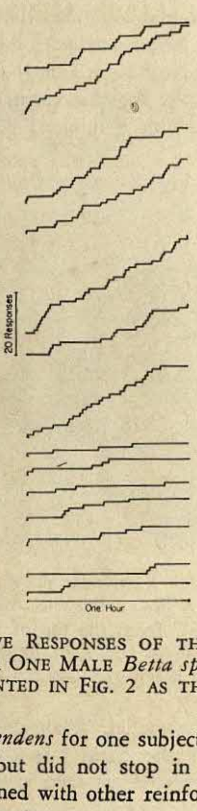

FIG. 3. CUMULATIVE RESPONSES OF THE FIRST FOURTEEN 1-HR. SESSIONS FOR ONE MALE *Betta splendens,* USING THE MODEL PRESENTED IN FIG. 2 AS THE REINFORCER

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[6] C. D. Ferster and B. F. Skinner, *Schedules of Reinforcement,* 1957.

APPARATUS NOTES

A HOMEMADE STEREOTAXIS

To round out the offerings of the psychological laboratory, and in particular to demonstrate the effects of lesions and of central stimulation, we have felt the need of a stereotaxis. Commercial models require capital outlays which may preclude their purchase by small departments. To meet this problem, the authors assembled a workable, if inelegant instrument, from a drill press, milling table, machinist's calipers, scrap aluminum, and plexiglass.

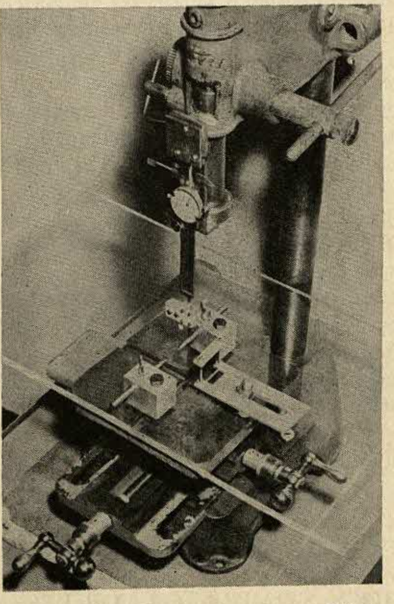

FIG. 1. THE STEREOTAXIS WITH ELECTRODE IN HOLDER

Fig. 1 shows the finished product. A plexiglass surgical table (3/8 in. thick, 11 in. wide, 22 in. long) is bolted to the base of the milling table. Two pieces of aluminum stock (1¼ in. wide, 1¾ in. long, and 1⅜ in. thick), with threaded wing nuts, are attached to the table to carry the ear-bars. The ear bars were fashioned from ¼ in. steel rod with a 45° angle filed on one end. The nasal clamp is two pieces of aluminum (⅛-in. thick and 1¾-in. wide). The top piece is ½-in. long and the bottom one is 1½ in. in length. The bolt and nut arrangement enable the clamp to adjust the angle of the head and keep *S* rigidly chucked up. *S*'s upper incisors hook over the slot filed in the end of the clamp. The inner surface of the top piece is rounded to form fit over the nasal bones. The electrode is carried between two pieces of plexiglass which are fastened to the depth-gauge of machinist's dial calipers. The calipers are bolted to an aluminum bracket which replaces the chuck in the drill press. Extension of the depth-gauge is used for implantation. A ruling pen may be fastened to the depth-gauge for a simple electrode carrier instead of the plexiglass.

S is placed in the instrument and the angle of the skull adjusted. The table can be unbolted from the drill press frame for easy access during surgery or the drill press can be swung to one side. In actual practice, it is not necessary to do either as there is ample room for surgery with the drill press in the implanting position. The latter procedure eliminates possible error in returning to the predetermined zero on the ear-bar. Anterior-posterior adjustments are made with the control knob on the right side of the table and the crank at the front controls lateral movement. The dial micrometer, anterior-posterior and lateral verniers, are in thousandths of an inch which give excellent control. A table converting inches to millimeters is kept taped to the base to eliminate computation during surgery.

This device does not pretend to be a substitute for a fine instrument. It can, however, be satisfactorily used where no stereotaxia is available or to teach laboratory procedure to the neophyte and thereby save wear and tear on the research instrument. The quality of work, however, appears to be reasonably good at least for the purposes of laboratory classes. We have been able to place our implants in the desired targets in the hypothalamus within ± 0.5 mm. of the intended location. The technique of angle approaches appears to be beyond the capabilities of this particular device. Any department desiring such a tool should be able to cut costs considerably by substituting enameled or varnished wood for plexiglass and aluminum. We chose the materials and components most readily at hand rather than considering new purchase price. The basic design can easily be adapted, however, by those interested.

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A SHOCK STIMULATOR FOR RESEARCH WITH HUMAN SUBJECTS

Fig. 1 shows two identical constant current stimulators which have repeatedly been used to supply shock in experiments utilizing one or two *S*s. For single *S* experiments, electrodes are attached at either A or B and the amonut of current is read off in milliamperes from meter MADC by throwing SW_2 to positions A or B. Of course, to read peak to peak values the output is passed through an oscilloscope. Shock levels are controlled by turning the shafts on potentiometers R_2 and R_3, respectively. With an inserted socket and a few added connections, the circuit may be used for two-person studies in which *E* controls the administration of shock to one or both *S*s, or the two *S*s interact directly, *i.e.* a response by S_1 shocks S_2, and vice-versa.

The apparatus produces a half-wave rectified (peak to peak) AC 60 cps sine wave.

Shock is so delivered through the 6SK7 tube that very high resistance is in series with *S* thus minimizing the effects of changes in *S*'s resistance.

This circuit has been used safely with over 5,000 human *S*s in both single and two-person experiments. A skin-burn was noted in only one case, an experiment requiring *S* to judge the thresholds of pain while using a Tursky arm-electrode.[1] This, however, was the fault of the Spartan like attitude of *S*, a woman. Both finger and arm electrodes have been used successfully.

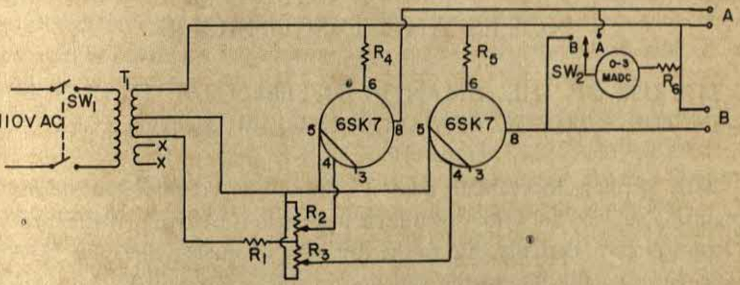

FIG. 1. CIRCUIT-DIAGRAM OF APPARATUS FOR SHOCKS IN HUMAN RESEARCH CIRCUIT CAPABLE OF 0-33 MA. PEAK TO PEAK OUTPUT.

T_1—Thordason 24R12U
R_1—22K 1W
R_2—20K pot
R_3—20K pot
R_4—100 1W
R_5—100K 1W
R_6—33K ½W
SW_1—DPST
SW_2—SPDT

San Diego State College

JOSEPH B. SIDOWSKI

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[1] B. Tursky, P. D. Watson, and D. N. O'Connell. A concentric shock electrode for pain stimulation, *Psychophysiol.* 1, 1965, 296-298.

NOTES AND DISCUSSIONS

THE USE OF THE SEMANTIC DIFFERENTIAL TECHNIQUE IN THE ESTHETIC EVALUATION OF MUSICAL EXCERPTS

Early attempts to evaluate musical composition confined themselves to requiring subjects to write down their impressions of live performances of various pieces.[1] Partially due to the difficulty of quantifying such impressions and partially due to the variation in the performances, only general statements as to the intrinsic powers of music to arouse feelings and emotions (unspecified) in the listener, trained or otherwise, could be made. With the advent of the phonograph, however, at least one of these variables could be controlled.

As with the need for controlled reproduction, it soon became evident that better measuring devices were needed if we were to understand the esthetics and other aspects of music. Hevner's check list,[2] later evaluated by Farnsworth,[3] proved an invaluable tool. A later development in general psychological measurement—the Osgood Semantic Differential technique[4]—has, however, offered still another mathematically more sophisticated method of approaching the perplexing problem of quantifying esthetic evaluation.

To explore the comparability of results obtained by this technique with those of the usual adjective check lists, 74 *S*s (11 men and 63 women) evaluated 10 musical excerpts, using a three-factor (evaluative, potency, and activity) 15-scale semantic differential technique.

The results derived from this form of measurement replicate those obtained with adjective check lists; namely, neither the sex nor the formal musical training of the listener appreciably affects the evaluation of musical excerpts.[5] That this procedure offers similar results to previous methods does not, in itself, justify its use. What does justify its selection over other techniques is that it offers the investigator the opportunity to apply a broader range of statistical analyses to the evaluation of esthetics. Thus, we may bring even more precision to bear on esthetics, and may, with this increased precision, be better able to ferret out the elusive variables of this portion of human behavior.

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[1] B. I. Gilman, Report of an experimental test of musical expressiveness, this JOURNAL, 4, 1892, 558-576; 5, 1893, 42-73; J. E. Downey, A musical experiment, this JOURNAL, 9, 1897, 63-69.

[2] Kate Hevner, Expression in music: A discussion of experimental studies and theories, *Psychol, Rev.*, 42, 1935, 186-204; The affective character of the major and minor modes of music, this JOURNAL, 47, 1935, 103-118; Experimental studies of the elements of expression in music, this JOURNAL, 48, 1936, 246-268.

[3] P. R. Farnsworth, A study of the Hevner adjective list, *J. Aesthet.*, 13, 1954, 97-103.

[4] C. E. Osgood, G. J. Suci, and P. H. Tannenbaum, *The Measurement of Meaning*, 1957.

A detailed analysis of the three factors exemplified the potential of the semantic differential. A significant inverse relationship between the potency and evaluative factors indicates that composers wishing to enhance the perceived esthetic quality of their music should emphasize the 'feminine,' 'soft,' 'light' qualities of their productions. Apparently, since activity and potency also show a significant inverse relationship, one method of achieving this end is by enhancing the rhythmic aspects of the composition.

This suggestion of the potentially singular importance of rhythm for conveying various esthetic qualities appears also in a more detailed analysis of the activity and evaluative factors. This comparison suggests, as Henkin did before,[6] that rhythm is a major, if not *the* major, component in esthetic evaluation. A simple comparison between rhythmic statements and fully orchestrated musical excerpts would offer further refinement of this hypothesis. In addition, the presence or absence of an interactional relationship between the natural, physiological rhythms of the body and those portrayed in musical composition might be investigated. Some of the work on physiological responses has already suggested that certain musical rhythms may evoke predictable changes in ongoing physiological processes.[7] Although our study would seem to indicate a general esthetic quality regardless of the listener's internal rhythms, whether or not the ongoing natural rhythmic (physiological) processes interact with musical rhythm to affect perceived esthetic quality was not directly investigated.

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[5] Hevner, *op. cit.*, this JOURNAL, 47, 1935, 103-118; A. L. Sopchak, Individual differences in responses to different types of music, in relation to sex, mood, and other variables, *Psychol. Monog.*, 69, 1955 (No. 396), 1-20.

[6] R. I. Henkin, A factorial study of the components of music, *J. Psychol.*, 39, 1955, 161-181; A reevaluation of a factorial study of the components of music, *ibid.*, 43, 1957, 301-306.

[7] D. S. Ellis, Effects of music on respiration and heart-rate, this JOURNAL, 65, 1952, 39-47; Henkin, The prediction of behavior response patterns to music, *J. Psychol.*, 44, 1957, 111-127; E. W. Weidenfeller and G. H. Zimny, Effects of music upon *GSR* of depressives and schizophrenics, *J. abnorm. soc. Psychol.*, 64, 1962, 307-312; G. H. Zimny and E. W. Weidenfeller. Effects of music upon *GSR* of children, *Child Developm.*, 33, 1962, 891-896; Effects of music upon *GSR* and heart-rate, this JOURNAL, 76, 1963, 311-314.

Thus, in addition to demonstrating the measurement and heuristic value of the semantic differential technique, the results of the present study appear to point to some sort of intrinsic esthetic component to musical composition, which is discernible by both the sexes. The fact that musical training does not affect esthetic evaluation (at least with the pieces evaluated, if not with the periods of music represented) might indicate that while musical training broadens and sharpens one's ability to detect and perform the technical aspects of music, it neither enhances nor greatly disrupts a natural propensity for the recognition of musical esthetic quality.

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AN ANALYSIS OF DATA GATHERED BY THE STAIRCASE-METHOD

An experimenter E has three options in analyzing data gathered by the staircase-method. First, he may treat the data as gathered by a modified method of constant stimuli and select as the threshold or point of subjective equality (*PSE*) the equivocal stimulus, *i.e.* the stimulus which draws positive and negative responses with equal frequency.[1] Secondly, he may treat the data as gathered by a modified method of limits and take each change in judgment as a crossing of the momentary threshold or *PSE*. These momentary values are averaged over the session. Third, he may take as the threshold or *PSE* the average of the stimuli delivered to *S*.[2] In computing the average, each stimulus is weighted by its frequency of presentation. In all cases, but particularly in the third, the results of initial trials may be discarded to prevent the choice of the initial stimulus from influencing the final measure. In most cases, the three methods of analysis yield like results and *E* may choose whichever method is most consonant with other experimental requirements.

When the staircase-method is used in combination with a *forced-choice* procedure, the three methods of analysis may not agree precisely. The stimulus, or combination of stimuli, to which *S* responds positively and negatively with equal frequency is the stimulus with the highest expected frequency of presentation. The expected distribution of stimulus-presentations is, however, skewed toward lower stimulus-values because the probability of a correct response at low values never goes below chance. Consequently, the average of the distribution may be lower than the value at which positive and negative responses occur with equal frequency. Similarly, the average of the momentary thresholds may also be lower than the latter.

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[1] T. N. Cornsweet, The staircase-method in psychophysics, this JOURNAL, 75, 1962, 485-491.

[2] *Ibid.*, 485.

To evaluate the importance of these discrepancies, *E* needs to know: (1) the typical magnitude of the differences; and (2) whether the differences will affect comparisons among experimental conditions when the same method is applied to all conditions. To provide some information on these points, we conducted an experiment using a *forced-choice* procedure and analyzed the results by all three methods. *S*'s task was to indicate in which of two positions a blurred contour was presented. The degree of blur was varied from one condition to another. We used the forced-choice procedure developed by Heinemann.[3] Each trial consisted of two presentations and the trial was scored as positive if *S* responded correctly on both presentations. Each series consisted of 60 trials, but only the last 40 trials were analyzed. Stimulus-intensity was varied in steps of 0.05 log units. A total of 48 series was conducted: 2 series by each of 2 *S*s under 12 conditions. The data from each series were analyzed by all three methods. In selecting the equivocal stimulus, we discarded percentages based on less than 5 trials. Medians were used as the averages of the momentary thresholds and of the stimulus-distributions. The average difference between thresholds based upon the equivocal stimulus and the median of the stimulus-distribution was nil, but thresholds derived from the momentary thresholds were significantly lower. The median of the momentary thresholds was lower than the equivocal stimulus in 31 of 40 cases of disagreement ($X^2 = 8.9$, $df = 1$, $p < 0.01$) and lower than the median of the stimulus distribution in 34 of 41 cases of disagreement ($X^2 = 12.1$, $df = 1$, $p < 0.01$). The mean difference, however, was only 0.04 log units. Further, the thresholds derived by the three methods were highly intercorrelated: the correlation coefficient was 0.98 for each of the three possible comparisons.

The fact that the analysis of momentary thresholds gave lower thresholds indicates that, in our experiment, *S* tended to make more reversals when doing well than when doing badly. This finding is probably unrelated to the theoretical expectations described above.

We conclude that the three methods of analysis yield substantially the same results. That the expected differences were not demonstrable testifies to the efficiency with which the staircase-method restricts stimulation to a small range on either side of the equivocal stimulus. As Cornsweet has pointed out, this characteristic of the staircase-method is its major advantage.[4]

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[3] E. G. Heinemann, The relation of apparent brightness to the threshold for differences in luminance, *J. exp. Psychol.*, 61, 1961. 392-393.

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THE STAIRCASE-METHOD CRITICALLY EXAMINED

The "staircase-method" evaluated by Feeney, Kaiser, and Thomas in the preceding Note was proposed as a psychophysical method by Cornsweet in 1962.[1] It is, however, not new. Variations of it have long been used in technology for fitting eyeglasses and in testing hearing. It has never before, however, been seriously proposed for use in science. The reasons are not far to seek. They rest upon the nature of the problem and the interests of the investigator attempting the solutions.

Psychophysics is an old science. It is credited as being the first approach to an experimental psychology. Fechner, its father, dates its beginning before Weber to Delezenne in 1826;[2] Lipps, to Lambert and Boager in 1760;[3] and Titchener pushes the date back to Sauver in 1700 and writes that there were "many brave men before Sauver."[4] Throughout the history of psychophysics, the workers in the field were characterized by James as "men who could not be bored,"[5] whose chief interests were 'facts' and the improvement of the methods and techniques of obtaining them. Accuracy, not time, was of the essence. Researches were continued over days, weeks, months, and even years. Errorless techniques were sought that facts would be obtained.

In fitting eyeglasses and determining hearing losses, time is very much a factor. An oculist, for example, cannot devote an afternoon to determining the prescription, to say nothing of a day or week. To maintain his practice, he must be brief. Cornsweet's method is brief. It is a rough and ready adaptation of the method of limits, or minimal changes, as Wundt calls it,[6] in which all the *observational* errors[7]—*constant, variable,* and *accidental*—are ignored or overlooked. The errors exist and they cannot be avoided by closing one's eyes to them or wishing them away. They must be considered and avoided or we shall fall into them.

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[4] Cornsweet, *op. cit.*, 488-489.

[1] T. N. Cornsweet, The staircase-method in psychophysics, this JOURNAL, 75, 1962, 485-491.

[2] G. T. Fechner, *Elements der Psychophysik*, 1859, I, 73.

[3] G. F. Lipps, Die Massmethoden der experimentelles Psychologie, *Arch. ges. Psychol.* 3, 1904, 154-158.

[4] E. B. Titchener, *Experimental Psychology, II. Quantitative Experiments, Part ii, Instructor's Manual*, 1905, 100 ff.

[5] William James, *Principles of Psychology*, I, 1890, 192.

[6] Wilhelm Wundt, *Grundzüge der Physiologischen Psychologie*, 4th, 1893, 336 ff., 341 ff. 6 ed., 1908, 588 ff.

The *constant* errors, time or space, and sometimes both, are present in many experiments. They are controlled or avoided by changing the order in which the standard and variable stimuli are given *S*. Now, the standard comes first and then the variable, and then the experiment is repeated with the variables first and the standard second. Cornsweet makes no provision for these errors. He could easily have done so, but not without greatly lengthening his method.

A careful psychophysicist seeks to remove or to minimize the *variable* errors—*practice, fatigue, expectation* in its two forms, and *habituation*—by the arrangement and presentation of the stimuli. The longer *O* serves, the more practiced and the more fatigued he becomes. Practice and fatigue thus work against each other and fatigue will in part be eliminated by frequent rest-periods. Cornsweet could adopt this procedure, but again not without greatly increasing the length of his method.

The stimulus-series in the classical method alternate between descending and ascending, from 4-12 steps in length, being divided approximately equally among long, medium, and short series, *O* is not informed of the starting point, but is told the direction of the series—*i.e.* descending or ascending. Easy judgments are thus interposed among the difficult, stabilizing the *O* and causing him to concentrate his attention at critical points, as they are approached. At the beginning of a series, *O* is set for and is expecting a change. He is in the habit of reporting 'change' at the start and is predisposed to expecting that the 'change' is a few steps away. Thus, 'habituation' works in the same direction as one form of 'expectation' and in the opposite direction to the other form. Whatever effect they have on one another is further reduced when the results of the paired series are combined. In the descending and ascending series they work in opposite directions. Series of small steps of different lengths is the essence of the method. Cornsweet, however, recommends that "the size of the steps be so selected that no more than two, three, or four like responses are made before a reversal of the response occurs."[8] He introduces a new error in the procedure without offering a means of avoiding it. *O*s will soon acquire the habit of responding after a given trial.

In addition to the constant and variable errors mentioned, the experiment is also subject to *accidental* errors which arise from the fluctuation and distraction of attention; from variations in *O*'s mood and state of health; from *E*'s errors in manipulation of the apparatus; and from other physical disturbances. These accidental errors are best counterbalanced by frequent repetition of the series. Fifty paired series averaging 16 experiments per pair (800 in all) are not an excessive amount for work in science. Apparently, a preliminary run of 8-10 trials and an experimental run of 15-20 trials—23 to 30 trials in all—meet Cornsweet's ideal.[9]

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[7] For the observational errors see: Titchener, *op. cit., Instructor's Manual,* 99-136; *Student's Manual,* 1905, 55-65; J. P. Guilford, *Psychometric Methods,* 1936, 116 ff.

[8] Cornsweet, *op. cit.,* 486.

[9] Cornsweet, *op. cit.,* 487.

The method of calculating the data obtained—the *RL, DL,* or *PSE*—has varied considerably over the years. At one time, the terminal values of the separate series were averaged; at another time, the first stimulus-values of two consecutive changes were averaged. At a still later period, interpolated values between the last two reports of a series were averaged; that is, the values between the last '+' and the first '−' of a descending series and the last '−' and first '+' of an ascending series. This was done because neither of these reports, at the particular moment given, was at the limen. It stood somewhere between them. The interpolated value was theoretically closer to the limen than either of the others. Interpolation did not change the value of the limen, but it did affect the *SD*. If the descending and ascending series interlaced, the *SD* was reduced; if they separated, the *SD* was increased.

"Doubtful judgments" reported during the course of an experiment have been variously received. Some experimenters accepted them as marking the limen;[10] others refused them, requiring their *O*s to make a 'forced choice'; that is, to report a 'doubtful' positively as '+' or '−'.[11] This 'forced choice' was continued by many investigators until S. S. George, in 1916, investigated the problem of the 'doubtful' judgments and showed that they differed in kind from 'positive' judgments of 'greater,' 'equal,' or 'less,' and that they should be excluded from the psychophysical computations.[12] 'Forced choice' solves nothing, and an *O* forced by *E* to alter a 'doubtful' judgment, to report 'greater,' 'equal,' or 'less,' will oblige, but he is powerless to alter the fact that it was originally 'doubtful.' A much better way of removing 'doubtful' judgments from consideration—which is the purpose of 'forced-choice' procedure—is either for the *E* to say nothing and immediately to repeat the 'doubtful' experiment or, if doubt occurs again at the subsequent trial, to interrupt the series, declare a rest-period and then, after a brief rest, begin that particular series over again. If doubtful judgments still persist, *E* should declare a holiday; he has pushed his *O* enough for that time and period.

The search for short-hand methods in technology is laudable enough, but it is entirely out of place in science, where new trails are being blazed and attempts are made to reduce and to eliminate all of the errors of observation. Ease and convenience are poor experimental guides.

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[10] For tables in which 'doubtful reports' (?) are received, see Titchener, *op. cit., Instructor's Manual,* 1905, pp. 6, 17.

[11] E. C. Heinemann, The relation of apparent brightness to the threshold for differences in luminance, *J. exp. Psychol.,* 61, 1961, 392 f.; Sharon Feeny, P. K. Kaiser, and J. P. Thomas, An analysis of data gathered by the staircase-method, this JOURNAL, 79, 1966, 652-654.

[12] S. S. George, Attitude in relation to the psychophysical judgment, this JOURNAL, 27, 1917, 1-37.

George Humphrey: 1889–1966

George Humphrey was born July 17, 1889, in England, in the County of Kent. In due course he went up to the University of Oxford where he was graduated, with high honors, in mathematics, the classical languages, and in philosophy. Then, with a scholarship to help, he studied psychology in Leipzig, where Wilhelm Wundt still "held undisturbed his ancient reign." Upon Humphrey, as upon many others, Wundt made a great and lasting impression, and did much to set the main stream of his life. Following this, George went to America, where he engaged in a number of varied teaching jobs. He acquired a Ph.D. at Harvard in 1920, and went immediately to the Wesleyan University, where for four years he worked with Raymond Dodge, for whose experimental eminence he retained the greatest respect and admiration to the end of his life.

In 1924, Humphrey became Charlton Professor of Psychology at Queen's University, Ontario. There he continued, with occasional visits to Europe, particularly to England and Cambridge, and to the U.S.A., until 1947. He developed at Queen's a highly successful psychological laboratory, and during the Second World War, he took an active part in directing much experimental and personnel research for the Canadian Army.

In 1947, St. John's College, Cambridge, established a "Dominion Fellowship," and Humphrey, already well known and liked at St. John's, became the first Dominion Fellow. He took up residence in the Long Vacation, but before the beginning of the Academical Year 1947-48 Oxford University decided to have an honors school in psychology, philosophy, and physiology, with a Professor to run it. Humphrey became that Professor, and, until his retirement in 1956, he lived and worked at Oxford, building a department of psychology based firmly upon an experimental foundation. He persuaded the University to establish the Oxford Institute of Experimental Psychology in 1948, and became its first Director. He did not always find the way of the psychologist at Oxford easy, but he worked with unremitting devotion, and subsequent developments have shown that he built both wisely and well.

After his retirement, he gave lectures in Germany for the British Embassy. Then he lived for a while at Hove in Sussex, but the pull of Cambridge, where he had always felt happy and "at home," was strong, and it was there, once more officially welcomed by St. John's, that he spent the last years of his life, full of activity and with many friends near at hand.

Besides writing many articles and reviews, Humphrey produced eight books on psychological topics and, as "Donald MacPherson," two novels which won him considerable success. His psychological books were in 1923, *The Story of Man's Mind;* in 1932 a translation (with Muriel Miller, his first wife) of Itard's *The Wild Boy of Aveyron;* in 1933 *The Nature of Learning;* in 1948 *Directed Thinking;* in 1951 *Thinking: An Introduction to its Experimental Psychology;* in 1963 (as co-editor) *Social Psychology through Experiment;* in 1963 (also as co-editor) *Psychology through Experiment;* and in 1963 (with R. V. Coxon) *The Chemistry of Thought.*

Of these volumes the major contributions are his study of learning and his description and analysis of experiments on thinking processes, which mainly concerned the work carried out by Külpe and his associates at Würzburg. Both these works are analytical in a constructive manner, displaying a wide and accurate knowledge of their subject matter, and a strong grasp of their underlying philosophy. Humphrey did not himself plan, and carry out any original major experimental research, but his knowledge of psychology in almost all aspects was great, his judgment firm, his writing clear and elegant, and the bent of his mind positive. These two books especially will always remain readable and ought to continue to be read.

Humphrey's two novels, published in the late 1930's under the assumed name of George MacPherson, were *Go Home Unicorn,* and *Men are Like Animals.* They appeared, as will be seen, when Freudian influence in psychology was tremendous, and both bear some witness to the effect of the period. In completing the whole picture of Humphrey and his achievements they must not be forgotten, for they illustrate his insight into human behavior, his lively but controlled imagination, and his power with words.

He was twice married: first to Muriel Miller in 1918, and then, in 1956, to Berta Hochberger, who worked with him during his post-war lectures in Germany. He was a man a bit above normal height, neat, active, very friendly; but also, on occasion, unyielding, and a good companion.

After a brief illness, George Humphrey died at Cambridge on April 24, 1966.

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BOOK REVIEWS

Edited by T. A. RYAN, Cornell University

Philosophical Problems of Psychology. By EDWARD H. MADDEN. New York, Odyssey Press, 1962. Pp. vii, 149.

The author of this modestly-sized and modestly-spoken volume offers a set of philosophical essays that should be of considerable interest to psychologists. He has examined specific critical assumptions on which some important psychological systems rest, and in discussing them he also takes issue with previous philosophical criticism of these concepts. Madden has no mind to examine each of these conceptual systems in its entirety, but limits himself to some of the premises and pivotal assumptions. It should be said at once, however, that he usually succeeded in apprehending the crucial or central issues of Gestalt and Learning Theories, of Verstehende Psychologie and Psychoanalysis, which are his topics. In all these instances his aim is to clarify "the logical structure of explanation in psychology and of separating scientific and philosophical discourse where they have been inadvisably joined."

Concerning Gestalt psychology, Madden shows on the hand of the Newtonian problem of η-bodies that Wertheimer's hypothesis "the whole is more than the sum of its parts" does not hold up unequivocally if interpreted as emphasizing configuration or pattern over elementals. In other words, what he describes can also be accomplished in analytical (*i.e.* elemental) terms. The investigator, says Madden, only "must continue to add constituents to the initial description until he can state a law enabling him to predict subsequent conditions." Thus he questions Wertheimer's famous example of the melody which sounds the same even though transposed to another key: description of elemental conditions and the unabashed use of "and," according to Madden, would, at least in principle, not be inadequate or "too elementristic" to accomplish the task.

To Madden's apt analysis one must, however, add two conditions. One is the fateful distinction as to where configuration enters into the picture. Do we perceive the world in configurational terms, or is physical reality itself constituted according to *Gestalt* principles? In other words, Gestalt psychology may be conceived along more subjective or more objective lines. Köhler at one point chose to extend Wertheimer's perhaps more subjectivist hypotheses onto the physical world at large, thereby creating a mythology of nature, in essence not too different from the grand constructions of the Renaissance philosophers and, later on, of Schelling and his school. Madden takes up this question again in the third chapter of his book. Against his rule of *completeness* as stated there one might, however, argue that *completeness* is only approximated but never reached unless the psychological event under study is, after all, formulated in configurational terms. Precisely this formulation brings in—even though by the expedient of a slightly irrational leap—that which can not be described by "and" operations. Madden makes his point by recourse to physical examples, that is, inanimate objects. He doesn't really examine an act of perception, let alone more complex behaviors such as Goldstein and Lewin chose to study. The rejoinder to Madden's logical analyses might be that the principle of configuration has to be understood in proportion to the complexity of a problem which, as in an infinite regression, escapes the final grasp of the investigator while it is being examined.

Madden devotes a whole chapter to the knotty problem of isomorphism. As in his previous analyses he stays close to the logic of the problem and does not allow himself to be drawn into specific psychological questions—which is, in fact, the level on which such concepts need to be examined—preferably before they have become part of the dogmatic underpinnings of a scientific enterprise or "school." Empirical investigation will show whether a set of concepts or basic hypotheses has pragmatic usefulness in organizing the data of observation or not. The logical structure of concepts, on the other hand, is a subject for which the philosopher is better qualified than the psychologist—especially if, as in Madden's case, he has knowledge of mathematics and physics. Madden's argument against isomorphism is too complex to bear repeating here. Let me only report that isomorphism does not come off too well in the end. Madden approaches the problem from a logical point of view. After reading him it occurred to me that, from an epistemological point of view, reasons could be added why isomorphism should be relegated to the less fruitful speculations of psychology. Madden also calls to task the phenomenological dualism on which Gestalt theory rests. He points quite correctly to the confusion between a scientific and an epistemic hypothesis. The former "has already accepted what constitutes the valid basis of knowledge irrespective of the causal genesis of that basis"—that is, the scientist's empirical labors can not possibly establish the durability of the axioms on which his observations are scheduled to rest. Madden refers then to the Gestalt psychologist argument about the irreducibility of the *behavioral environment* (that is, whether perceptual groupings can, or can not, possibly be the result of learning) and relates it to the primary argument of the Gestalt school against Wundtian *elementarism.*

In this connection, Madden also deals with 'understanding' based on *Einfühlung* and shows that on purely philosophical grounds empathy is neither a necessary nor a sufficient condition for understanding, which in the end always has to rely on a more objective kind of observation. The claim of "understandability" in this specific sense does not tell us any more about the subject under investigation than we would have known before, although it *does* tell us something about the investigator.

So far so good. Up to this point Madden is incontrovertible. If anybody were to argue seriously that social and environmental factors are useless in psychological explanation, he would surely tempt us not to take *him* seriously. The problem lies deeper, however: the talk about empathy does not explain how the individual should have succeeded thereby in making the jump from his own subjective inner experience to that of the other person's. What we call empathy is not a mysterious faculty or, at any rate, it does not help us when we leave it as that. It is in point of fact a rather inexact, grab-bag name for a series of complex conditions in the development of the individual self, such as identification, projection, and self-differentiation, upon which at least the more complex events of "understanding" between people rest. A detailed analysis of processes of "understanding" on that basis would show why people actually understand each other as well as they sometimes do; but also why they so persistently mistake each other's intentions.

Now to psychoanalytic propositions. When philosophers examined psychoanalytic propositions the usefulness of their criticism has frequently been reduced by certain elementary shortcomings. It is notably difficult to discuss the achievements or faults of psychoanalysis without some familiarity with its empirical operations. If a physicist would say the same thing about physics nobody would have the cheek to disagree with him. Oddly enough this is not so with psychoanalysis. Most of its critics have little idea of how it really works, but must have thought themselves fully equipped in spite of it—or, maybe, because of it—to castigate both its theory and practice. I wonder whether one can judge any science by merely reading about it; psychoanalysis, at any rate, is peculiarly unsuited for this approach. Moreover, by the very nature of its subject psychoanalysis is especially prone to cause the reader misgivings of a more personal sort even before his intellectual scruples have been roused—a fact to which Freud has devoted several thoughtful essays. Madden's virtue consists first of all in being keenly aware of all this, which can not be said of many of the philosophers who participated in a symposium on this subject to which he makes extensive reference. He points out that the lack of predictive capacity, a popular argument against the scientific status of psychoanalysis, does not deprive psychoanalytic propositions either of meaningfulness or the prospect of confirmation. To this I would add that predictability cannot be set up as a postulate *a priori* in any case. It would make little sense to speak of it in other than pragmatic terms, that is, in terms of what can be reasonably hoped to be predictable. Under these auspices it would not be warranted to make exacting demands of psychoanalytic propositions when no other theory of personality can do appreciably better. To those who would point to "experimental evidence" one would have to reply that it is apparently much less feasible to predict events in the immense, and largely obscure complexity of on-going life processes than variables in the limited confines of a controlled experiment. Furthermore, the idea of prediction has mostly been defined in terms of experimental situations. One of the several problems of prediction to which not enough attention has been given is that of ordinary or historical time. It would add a relevant perspective to the debate if, for example, an investigator would claim that within a year an individual will show a marked change of behavior—in his attitudes, his affects, in his response to others; but if he would stipulate at the same time that it is neither possible to state when exactly this change will occur nor what its practical scope will be. For change will, of course, not only depend on the individual in question but on the vicissitudes of his life continuously affected as they are by his health and his environment, including large-scale social and political events which in themselves are utterly unpredictable. That, precisely, is the condition of predictions in psychoanalytic psychotherapy.

"The upshot of this long discussion," Madden concludes, "is this: critics are not successful in showing that analytical hypotheses are simply *not confirmed*. . . . They are, in my mind, sufficiently confirmed to make psychoanalysis a perfectly respectable proto-science."

Madden examines further the concept of unconscious motivation and rejects it at least inasmuch as it presumes to refer to specific psychological entities, that is, when it postulates the existence of some specific motivational force affecting behavior and mood without being itself conscious. He believes that analysts have insisted on the ontological reality of such a concept because the suggestion of relegating it to the status of an intervening variable seemed to reduce it to a mere verbal transposition, a convenient redefinition which contributes nothing new. Madden proceeds then to show why an intervening variable (again, at least in principle) need not add any less to our understanding then a hypothetical construct. Madden may overrate the logical sophistication of many analysts when they refer to unconscious motivation; but he also underrates the number and persuasive force of incidents which make it so difficult to dispense with that concept in the somewhat improvised yet highly sophisticated laboratory of psychotherapeutic operations. People in therapy do really discover someday that they have been hating an important person in their life all the time; or that they have been silently prompted by unconscious guilt to do, or abstain from doing, a great many things. In the light of such a disclosure it is difficult to think of an explanation that stays clear of unconscious motivation. On the other hand, if we call it an intervening variable we really have not done more than give a promissory note on an explanation yet to come. I believe myself that the term *unconscious* is used too broadly in psychoanalysis. Sometimes ideas which are usually referred to as unconscious can be shown to have come to a dim and fleeting awareness, sensed and quickly glossed over by the perceiving self; but the relevant connection between them is established only when it is pointed out, or "interpreted," by the therapist. But then there still are events in the forgotten imagery affecting current symptoms which do not really fit this schema.

Finally, Madden gives an extensive discussion of the issue of determinism and responsibility. Madden concludes that psychoanalysis neither supports ethical determinism nor "establishes common-sense self-determinism and so salvages the notion of moral responsibility," though he seems to have a little more sympathy with the latter point of view. At any rate, "whether the issue the determinist raises is real or spurious, psychoanalysis has nothing to do with solving or dissolving it." Madden proceeds then to argue against the McNaghten rule and the present ambiguities of legal responsibility; but while his plea is both warm-hearted and humane it also tempts him to leave the secure position of the philosophical arbiter and to involve himself in practical suggestions far beyond his scope. The reader, while sympathetic to his point of view, still wishes that Madden had stuck to his logical guns which he handles so effectively. In fact, the end of the book and occasional touches of awkwardness in the presentation of subtle, if not rarified points of view comprise most of the criticism I have to offer. Disagreement does not belong in this score; for even where he disagrees the reader will appreciate the clarity and good sense with which Madden has set up the problem for him.

University of Michigan FREDERICK WYATT

ESP: A Scientific Evaluation. By C. E. M. HANSEL with an introduction by EDWIN G. BORING. New York, Charles Scribner's Sons, 1966. Pp. xxi, 263. $6.95.

What is wrong with parapsychology? Why, after all these years, has it made so little impact on scientific thinking and on scientific psychology? Of one thing at least we can be certain: Either the phenomena in question are extremely elusive and unstable or there are no such phenomena. Most of those who have pondered this dilemma have found it hard to reach any definite conclusion, one way or the other. Not so Professor Hansel, a British psychologist who has for long enjoyed the reputation of being among the boldest and most relentless of the skeptics. He is, it would seem, singularly free from doubts. Somehow he *knows* that *ESP* is impossible and his only problem is how best to explain away the evidence. This book is the fruit of his endeavors. Opinions will differ as to how far it succeeds, but there can be no denying its singleminded tenacity. Crowned with an introduction by Boring, it is likely to become the standard reference for all who seek to justify their rejection of the paranormal.

The case against *ESP* can be summed up by means of three propositions: (1) that, by and large, the more stringent the conditions of observation and control the less frequent and impressive the phenomena; (2) that experimental findings are seldom confirmed and predictions made from them seldom fulfilled; and (3) that the phenomena are such as readily lend themselves to trickery of various kinds and there has never been any lack of dishonest people to take advantage of this fact. Hansel is adept at using each of these three prongs as required, but it is in connection with the third that he really comes into his own. His audacious imputations of deliberate cheating as a possible counter-hypothesis to *ESP* are what give the book its special flavor and originality and are what will attract the major share of attention.

Since it would not have been possible to deal exhaustively with all the evidence that purports to demonstrate *ESP*, Hansel adopted the legitimate expedient of concentrating his critique on a few experiments which, by common consent, are regarded as evidentially the most compelling. Four investigations are selected as meeting this criterion and they are analyzed in some detail in Chapters 7, 8, and 9 which thus constitute the core of the book. In each case the author's solution of the mystery follows similar lines: one or more parties to the investigation cheated so as to have deceived the rest. In one case, it was the subject who was the culprit; namely Hubert Pearce (then a student of divinity, now a Methodist minister); in another, it was one of the experimenters, namely J. L. Woodruff (a respected member of the A.P.A.); while in the remaining two investigations, a whole conspiracy is postulated, its central figure being S. G. Soal (then a professor of mathematics at London University, now retired).

In evaluating Hansel's contribution, certain points must be kept clearly in mind. First, the author has been unable to unearth anything incriminating regarding any of these individuals; nor is he able to suggest any adequate motive that might have induced any of them to commit such an outrage. The only grounds for suspicion are the results themselves. Indeed it does not seem as if even the author regards his conjectures as having any psychological plausibility for, in the final chapter of the book, he falls back on the abstract Humean argument that, in the last resort, fraud is always and necessarily more credible than miracles. What Hansel has done, and with remarkable ingenuity, is to have demonstrated that, given sufficient skill and determination, a trick could have been worked. From this he rightly concludes that these experiments can no longer be regarded as foolproof. But the reader is unlikely to be satisfied with being told that cheating *could* have occurred, the interesting question is: *did* it occur?

Of special interest in this connection is Hansel's treatment of Soal. To fit his thesis, Soal has to be cast in a triple role: first as the cautious researcher of the prewar years ("by 1939 Soal had shown himself to be a careful and critical investigator"), then as the sinister figure who between 1941 and 1946 was engaged in laboriously manufacturing evidence for telepathy in collusion with his two star subjects and a number of the agents (has Hansel ever stopped to consider what appalling risks a man in Soal's position would have to face when making the initial overtures? After all, neither Shackleton nor Mrs. Stewart was known to him beforehand). Finally, we have the pathetic Soal of the post-war years, the innocent but gullible dupe of two Welsh schoolboys and their rapacious family. One might have supposed that, if Hansel were correct and the only 'telepathy' that Soal had ever witnessed prior to meeting the Jones boys was of his own contriving, he would be the last person to fall a prey to their machinations. And just how gullible was he? Certainly, he cannot be blamed for not realizing that the boys might have been communicating by means of an ultrasonic whistle that would be inaudible to their elders. Of the many persons whom Soal invited to watch the boys in action, some with long experience of this sort of thing, no one hit on this possibility, not even Mr. Salvin of the 'Magic Circle' who was reputedly the foremost authority in Britain on pseudo-telepathic codes. In fact, the credit for this suggestion goes entirely to Hansel himself. The tragedy of it was, however, that he was so tardy in coming forward with it that, by the time he and Christopher Scott went down to Wales to test the boys in March 1959, they had not been scoring for the past two years! Yet this in itself calls for comment. For if Hansel is right in suggesting that the boys had simply found an easy way of mulcting Soal of his money, why had they desisted so long before the game was up?

The remainder of the book is a general survey from the early days of the London S.P.R. to recent developments in America and Europe, ending with a quick glance across the Iron Cultain. Throughout, one detects a tendency to gloat over the more discreditable episodes and there is a notable lack of appreciation of the peculiar difficulties with which pioneers in this field have to contend, but it all helps to build up the case for the prosecution. The immediate effect of the book can only be to lower still further the prestige of parapsychology with the scientific community at large but, if parapsychologists prove capable of rising to Hansel's challenge, its long-term effect may be just the reverse. Hansel cannot be overthrown, however, by perpetuating the debate over bygone events; only the production of new and better evidence can do that. Lately, it has become something of a cliché among parapsychologists to say that too much time has already been spent on the vain attempt to satisfy the determined skeptic and that efforts now be directed toward elucidating the nature of *ESP*. Hansel's book has at least convinced me that this is dangerous complacency. I fully agree with him that a renewed attack on the evidential question using the full resources of modern automated techniques would be abundantly worthwhile, whatever the outcome.

University of Edinburgh JOHN BELOFF

Behavior in Uncertainty and Its Social Consequences. By JOHN COHEN. New York, Basic Books, 1964. Pp 207. $5.50.

Life is a gamble. You gamble when you walk across the street, buy a lottery ticket, attempt suicide, try to score in football, or drive a car along the highway. Professor Cohen draws on a diverse array of experimental results, statistical evidence, and common sense in his exploration of these and other behaviors that constitute gambles. He discusses decision-making in a variety of everyday tasks, and does so in an interesting, readable manner that can be enjoyed by the non-specialist.

The book begins with a series of abstract experiments which illustrate the important distinction between the pre-decisional process of assessing evidence and the actual selection among alternative courses of behavior, a distinction analogous to the contrast between thinking and deciding what to do. Unfortunately, the results of these abstract experiments are so complex that no simple principles emerge, perhaps because a typical paradigm for this class of experiment results in an investigation of subjective biases due to variables with little theoretical linkage. Take, for example, the following: Would you rather try to draw a winning ticket from a source containing 9 winning tickets in a population of 10, or from a source containing 90 winning tickets in a population of 100? Cohen finds that most subjects prefer to draw from the smaller population. This experiment, like so many others associated with decision-theory, requires the subject to choose between gambles so designed that an ideal decision-maker would be indifferent between them. The salient but theoretically irrelevant variables influence choice-behavior. It seems difficult, however, to integrate such effects into general principles, probably because the independent variables are not inherently linked to the theory under investigation.

The next section, a substantial portion of the book, is a series of chapters on different kinds of social implications of behavior in uncertainty. The topics include national lotteries, driving an automobile, suicide attempts, risk-taking in sports, and pathological decision-making. For each topic it is the context of the behavior that is paramount rather than the development of a psychological decision-theory to account for the behavior. For example, the chapter on traffic problems analyzes why traffic accidents occur, discusses the relation between alcohol and accidents, and contains many suggestions for increasing traffic safety. The chapter on suicide is primarily a statistical analysis of the kind of suicides attempted, the various reasons for suicide, the probabilities of success, and so on. Each chapter contains a collection of general interest items, but the topic under consideration is relatively insulated from the remainder of the book.

After discussing each topic individually, Cohen pauses to "knit together the many threads . . . into some more coherent pattern." This most illuminating chapter is an exploration of the historical, intellectual development of the elusive concept of randomness. The discussion begins with notions of randomness pitted against the philosophy of determinism and traces the resulting emergence of the theory of probability. It distinguishes between mathematical probability, psychological probability, and subjective probability, and then points out that the diversities of the behavior in the preceding chapters were all "undertaken with subjective uncertainty as to the outcome." Cohen follows the lead of other psychological decision-theorists in introducing subjective value (utility) as well as subjective probability to explain behavior in uncertainty. The idea is that there is both a subjective probability and a subjective value attached to each of the consequences of an act. These are substituted for mathematical probability and value in the framework of decision-theory. According to decision-theory, rational behavior requires that one choose the act that maximizes expected value: the analogous principle with the subjective dimensions is to maximize subjectively expected utility. It is this principle that Professor Cohen proposes as a provisional model for integrating the diverse topics of the preceding chapters.

The maximization of subjectively expected utility is proposed as an integrating model, but what specific principles of the diverse array of behaviors are accounted for by the model? For example, what characteristics of subjective probability and utility do you think lead persons to drink before driving, to buy a lottery ticket, or to attempt a goal from too great a distance? In what ways does a person attempting suicide maximize subjectively expected utility? Why does the obsessive compulsive, who is "the world's champion splitter of hairs," remain so long in the pre-decisional phase that he rarely decides to act? Professor Cohen has left the important task of integration up to the reader, a challenging task indeed.

In sum, this is an interesting book; its significant scientific contribution resides in pointing out a wealth of problems that may be interpretable within the proposed framework. Much integrative effort is necessary, however, before this contribution can have an important impact on experimental and theoretical aspects of psychological decision-theory.

University of Michigan CAMERON R. PETERSON

The Challenge of Response Sets. By JACK BLOCK. New York, Appleton-Century-Crofts, 1965. Pp. vi, 142. $5.00.

Paradoxically, the Minnesota Multiphasic Personality Inventory (*MMPI*) has been recently singled out for criticism on the dual grounds of prying into private areas of personality and failing to do so. While outraged citizens (*S*s) have publicized invasions of privacy, advocates of "response set" (*E*s) have attempted to demonstrate that the MMPI measures little more than test-taking habits which operate independently of personological content. Although the instrument could hardly be guilty on both counts, it steadily has been losing both public and scientific favor. Distinguished psychologists were quick to defend the MMPI on the privacy issue, but few have risen to the scientific challenge posed by the now popular response-set view. Professor Jack Block is a notable exception and his monograph is most deserving of the Appleton-Century-Crofts award it received for distinction in the field of psychology.

Block defends a substantive interpretation of what is measured by the MMPI against two major alternative views, acquiescence and social desirability. Acquiescence refers to tendencies on the part of *S*s to agree with personality statements regardless of their content. Scales which have a disproportionately high percentage of items scored "true" are said to be particularly vulnerable to this response-set. Social desirability refers to tendencies on the part of *S*s to attribute favorable statements to themselves and to deny unfavorable statements; again, with little concern for the content of the statements. When *S*s' endorsements of statements are highly correlated with the independently-determined desirability of the statements, social desirability is said to be operative. In recent years, advocates of acquiescence and advocates of social desirability have found so little challenge in the notion of "content" that they have become involved in intramural battles, each group arguing for the primacy of its favorite response-set. Declaring a plague on both their houses, Block devotes his entire monograph to a refutation of the relatively moderate (for this area) statement of Jackson and Messick that "acquiescence, as moderated by social desirability, plays a dominant role in personality inventories like the *MMPI*." Block's argument is, as he puts it, "involved and prolonged" and more than a casual acquaintance with psychometrics, factor analysis, personality theory, and general MMPI lore is required to see it through to the end. Despite the intricacies of argument, the exposition is beautifully clear, the data often compelling, and the tone remarkably temperate for an area which has suffered from *ad hominem* innuendos.

Block makes seven separate but closely related points in as many chapters: (a) The evidence for the existence of acquiescence response-set is controverted by the "admissibility of a content-basis for the results obtained." (b) "Content-relevant measures *can* be constructed which, beyond reasonable dispute, eliminate the possibility of an interpretation in terms of acquiescence." (c) "the first two factors of the *MMPI*—the factors held to be due to response sets—continue to exist and *in identical form* when the acquiescence component is completely removed from *MMPI* scales." (d) "certain intrinsic and in part accidental characteristics of the *MMPI* item pool have permitted the possibility of an acquiescence interpretation to develop." (e) The social desirability concept is "found to be of ambiguous import because of a confounding of social desirability with equally tenable, characterological dimensions of explanation." (f) The construction of a desirability-free measure of the first factor of the MMPI unconfounds this dimension and "demonstrates that the notion of social desirability is related only adventitiously to a personality dimension of fundamental behavioral significance." (g) The behavorial correlates of the first two factors of the MMPI are explored and "found to be numerous, of import, and to constellate in accord with previous content-based interpretations of these factors" (All quotations in (a)-(g) from Pp. 3-4).

Block has risen to the "challenge of response-sets and fought a versatile intellectual battle along two quite different lines of attack. First, he has met the proponents of response-set interpretation on their own ground—internal statistical analysis—and he has provided a brilliant display of a Black Belt Psychometrician in full battle. More important, he has marshalled extensive and impressive *external* evidence for his claims, based on independent and meaningful behavioral assessments. He concludes: "The validation of a scale is a process involving spiraling, reciprocal interplay between scale and criterion, theory and empiricism. On balance, I believe the *MMPI* has measured up well. The final triumph of the *MMPI* against the challenge of a response-set reinterpretation may be seen to derive from this homely and homing concern with empirical anchor points." (p. 129)

University of Illinois JERRY S. WIGGINS

Hypnotic Susceptibility. By ERNEST R. HILGARD, with a chapter by Josephine R. Hilgard, New York, Harcourt, Brace & World, 1965. Pp. xiii, 434. $9.50

This volume represents a major contribution to an understanding of the hypnotic phenomenon. It reports primarily on the work carried out by Dr. Hilgard and his group at Stanford in the years between 1957 and 1964. In many ways, it is analogous to Hull's classic, *Hypnosis and Suggestibility,* which was the outgrowth of work done primarily within Hull's own laboratory. Such an undertaking has the virtue that the author speaks about studies which he knows intimately. Because a systematic program of research has been followed, it is possible to present data which were obtained in order to answer specific questions, and it also allows him to see other work in the perspective of his own research. Such a book is very much in contrast to Weitzenhoffer's (1953) *Hypnotism: An Objective Study in Suggestibility,* which attempted to review the sum total of empirical work by others systematically and to reconcile divergent findings. Where a survey like Weitzenhoffer's is almost compelled to take any given study at face value, Hilgard is able to view other research critically in the light of his own findings.

In the introduction, the author makes it clear that he does not intend to cover the very large literature on hypnosis, particularly the older literature; further, that the data reported are essentially those derived from work by his own laboratory. Accordingly, he rarely presents data other than those obtained at Stanford, though the book is generous in crediting investigators elsewhere and frequently cites their conclusions. The reader seeking to find a comprehensive, balanced treatment of hypnosis, from a variety of viewpoints, will be disappointed. For the student or the reader unfamiliar with this field, it may be unfortunate that Professor Hilgard writes so lucidly. The maturity and sophistication of the book may tempt some readers to take it as a more general statement than the author intended. Certainly, he recognizes the many unresolved problems and does not attempt to put forth a unifying, central, theoretical position. Perhaps his strategy is the only appropriate one in a field as obscure as hypnosis.

The research approach of the Stanford group is largely based on their systematic work in developing a variety of scales of hypnotic susceptibility. With these instruments it has been possible to ask, and partially answer, a number of questions about hypnosis in general, as well as about some specific phenomena such as amnesia. Studies involving the direct, experimental manipulation of hypnotic phenomena are rarely reported, a straightforward, correlational approach being generally preferred. This choice of method was partly dictated by the problem which has been the focus of the group's interest: What are the psychological correlates of hypnotizability? The ingenuity, perseverance, and clinical, as well as statistical, sophistication brought to bear on this problem are extremely impressive. Nevertheless, there has been a relative dearth of positive findings. It remains a remarkable paradox that, despite the considerable success with which objective scales of hynotizability have been developed and the remarkable stability of the phenomenon, solid personality correlates have failed to emerge. A salient contribution of the book is its documentation of this arduous search, despite the disappointing results to date. This should serve as a warning to the naïve investigator who, on the basis of one study, feels that he has solved the problem of psychological correlates of hypnotizability. In a highly thought-provoking chapter, Josephine Hilgard proposes reasons for the failure to find such correlates. Although it will be exceedingly difficult to test the hypotheses she presents in a rigorous manner, they do provide plausible reasons why the search thus far has been disappointing.

This book is definitely recommended to any serious investigator or student as an eminently readable, sophisticated statement of the present knowledge about hypnosis, seen in the light of one laboratory's research. The incidental remarks about a philosophy of research, the difficulties encountered in approaching a complex problem such as hypnosis, and the need for psychology to face up to such problems are also of particular value and add to its significance.

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MARTIN T. ORNE

Shift Work; The Social, Psychological, and Physical Consequences. By PAUL E. MOTT, FLOYD C. MANN, QUIN MCLOUGHLIN, and DONALD P. WARWICH. Ann Arbor, The University of Michigan Press, 1965. Pp. 351. $8.50.

This book is recommended reading for all psychologists as well as the broad group of social scientists. In addition, business men, interested in the social consequences of shift work as it effects their production and staffing, would do well to be familiar with this volume. A major difficulty is that the appropriate title "Shift Work" is likely to limit the appeal, circulation and reading by the many who could benefit from this book.

The primary reason for being favorably impressed is that this volume is an encompassing study on the effects that shift work has, not only on the worker, but his family, friends, and the community. In this respect, it can be considered the first meaningful and worthwhile research on the topic.

The study is a good example of research, planning, and design. The authors have taken great pains to construct a plausible theoretical structure which becomes a basis for their inquiry. They have meticulously considered independent as well as dependent variables throughout their thorough investigation of the social, psychological, and physical consequences of shift work.

Surprisingly enough, they include a brief index of marital happiness that might well have wide and further applications. The authors ought to be congratulated for the effectiveness of integrating clinical psychology and industrial psychology in their study of the consequences of shift work.

The book consists of eight chapters including: 1. "Background," 2. "Theory and Method," 3. "Work Schedules and Family Roles," 4. "Shift Work, Marital Happiness, and Family Integration," 5. "Shift Work, Social Life, and Solitary Activities," 6. "The Psychological Reactions to Shift Work," 7. "Work Schedules and Physical Health" and 8. "Summary and Conclusions."

City College, C.U.N.Y. MILTON L. BLUM

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(The books listed here have not as yet been noted in our pages. Listing here does not, however, preclude their later review).

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